THE

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WORKS

OF THE HONOURABLE

ROBERT BOYLE.

VOLUME III.

Ex rerum Causis Supremam noscere Causam.

LONDON:
Printed for A. Millar, oppoſite *Catharine-Street*, in the *Strand.*
MDCCXLIV.

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BRITAN
NICVM

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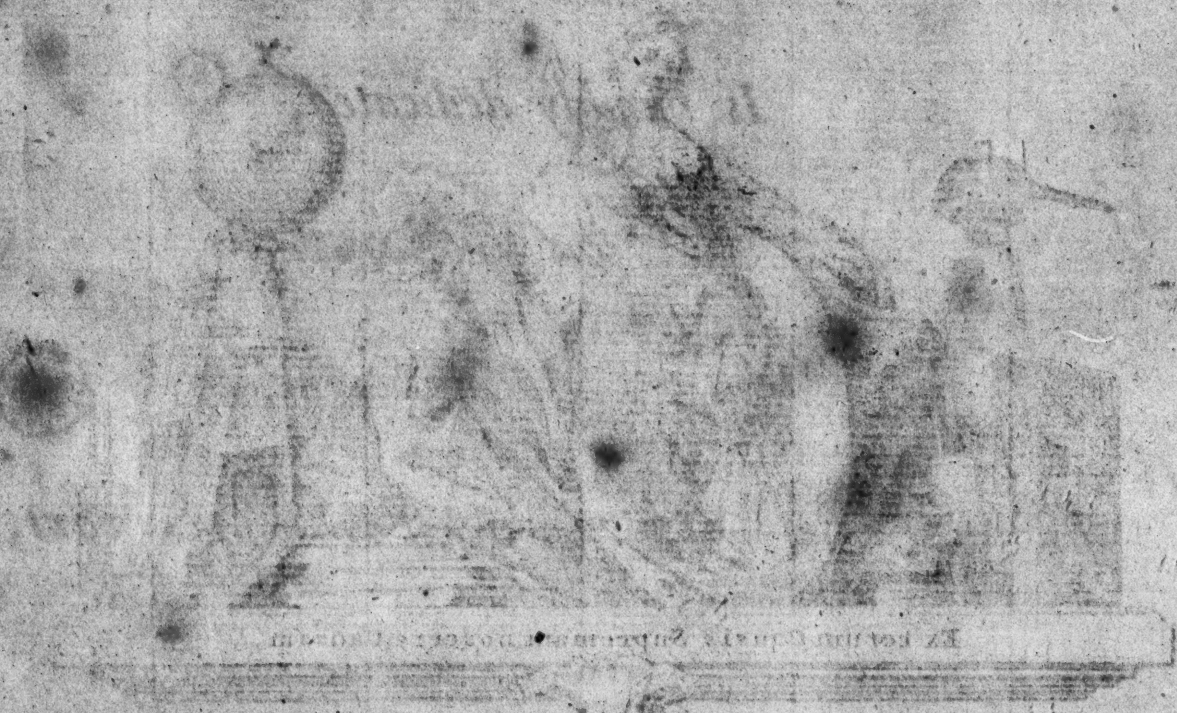

Ex rerum Causis Supremam noscere Causam

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TO

THE RIGHT HONOURABLE

JOHN Lord CARTERET,

One of HIS MAJESTY's Principal Secretaries of State,

THIS

THIRD VOLUME

OF THE

WORKS

OF

The Honourable *ROBERT BOYLE*

Is humbly dedicated,

By his LORDSHIP'*s*

Moſt devoted and

Moſt obedient Servant,

Andrew Millar.

A
CONTINUATION
OF
NEW EXPERIMENTS
PHYSICO-MECHANICAL,
TOUCHING THE
SPRING and WEIGHT of the AIR, and their EFFECTS.

THE FIRST PART.

Written by way of LETTER, to the Right Honourable the
Lord CLIFFORD AND DUNGARVAN.

WHERETO IS ANNEXED
A Short Diſcourſe of the ATMOSPHERES of CONSISTENT BODIES.

THE PREFACE.

HAVING at the beginning of the treatiſe, whereof this is a continuation, acquainted my readers with ſeveral things, that belong, in common, as well to the following experiments, as to thoſe there publiſhed; it will not be neceſſary for me to trouble the reader with a repetition of what he may have met with there already, nor to acquaint him in this addreſs with any other particulars, than thoſe that concern the experiments I am now about to preſent him.

I doubt not but it will be remembered by ſome, that I ſeemed in the above-mentioned book to have promiſed a ſecond part of it, or a large appendix to it: but intimations of that kind do many times reſpect only the thing itſelf, leaving the giver of them free in point of time: and I wanted not ſufficient inducements to delay a while to perform my promiſe, if I made any. I had, indeed, partly before the book already referred to came from the preſs, and partly ſometime after, made divers other trials, in order to a ſupplement of it: but being obliged to make ſome journeys and removes, which allowed me no opportunity to proſecute the experiments, I had made no very great progreſs in my deſign, before the convening of an illuſtrious aſſembly of virtuoſi, which has ſince made itſelf ſufficiently known under the title of THE ROYAL SOCIETY. And having then thought fit to make a preſent, to perſons ſo like to employ it well, of the great engine, I had till then made uſe of in the phyſico-mechanical experiments about the air; and being unable afterwards to procure another ſo good, I applied my ſtudies to other ſubjects, and gave over, for a great while, the care of making more experiments of that kind: and the rather, becauſe that finding by the very favourable reception thoſe I had publiſhed had met with among the curious in ſeveral parts of *Europe*, that they were like to be conſidered and peruſed, I thought I might ſafely leave the proſecution of them to others, who would probably come more freſh and untired to ſuch an exerciſe of their curioſity.

BUT, obſerving that the great difficulties men met with in making an engine, that would exhauſt and keep out a body ſo ſubtle as air, and ſo ponderous as the atmoſphere, (beſides, perhaps, ſome other impediments) were ſuch, that in five or ſix year I could hear but of one or two engines, that were brought to be fit to work, and of but one or two new experiments, that had been added by the ingenious owners of them; I began to liſten to the perſuaſions of thoſe that ſuggeſted, that unleſs I reſumed this work myſelf, there would ſcarce be much done in it. And therefore having (by the help of other workmen than thoſe I had unſucceſsfully employed before) procured a new engine, leſs than the other, and differing in ſome circumſtances from it, we did (though not without trouble enough) bring it to work as well as the other, and, as to ſome purpoſes, better. And having once got this, I made haſte to try with it thoſe experiments, that belong'd to the deſigned continuation, and do now make up this book.

I hope, that to ſuch readers as the following papers are principally intended for, I ſhall not need to make an apology, either for the plainneſs of my ſtyle, (wherein I aimed at perſpicuity, not eloquence,) or for my not having adorned or ſtuffed this treatiſe with authorities, or ſentences of claſſic authors, which I had neither the leiſure to ſeek, nor thought I had any great need to employ, though it had been far more eaſy, than perhaps it would have proved, to borrow from them things, that would have been very proper to a treatiſe, where my main deſign was, to make out, by practical experiments, divers things, among others, that have not hitherto been advantaged by that way of probation, nor perchance thought very capable of it; ſo that I ſhall have obtained a great part of what I aimed at, if I have ſhewn, that thoſe very phænomena, which the ſchool-philoſophers, and their party, urge, and ſometimes triumph in, as clear proofs of nature's abhorrency of a vacuum, may be not only explicated, but actually exhibited, ſome by the gravity, and ſome alſo by the bare ſpring of the air. Which latter I now mention as a diſtinct thing from the other; not that I think it is actually ſeparated in theſe trials, (ſince the weight of the upper parts of the air does, if I may ſo ſpeak, bend the ſprings of the lower) but becauſe, that having in the already publiſhed experiments, and even in ſome of theſe, manifeſted the efficacy of the air's gravitation on bodies, I thought fit to make it my taſk in many of theſe, to ſhew, that moſt of the ſame things, that are done by the preſſure of all the ſuperincumbent atmoſphere acting as a weight, may be likewiſe performed by the preſſure of a ſmall portion of air, included indeed (but without any new compreſſion) acting as a ſpring.

THE preſent firſt part of our continuation might, I confeſs, have been, not inconveniently, divided into two parts. For firſt, it contains ſome experiments, that are already related in the printed books, though they be here ſo repeated, as to be confirmed, illuſtrated, or improved, by being reiterated either with better inſtruments, or with better ſucceſs, than when they were made in my large receiver, which holding (if I miſremember not) about eight gallons, could not eaſily be ſo well exhauſted as thoſe ſmall receivers I often ſince employed. And ſecondly, the other, and far more numerous ſort of experiments, related in this firſt part, are new, and ſuperadded. And yet I forbear to aſſign each of theſe two ſorts a place by itſelf, becauſe I could not conveniently ſet down my trials otherwiſe than as they came to hand among my notes; and I conſidered, that in divers places the new ones and the old ones being mentioned together, might ſerve by their neighbourhood to illuſtrate or confirm each other. And however, at another edition of our Continuation, it will be a very eaſy taſk, if it appear to be a requiſite one, to give the improvements of the former experiments, and the ſuperadded new ones, diſtinct titles and places.

As for the mechanical contrivances I employed in making the following experiments, though moſt of them have had the good fortune to meet with an approbation, and ſome of them with more than that, from no mean virtuoſi and mathematicians; yet as I expect, that critical readers will judge, that in ſome experiments more artificial inſtruments might have been made uſe of, ſo I hope that they will not look upon thoſe I was reduced to employ, as always the beſt that ever I could have directed, ſince it ſufficiently appears by divers paſſages of the following experiments, that they were not made at *London*, but in places where the want of a glaſs-houſe, and other accommodations reduced me to make my trials not after the beſt manner I could deviſe, but in the beſt way I could then and there put in practice. And let me add on this occaſion to what I have elſewhere ſaid to the like purpoſe, that it is both a great diſcouragement to many ingenious men, and no ſmall hindrance to the advancement of natural philoſophy, that ſome nice criticks are ſo cenſorious in exacting from attempters the very beſt contrivances, and many, that would be attempters, ſtand too much in awe of ſuch mens judgments; for though in very nice experiments the exactneſs of inſtruments is not only deſireable and uſeful, but, in ſome caſes, neceſſary; yet in many others, where the production of a new phænomenon is the thing aimed at, they are to be looked upon as benefactors to the hiſtory of nature, that perform the ſubſtantial part of a diſcovery, though they do it not by the moſt eaſy and compendious ways deviſeable, or attain not to the utmoſt preciſeneſs, that might be wiſhed, and is poſſible. For ſuch performances, notwithſtanding their being ſhort of perfection, make diſcoveries to the world of new and uſeful things; which though others, that are more lucky at contrivances, and have better accommodations, may compaſs by more compendious ways, or with greater preciſeneſs, yet ſtill the world is beholden to the firſt diſcovery for the improvement of it, as we are to *Archimedes* for the firſt deviſing a way to find, by weighing bodies in water, how much gold or how much ſilver a mixture of thoſe metals does contain, though (if hiſtorians have not injured that great man in the relation) he went a more laborious and leſs accurate way to work than modern hydroſtatians, who (as I elſewhere ſhew) may perform the ſame thing by a far better way, which yet, probably, we ſhould not have thought of, if that attributed to *Archimedes* had not preceded, and afforded us a fundamental notion. And that the not being

ing ſo dexterous at contriving the ways to effect a thing, is no ſure argument, that a man has not a true and ſolid knowledge of it, we may eaſily learn from *Euclid*, whom our geometricians generally and juſtly acknowledge to be their maſter, and to have enriched the world with many uſeful truths, and ſolidly demonſtrated all his propoſitions, though divers of his modern commentators have found out more compendious ways for effecting ſeveral of his problems, as well as of demonſtrating divers of his theorems, eſpecially ſince the excellent invention of ſpecious algebra, by whoſe help that accurate mathematician Dr. *Wallis* has, beſides other ſpecimens upon intricate propoſitions, clearly demonſtrated the ten firſt and for the moſt part perplexing theorems of the ſecond element, in little more than as few lines. In ſum, in experiments that are very nice, accurate contrivances and inſtruments are induſtriouſly to be ſought, and highly to be valued; and even in ſuch other experiments, as are frequently to be reiterated, the moſt commodious and eaſy ways of performing them are very deſirable: but thoſe practical compendiums, though very welcome to them that would repeat trials, are not ſo important to the generality of readers, as being but uſeful to ſave pains, not neceſſary to diſcover truths; to which men may oftentimes do good ſervice, without any peculiar gift at mechanical contrivances, ſince in moſt caſes they may be looked upon as promoters of natural philoſophy, who deviſe experiments fit to diſcover a new truth, if the attempt ſucceeds, and propoſe ways of bringing it to trial, which though perhaps not the moſt ſkilful or expeditious, are yet ſufficient and practicable, the increaſe of phyſical knowledge being the product of the things themſelves that are diſcovered, whatever were the inſtruments men employed about making the diſcoveries.

As for the cuts, I endeavoured to make their relations, and deſcriptions of moſt of the experiments, ſo full and plain, as to need a few ſchemes as might be to illuſtrate them: but though I hope, that they, who either were verſed in ſuch kind of ſtudies, or have any peculiar facility of imagining, would well enough conceive my meaning only by words; yet leſt my own accuſtomance to deviſe ſuch trials, and to ſee theſe made, ſhould make me think them more eaſily intelligible than moſt readers will find them, I adviſed with a learned friend or two, fit to be conſulted on ſuch an occaſion, what experiments were requiſite to be illuſtrated with diagrams, and to ſuch I took care they ſhould be annexed. Only I forbore to add to the figure of each inſtrument alphabetical explications of its parts, as judging that troubleſome work leſs eaſy for me, than it would be for ſuch readers as this tract is deſigned for, to underſtand what is delivered by the help of a little attention in conferring the ſchemes of the inſtruments with the verbal accounts of the experiments they relate to. But there is one particular about the cuts may require both to be given notice of and excuſed; which is, that having occaſion to alter the method of my experiments, when I began to foreſee, that I ſhould be obliged to reſerve divers things for another opportunity; and being myſelf abſent from the engraver for a good part of the time he was at work, ſome of the cuts were miſplaced, and not graven in the plates, in which, according to the preſent ſeries of experiments, they might moſt properly have been put.

BUT perhaps I may (for I am not ſure of it) more need the reader's pardon for (unknowingly) troubling him in this continuation with ſome paſſages, that he may have already met with in the book it refers to: which though I had not read over for ſome years before, I chanced not to have at hand, when divers of the following papers were written; and though afterwards I recovered it, yet the indiſpoſition of my eyes made me think it unfit rather to tire them by reading over the whole book, than to truſt to the reader's good nature (in caſe I ſhould need it) for the pardon of a few unintended repetitions.

I doubt not, many readers will be inquiſitive to know, why this treatiſe is ſtiled the firſt part of a Continuation. To give theſe ſome account of the title, I muſt put them in mind, that in the already publiſhed experiments I intimated, that two ſorts of trials might be made by the help of our engine: the one, ſuch as needed but a ſhort abſence of the air, and the other ſuch as required, that the air ſhould not only be withdrawn for a while, but kept out for a conſiderable time, from the bodies whereupon the trial is made. Of the former ſort of experiments are theſe this preſent book does (as well as that heretofore publiſhed did) conſiſt of. And though I have been ſo much called upon, and troubled for certain writings, whereof I have made ſuch mention in thoſe that paſſed the preſs, as ſome readers interpreted to be an engagement, that it made me think fit, when I ſatisfied their demands, to be thenceforward very ſhy of making the publick any promiſe; yet I was induced not to alter the title of this treatiſe, partly becauſe it may intimate to the curious, that there are yet a great many things to be performed by our engine, beſides the productions of it I have hitherto preſented them; and partly becauſe, though I ſtill perſiſt in my former averſeneſs to make promiſes to the world; yet it is very poſſible, that if God grant me life and health, I may, in due time, preſent my friends with what may ſerve for a ſecond part of our Continuation, conſiſting of experiments, that require a longer abſence of the air from the bodies to be wrought upon: and I ſhall think, if this firſt part prove not unexceptable to the curious, that the latter will be not unwelcome to them, as being deſigned to conſiſt of ſets of experiments, which by their being moſt of them new, and ſome of them odd enough, may perchance afford ſome not deſpicable hints to the ſpeculative. But the very nature of theſe experiments requiring, that ſome of them ſhould be long in making, my friends could not reaſonably expect a quick diſpatch of work of this kind, though I ſhould not meet for the future with ſuch intervening impediments, as have hitherto diſturbed it, (as want of inſtruments, of health, of leiſure, and of the liberty, which is ſo requiſite in this caſe, of ſtaying long enough in one place :) notwithſtanding all which difficulties I have by ſnatches been able through God's bleſſing to make forty or fifty of deſigned trials, being ſuch as require the leaſt of time to be performed in, though I now think not fit to mention any of them, as well for other reaſons, as becauſe though they be made by the help of our engine, yet they require a peculiar apparatus of inſtruments, very differing from thoſe we have hitherto mentioned, and not to be intelligibly deſcribed without many words and divers figures. In the mean time, leſt the induſtrious ſhould be diſcouraged by a ſurmiſe, that there is nothing left for them to do by the help of our engine, at leaſt as to the firſt ſort of experiments, I ſhall inform them, that I had thoughts to have added divers others of that kind to theſe that now come forth, and particularly two cluſters of pneumatical trials, the one about reſpiration, and the other about fire and flames; but ſeveral of my notes and obſervations being at preſent out of the way, my having neither health nor leiſure to repair theſe inconveniencies, and proſecute trials of that ſort with any aſſiduity, makes me chuſe rather to reſerve them for an appendix, than to make thoſe that now come abroad ſtay for them. Which will not, I preſume, be the more diſliked, becauſe by taking this courſe I may, in delivering of the phænomena of nature, imitate nature herſelf, of whom it is the Roman philoſopher's ſaying, *rerum natura ſacra ſua non ſimul tradit*. Seneca quæſt nat. lib. 7. c. 31.

Some ADVERTISEMENTS touching the ENGINE itſelf.

THOUGH the engine already publiſhed, and that which I employed in the following trials, have the ſame uſes, and agree both in the ground and the main part of their conſtruction, yet they differ in ſome particulars fit to be taken notice of: for after I had preſented the great engine I formerly made uſe of to the Royal Society, partly the difficulty of procuring ſuch another of that ſize and make, and partly the deſire of making ſome improvements invited me to make ſome alterations in the ſtructure; ſome of them ſuggeſted by others, (eſpecially by the ingenious Mr. *Hook*,) and ſome of them that I added myſelf, as finding that without them I could not do my work. Wherefore it will not be amiſs to point at the chief differences between the former and the latter engine, and to intimate ſome of the conveniences and inconveniences that attend them.

As for the conſtruction of the ſecond engine itſelf, ſince it is preſumed, that the readers of this book have already peruſed that, of which this is a Continuation, and underſtood the contrivance of the inſtrument, that belongs to it, it was preſumed ſufficient to exhibit in the firſt plate the delineation of the entire engine ready to be ſet at work; and in the ſecond, the figures of the ſeveral metalline parts, that compoſe it, before they are ſet together. For though theſe have not verbal and alphabetical explications annexed to them, yet the ſight of them may ſuffice to make thoſe, that have an imagination fitted to conceive mechanical contrivances, and are acquainted with the former engine, comprehend the ſtructure of this; which, alphabetical explications would ſcarce make ſuch readers do, as are not ſo qualified: only two things there are, which being of ſome difficulty, as well as of importance to be conceived, I ſhall here particularly take notice of. The firſt of which is, that in regard the ſucker is to be always under water, and the perforation *p q*, that paſſes perpendicularly quite through it, and ſerves together with the ſtick *r s* for a valve, is to be ſtopt at the bottom of the cylinder, as at *n o*, when it is full of water, it was requiſite to make the ſtick *r p* of a conſiderable length, as two or three foot. The other and chief thing is, that in the ſecond plate, the pipe *A B*, whoſe end *B* bends upward, is made to lie in a groove or gutter purpoſely made in the flat wooden board *c d e f*, on which the receivers are to reſt; which ſquare board I cauſed to overlaid with very good cement, on which I took care to apply a ſtrong plate of iron, of the bigneſs and ſhape of the board, leaving only a ſmall hole for the erected part of the pipe to come out at, which I added, not only to keep the wooden board the better from warping, but becauſe I knew (what will perhaps be thought ſtrange) that the preſſure of the atmoſphere on one ſide of the board, when there is no preſſure or but very little on the other, will enable many aerial particles to ſtrain through the very wood, though of a good thickneſs, and imbued with oil to choak the pores. To this iron-plate we ſometimes fit a lip turning up about it, to hinder the water, that on ſome occaſions will come from the receiver, from falling on the room; and (to add that upon the by) though the ſtop-cock *g h i k*, that belongs to the hitherto mentioned pipe, may be inſerted at *I*, into the barrel or cylinder *l m n o*, by the help of ſoder, yet we choſe as a much better way to have the branch *I*, of the ſtop-cock, made like a ſcrew, which being once firmly ſcrewed into the barrel, is not apt to be broken off, and may be more eaſily mended, if any thing happen to be out of order, which the engine is the moſt liable to be in or about the pipe; partly becauſe it may fall out, (though but very rarely, if due care be but taken) that the air will inſinuate itſelf between the wooden board and the iron-plate, and ſo get up (where the pipe bends upwards) into the cavity of the receiver; and partly becauſe the pipe being for a juſt reaſon made but ſlender, and the part of it that looks upward very ſhort, it happens not very unfrequently, that when we imploy receivers with narrow orifices, where the cement muſt lie cloſe to the opening of the pipe, it happens, I ſay, that the cement, eſpecially if it be much ſoftned by heat, is ſucked (as they ſpeak) into the pipe, and ſo choaks it up; or elſe that ſome part of the body included in the receiver is drawn to the orifice of the pipe, and lying upon it as a cover hinders the free paſſage of the air into the barrel; againſt which inconvenience, to add that upon the by, we uſe amongſt other expedients to place juſt about the orifice of the pipe a ſmall cover of tin, like that of a little box, which covers it at the top to hinder any thing from lying immediately upon the pipe, and has a ſmall opening or two in the ſide, to give the air of the receiver free acceſs to the pipe.

The ſquare and hollow wooden part of this engine, diſcernable in the firſt plate, is ſo made, that it may contain not only the cylinder, but ſo much water, as will always keep the cylinder quite covered with that liquor; by which means the ſucker, lying and playing always under water, is kept ſtill turgid and plump, and the water being ready at hand to fill up any little interval or chink, that may happen to be between the ſucker and the inſide of the barrel, does, together with the newly mentioned plumpneſs of the ſucker, very much conduce to the exact keeping out of the air. But this advantage is not without ſome inconvenience; for divers times, if great care be not taken in turning the ſtop-cock, the water will be impell'd into the receiver, and much prejudice ſundry experiments, when the included bodies are ſuch that may be ſpoiled or impaired (at leaſt for the preſent) by that liquor. The ſmalneſs of our cylinder is a convenience in regard of the facility it affords to make and diſpatch thoſe many experiments, that may be performed in ſmall receivers, though it make thoſe more troubleſome and tedious, that require the exhauſtion of large and capacious ones.

See Plate III. Fig. 1.

The flat plate (mentioned a little above) has this great conveniency in many experiments, that the receiver needs no ſtop-cock of its own; for ſuch a veſſel being made all of an entire piece of glaſs, and whelmed on upon the plate well covered with cement, can better keep out the air, than if there were a ſtop-cock, at which the air does but too frequently get in: but beſides that in divers experiments ſuch receivers do uſually require to be wide mouthed, whereby a greater compaſs is to be fenced againſt the ingreſs of the air, ſeveral experiments cannot ſo conveniently be tryed in this ſort of receivers.

But becauſe, that though this ſecond form of our engine hath as to ſeveral purpoſes its peculiar conveniencies and advantages, yet ſome virtuoſi may be furniſhed with the other already, and ſome may conceive it the more clearly of the two, or may judge it preferable for their particular deſigns; I ſhall here intimate, that for moſt of the experiments, if not all, that follow in this treatiſe, they may make uſe of, or at leaſt make a ſhift with the firſt engine, with a few alterations; whereof the chief is to be this, that to the upper part of the great cylinder, on the ſide oppoſite to the iron-rack, there is to be faſtned ſuch a ſquare board, and ſuitable iron-plate, as is uſed in the ſecond engine, betwixt which board and plate is to be lodged ſuch a pipe as was lately deſcribed, being either a continuation of the outward branch of the ſtop-cock, or elſe firmly faſtned to it by ſodering or ſcrewing: for by this means, when the ſucker is depreſſed, the air will through the cavity of this pipe, and the ſtop-cock whereto it is annexed, paſs freely, by virtue of its own ſpring, out of the receiver into the exhauſted cylinder; though this, and the ſucker that moves in it, being not kept, as in the ſecond form of the engine, under water, the greater care will be needed to keep the air from inſinuating itſelf between them. A good cement, to faſten the receivers to the often mentioned plate of iron, is a thing of no ſmall moment in making the following experiments, of which we imploy differing compoſitions for differing purpoſes, ſome of which are not neceſſary to be mentioned in that part of this work, that now comes forth; but that, which in almoſt all the following trials we chiefly make uſe of, is a well wrought mixture of yellow bees wax and turpentine, which compoſition, as it ſerves better than moſt others to keep out the air, ſo it has the conveniency, which is no ſmall one, of ſeldom needing to be heated, and ſeldomer to be much ſo; eſpecially if we imply a little more turpentine in winter than in ſummer, in the former of which ſeaſons, as much, or very near as much of that ingredient as of the wax does well, for as in ſummer a mixture of three parts of wax to about two of turpentine is more proper.

A CONTINUATION OF
NEW EXPERIMENTS
PHYSICO-MECHANICAL,
TOUCHING THE
SPRING and WEIGHT of the AIR, and their EFFECTS.

MY DEAR LORD,

SINCE I have already in proper places of the phyſico-mechanical experiments about the air, which I formerly preſented your lordſhip, given you a ſufficient account of ſeveral things touching the ſcope, occaſion, &c. of my attempt; it will not be neceſſary to make a ſolemn preface to the enſuing experiments. And therefore preſuming upon an acceptance, which the favourable entertainment, which your lordſhip, as well as the publick, was pleaſed to give my firſt trials of this kind, encourages me to expect, I ſhall, without troubling you with any further preface, immediately fall upon a continuation; eſpecially ſince your lordſhip will perhaps wonder, that you have not received it much ſooner, as, indeed, you ſhould have done, if I had been befriended with accommodations and leiſure.

EXPERIMENT I.

About the raiſing of mercury to a great height in an open tube, by the ſpring of a little included air.

DIVERS ways have been propoſed to ſhew both the preſſure of the air, as the atmoſphere is a heavy body, and that the air, eſpecially when compreſs'd by outward force, has a ſpring, that enables it to ſuſtain or reſiſt a preſſure equal to that of as much of the atmoſphere, as can come to bear againſt it; and alſo to ſhew, that ſuch air as we live in, and is not condenſed by any human or adventitious force, has not only a reſiſting ſpring, but an active ſpring (if I may ſo ſpeak) in ſome meaſure, as when it diſtends a flaccid or breaks a full-blown bladder in our exhauſted receiver.

BUT obſerving, that there ſeems to want a viſible experiment to convince thoſe, that are not ſo eaſily ſatisfied with reaſons, though drawn by juſt conſequence from phyſical or mechanical truths, or even from other experiments; taking notice, I ſay, hereof, I made the following experiments; not ſo much to prevent or remove a ſcruple no better grounded, as to have a new way of making an eſtimate by ſome known and determinate meaſure of the force of the bare ſpring of the air, both in its natural ſtate, (as it is ſaid to be, when not compreſſed nor rarified, more than the free air we breath,) and according to its ſeveral degrees of expanſion.

WE took then a vial, with a neck not very large; and having filled about a fourth part of it with quick-ſilver, we ſo erected and faſtened a long and ſlender pipe of glaſs, open at both ends in the neck of the vial, with hard ſealing-wax, that the lower end reached almoſt to the bottom of the quick-ſilver, and the upper more than a yard above the vial. Then having blown in a little air, to try, whether the inſtrument did not leak, (which it is very difficult to keep ſuch inſtruments from doing,) we conveyed it into a long and ſlender receiver, fit for ſuch an uſe; and having withdrawn the air as well as we could, we found, according to our expectation, that the ſpring of the air, included in the viol, impelled up the quick-ſilver into the erected pipe, to the height of 27 inches; and having ſuffered the external air to return into the receiver, the quick-ſilver ſubſided in the tube, ſometimes almoſt, and ſometimes quite as low as the ſtagnant quick-ſilver in the vial.

See Plate III. Fig. 1.

FOR the better illuſtration of this experiment, thus ſummarily related, but with the like ſucceſs, as to the main, ſeveral times repeated, we will ſubjoin the following obſervations and notes.

I. THAT we tried this experiment ſeveral times, and the laſt time in the preſence of the famous Savilian geometer, Dr. *Wallis*, who ſaw the quick-ſilver in the pipe impelled up to 27 inches, being one himſelf of the meaſurers. And though at other times we found it to be much about the ſame height with the laſt, yet once it ſeemed plainly to be a pretty deal higher; which yet we ſpecified not, becauſe a miſchance took off the mark, which we had made to meaſure the height by.

II. HAVING once, to try the ſtanchneſs of the vial, blown in ſo much air, (without taking out any thing as we uſe to do in the like caſe) that the air in the cavity of the viol raiſed and kept the quick-ſilver 3 inches high in the pipe, when we went on with the reſt of the experiment, according to the way above deſcribed, we found, by emptying the receiver of air, that we were able to raiſe the quick-ſilver in the cane 30 inches, or ſomewhat more above that in the vial.

III. SOMETIMES it may happen, that the mercury, when taken very ſoon out of the receiver, will not appear to have ſubſided to its firſt lowneſs, which perhaps it will not ſink to in ſome while after: which is not to be wondered at, ſince in ſuch a receiver, which contains but little air, the heat of the cement and the iron, imployed to melt it quite round the receiver, may impart a little warmth to the air in the vial, which will after return to its former temper. But this accident is neither conſtant nor neceſſary to the experiment.

IV. IT is very remarkable, that if the receiver be fitly ſtopped, and ſlender enough; upon the turning of the ſtop-cock, to let out the air at the firſt exſuction, the mercury will be impelled up by the ſpring of the air in the viol, ſuddenly flying abroad or ſtretching itſelf, ſo that it will be raiſed ſeveral inches above the height it will reſt at afterwards, and will make ſeveral vibrations up and down before it come to ſettle, juſt as the mercury does in the Torricellian experiment, (the bare preſſure of the little air doing here to the mercury what the weight of the atmoſphere does there,) and ſuch motions of the mercury will be made four or five ſubſequent exſuctions, upon the withdrawing of the air in the receiver. But as theſe grow leſſer and leſſer, as the ſpring of the included air grows fainter, ſo none of them is any thing near ſo conſiderable as the vibrations made upon upon the firſt ſuck.

V. AGREEABLE hereunto we obſerve, that at the firſt exſuction, when the ſpring of the included air was yet ſtrong, the mercury would be raiſed by our eſtimate above half, if not ⅔ of the whole height, whereto it will at length be brought, (though that muſt be according to the bigneſs of the receiver, and other circumſtances,) and the ſubſequent exſuctions do ſtill add leſs and leſs proportions of height to the mercurial cylinder, and that for two reaſons: the one, becauſe the more there is of mercury impelled into the tube, the greater weight of mercury preſſes upon the included air: and the other, becauſe the air has ſo much the more room in the vial to expand itſelf, whereby its ſpring muſt be proportionably weakened.

LASTLY, when we made moſt of theſe trials, I had the curioſity to obſerve the height of the mercury in a good barometer; and thereby found, that the air was then but light; its greateſt height reaching but to 29 inches, and ½, and its height ſoon after the trial, whereof Dr. *Wallis* was a witneſs, amounting but to 29 inches.

TO make an eſtimate of the quantity of air, that had raiſed the quick-ſilver to 27 inches, we took the vial, that was imployed about this experiment; and having counterpoiſed it, whilſt it was empty, we afterwards filled it with water, and found the liquor to weigh 5 ounces, 2 drams, and about 20 grains; and then having poured out the water, till it was ſunk to a mark, which we had made on the outſide of the glaſs, to take notice how high the quick-ſilver reached, that we poured in: and laſtly, weighing the remaining water, equal in bulk to the quick-ſilver, we found it to amount to 1 ounce, 2 drams, 14 grains; ſo that the air, that had raiſed up the mercury, poſſeſſed (before its expanſion) in the vial the place but of 4 ounces, and a few odd grains, i. e. of about ¼ of a pint of water. And as for the pipe alſo, imployed about the ſame experiment, we found its cavity to have about ⅛ part of an inch in diameter.

IT was one of the uſes I hoped to make of this experiment, that by comparing the ſeveral degees of expanſion of air included in the vial with the reſpective and increaſing heights of the mercury, that was impelled up into the pipe, ſome eſtimate might be made of the force of the ſpring of the air weakened by ſeveral degrees of dilatation; but for want of conveniencies I forbore to venture upon ſuch nice obſervations, eſpecially becauſe the preſſure of the dilated air, that remains in the receiver, and is external to the air included in the vial, muſt alſo be taken into conſideration.

ANOTHER uſe of our experiment may be this; that it may ſupply us with a conſiderable argument againſt ſome learned men, who attribute the ſuſpenſion of the quick-ſilver in the Torricellian experiment to a certain rarified matter, which ſome call a funiculus, and whereto others give other names; which rarified ſubſtance they ſuppoſe to draw up and ſuſtain the quick-ſilver, in compliance of nature's abhorrency of a vacuum. For in the experiment under conſideration, the quick-ſilver being not only ſuſtained at the height of 27 inches in the tube, but elevated thither; if the cauſe of this be demanded, it will be anſwered, according to their hypotheſis, that the air in the receiver, external to that of the vial, being, by reaſon of the ſucking out of ſome of it by the pump, more rarified than that in the vial, it draws up to it the quick-ſilver in the cane, and the more it is rarified, the higher it is enabled to draw it. But then I demand, whence it comes to paſs, that though we can, by perſevering to pump, more and more rarify the little remaining air, or the aereal ſubſtance in the receiver, that in the vial not appearing to be alſo rarified, yet the air in the receiver does not by virtue of its ſuperadded rarefaction, whereby it exceeds that of the air in the vial, pull up the quick-ſilver to a greater height in the tube than 27 inches: for, that this is not the greateſt height, to which mercury may be raiſed by this rarified ſubſtance, our adverſaries muſt not deny, who tell us, that in the Torricellian experiment it ſuſtains a mercurial cylinder of 29 inches, and ½, and can raiſe a cylinder of 29 inches to 29 ½, or higher, in caſe that the cylinder be made to vibrate up and down in the tube.

AND as for thoſe, that will in ſuch caſes, as our experiment ſuggeſts, have recourſe only to that which they call the *fuga vacui*, they may pleaſe alſo to conſider, that ſince the quick-ſilver remains the ſame, its aſcenſion in the tube will not be available for what they think to be nature's purpoſe; for, whether it reach higher or lower in the tube, it will adæquately

See the latter part of the following Experiment.

quately fill no more ſpace in one poſture, or in one figure, than in another, in what part ſoever of the cavity of the receiver it be placed.

EXPERIMENT II.

Shewing, that much included air raiſed mercury in an open Tube, no higher than the weight of the atmoſphere may in a baroſcope.

IN the former experiment, by reaſon of the ſmalneſs of the vial, that was employed about it, there was ſo little air included, that the expanſion of it, ſo far as was requiſite to impell up the mercury in the pipe to the above mentioned height of 27 inches, may be probably ſuſpected to have very much weakened its ſpring, and therefore it may be thought, that (eſpecially conſidering the great force, that ſeveral of our experiments manifeſt impriſoned air to have,) if there were a greater quantity of air included in the veſſel, ſo that the expanſion, ſufficient to raiſe the mercury to the former height, would not need to be conſiderable, (becauſe that the capacity of the tube being but the ſame, the whole included air will be ſo much the leſs expanded, by how much the more of it there is,) it ſeemed probable, that the ſpring of the air, being but a little weakened by ſo ſmall a dilatation, would remain ſtrong enough to raiſe a much taller cylinder of mercury in the tube, and perhaps make the liquor run over into the receiver.

But though this ſuggeſtion ſeem probable enough, yet when I conſidered, that the weight of the atmoſphere is able to ſuſtain a cylinder of quick-ſilver but of 30 inches, or thereabouts (in perpendicular height) and conſequently, that the preſſure of ſuch a mercurial cylinder is equivalent to that of an atmoſpherical cylinder of the ſame bore; it was not difficult to conclude, that ſince the air in a vial, before the mouth is cloſed, has a ſpring but equal in ſtrength to the weight of the atmoſpherical pillar that leans upon it, (for if the ſpring were too ſtrong for the weight that leans on it, ſome of the air would get out of the vial) a greater vial, and conſequently a greater quantity of included air would not be able by its ſpring to elevate and ſuſtain a longer cylinder of mercury, than the weight of the atmoſphere is able to do; nor indeed altogether ſo much, becauſe of ſome little (though but little) diminution of the ſpring by ſome (though but a ſmall) expanſion, that the included air ſuffers, by ſucceeding in the place of mercury, that is impelled up.

To clear therefore this matter by an experiment, we took a ſtrong glaſs-bottle, capable of holding about a quart of liquor; and having put into it a convenient quantity of quick-ſilver, we erected in it a very long and ſlender pipe of glaſs, open at both the ends, and reaching at the lower end beneath the ſurface of the ſtagnant mercury; and having faſtened this pipe in the neck of the bottle, by choaking up that neck very accurately with good cement, that none of the included air might be able to get out, we conveyed the whole into a receiver, like that imployed about the firſt experiment in ſhape, but much larger, that it might be able to contain ſo great a veſſel; and then the engine being ſet at work, we quickly raiſed the quick-ſilver to a greater height than formerly; and when we ſaw it come to a ſtand, we did by the help of ſome marks, made before hand on the pipe, and by the help of a very long and well divided ruler, meaſure, with as much care and accurateneſs as the figure of the veſſels would allow us to do, the height of the mercurial cylinder, which we found to be 29 inches, and about ⅞, to which abating half an inch, which was raiſed, before the pump was employed, by ſome air, that had been blowed into the bottle, to try whether it were ſtanch; deducting, I ſay, this half inch of quick-ſilver, which remained in the tube after the external air was let in, (as well as it had been there before the receiver was exhauſted,) out of the newly mentioned number there remained 29 inches, and near ⅜, for the height of the mercury, raiſed by the ſpring of the air, ſhut up in the bottle; and then conſulting with the above-mentioned baroſcope, which ſtood in a window in another part of the houſe, I found, that the weight of the atmoſphere did bear a mercurial cylinder of about 29 inches and ½, which was higher by ⅛ than that to which the ſpring had raiſed the quick-ſilver in the exhauſted receiver: and the difference perhaps would have been greater, if the place, where the experiment was made, had not by its warmth added ſome little matter to the ſpring of the air; and if alſo we could have kept the mercury ſo long elevated, as to give it leave to diſcharge its ſelf of thoſe ſmall bubbles, which it is almoſt impoſſible in ſuch experiments as this to free quick-ſilver from, without ſome help from time.

See the latter part of the following Experiment.

Lastly, though we cauſed the pump to be plied, to try whether we could not, by the more diligent exſuction of the receiver, raiſe the quick-ſilver above the height of that, which the atmoſphere kept ſuſtained in the baroſcope; yet our labour gave us but a confirmation, that the ſpring of the air would not raiſe the mercury higher, than did the weight of the atmoſphere, which may not a little confirm the ſecond obſervation.

N. B. This was not the only nor the firſt experiment we made of this kind; but this being carried on without miſchances, (with which divers others were attended,) and made with much care, I thought fit to ſet down this inſtead of all, intimating generally about the reſt, that they ſeemed to agree well for the main with that, which is here recited. Only there is one thing relating to thoſe other experiments, that ſeems not altogether unworthy to be taken notice of; which is, that when our trials were made in veſſels, that contained a conſiderable quantity of air, though upon the exhauſtion of the receiver, the ſpring of the included air could not raiſe the quick-ſilver to the top of the pipe, yet ſometimes by other effects it manifeſted itſelf to be very ſtrong, as once or twice by the blowing out or breaking the cork or cement, and other matter that was imployed to ſtop the glaſs it was ſhut in; and once by an accident too memorable to be here paſſed over in ſilence.

I had one day invited Dr. *Wallis* to ſee ſuch an experiment as I have been relating, made with (not a vial, but) a bottle of green glaſs, (ſuch as we uſe now for wine,) and four or five pounds of mercury. After this learned perſon and I had continued ſpectators as long as we thought fit, we withdrew into another room, where we had not ſat long by the fire, before we were ſurprized by a ſudden noiſe, which the perſon, that occaſioned it, preſently came running in to give us an account of, by which it appeared, that this ingenious young man, (whom I often imploy about pneumatical experiments, and whom I mentioned to your Lordſhip, becauſe *J. M.* has the honour to be ſomewhat known to you,) being deſirous in our abſence to ſatisfy the curioſity he had to know, whether the quick-ſilver could not be raiſed higher in the pipe than I had foretold, plyed the pump ſo obſtinately, that at length the bottle being not, it ſeems, every where equally ſtrong, the impriſoned air found it more difficult to make the quickſilver run over at the top of the pipe, than to break the bottle in the weakeſt place; and accordingly did not only throw off a piece of the bottle, but threw it with ſuch violence againſt the large and ſtrong receiver, as broke that alſo, and rendered it unſerviceable for the future. But the doctor and I laying together the pipe, which happened to be broken into but few pieees, concluded by the place, to which we were told it reached when this accident happened, that it had not exceeded, nor indeed fully equalled the height, to which the weight of the atmoſphere might have raiſed it.

EXPERIMENT III.

Shewing that the ſpring of the included air will raiſe mercury to almoſt equal heights in very unequal tubes.

HAVING ſhown in the two former experiments, that the active ſtrength of the air's ſpring is very conſiderable, I thought good alſo to examine, whether or no to the other reſemblances in operation between the weight of the free air, and the preſſure of the included air, this alſo may be added, that as the gravitation of the atmoſphere is able (as we ſhall hereafter prove) to ſuſtain the mercury at the ſame height in leſſer and greater tubes, ſealed at the top; ſo the preſſure of the included air may be able to ſuſtain the mercury at the ſame height in ſlenderer and in larger tubes, though in the latter it muſt ſuſtain a far greater weight of mercury than in the former; provided allowance be made for the weakening, which the ſpring of the included air muſt be ſubject to, by reaſon that, to ſucceed in the place of a large cylinder of mercury impelled up into the greater tube, it muſt expand it ſelf more, and conſequently have its ſpring more weakened, than if the tube were ſlender.

TO proſecute this experiment, I thought on a peculiar ſhape of veſſels, which, if I had been where there is a glaſs-houſe, I would have cauſed to be blown for the more convenient trying of two pipes of different bores at the ſame time. But though I wanted this accommodation, I thought I might well enough ſhew what I intended by imploying ſucceſſively two tubes of very differing ſizes, provided the veſſel for the including of the air were the ſame.

WHEREFORE taking the glaſs bottle, made uſe of to try the former experiment, and erecting in it after the manner above deſcribed a cylindrical pipe of glaſs, a good deal larger than the former, (if not as large again) we proſecuted the experiment as we had made it, with the ſlender tube above mentioned, and found, that we were able, by the ſpring of the air in the bottle, to raiſe the quick-ſilver to a conſiderable height, which, meaſuring as well as the veſſel would allow us, was, by the leaſt eſtimate that was made of it, (which was mine) 28 inches, and ⅛, by which it appeared to want ſomewhat above an inch of the height of the mercurial cylinder, which the weight of the atmoſphere could have ſuſtained, as appear'd by the barometer, wherein the quickſilver at that time was about 29 inches, and ¼ high; which difference was no more than I expected, conſidering, that, whereas the weight of the atmoſphere is ſtill the ſame, when the mercury is at its full height (and that whether the pipe be great or ſmall) in a ſealed tube; the ſpring of our included air muſt needs be weakened the larger the tube is, and the higher the liquid metal is impelled in it; ſo that it ſeemed a conſiderable phænomenon, that the ſpring of ſo little air ſhould be able to raiſe the mercury as high within an inch or thereabouts in a wider as in a ſlenderer tube, ſince the diameter of the cavity of the former being by our eſtimate double to that of the latter, (into which the ſlender pipe could eaſily be put as into a caſe too big for it:) the greater mercurial cylinder may be ſuppoſed to have weighed near four times as much as the leſſer; I ſay, near, becauſe there was an inch difference in their heights: but in caſe theſe had been equal, then the ſolidities of the cylinders would have been to one another as their baſes; and ſince theſe, being circular, are in duplicate proportion to their diameters, that is, as the ſquares of their diameters; it is plain, that if the diameters be as one to two, the ſquares of them muſt be as one to four; and theſe cylinders conſiſting of the ſame mercury, their weights will have the ſame proportions with their ſolidities, and conſequently would be as one to four, making the abatement formerly intimated for the inch and a little more of mercury, by which the larger cylinder came ſhort of the height of the former.

N.B. 1. This and the two former experiments tried by us with quick-ſilver may be alſo tried with water; but beſides that we could hardly procure tubes long enough for ſuch trials, we were not very ſollicitous about it: for if we attentively enough conſider, what has been already delivered, and the proportion in ſpecifick gravity betwixt water and quick-ſilver, (whereof the latter is near 14 times as heavy, bulk for bulk, as the former) it will not be difficult to foreſee the event of ſuch experiments, which he, that has a mind to make, ſhould be furniſhed not only with long tubes, but with capacious veſſels to ſhut up the air in; elſe the air will be ſo far expanded before the water has attained near the height, to which the weight of the atmoſphere may raiſe it, that the experiments will not ſeem to ſucceed near ſo well with water, as ours did with quick-ſilver.

2. WE thought it worth trying, whether, when the included air had raiſed the great cylinder of mercury to the utmoſt height, it could elevate it to, by the ſpring it then had, it would not be brought to raiſe the quick-ſilver yet higher, if, notwithſtanding the expanſion it had already, there were an agitation made by the heated corpuſcles of the ſame air. And in purſuance of this curioſity having cauſed an hot iron and a ſhovel of kindled coals to be held near the oppoſite parts of the receiver, we perceived after a while, that the mercury aſcended ⅛ of an inch or better above the greateſt height it had reached before. But conjecturing, that it would have riſen higher, were it not, that whilſt the application of the hot bodies was making, ſome particles of air had unperceivably ſtolen into the receiver, I cauſed the pump to be plied again to withdraw the air, I ſuſpected to have got in, by which means the mercury was quickly raiſed ⅛ of an inch, or better, by virtue of this adventitious ſpring, (if I may ſo call it) which the included air acquired by heat; and I made no doubt, that it might have been raiſed much higher; but I was unwilling by applying a leſs moderate heat to hazard the breaking of my glaſſes, in the place I then was in, where ſuch a miſchance could ſcarce have been repaired.

EXPERIMENT IV.

About a new hydraulo-pneumatical fountain, made by the ſpring of uncompreſſed air.

I SHALL now add ſuch an application of the principle, whereon the former experiment was grounded, as I ſhould ſcarce think worth mentioning in this place, were it not, that beſides that divers virtuoſi ſeem not a little delighted with it, it may for ought I know prove to be of ſome philoſophical uſe (to be pointed at hereafter.)

WE took a glaſs-bottle with a convenient quantity of water in it, and fitted this bottle with a ſlender glaſs-pipe open at both ends, and about three foot long, which was ſo placed, that the lower orifice was a good way beneath the ſurface of the water, and the pipe it ſelf paſſed perpendicularly upwards through the neck of the bottle, which neck was, by the pipe and by good hard cement imploy'd to fill the ſpace betwixt the pipe and the inſide, ſo well and firmly cloſed, that no water or air could get out of the bottle, nor no external air could get into it, but by paſſing through the pipe. This inſtrument was conveyed into a large receiver ſhaped like a pear, of which a good part of the blunt end, and a ſmall part of the ſharp end are cut off by ſections parallel to the horizon, and conſequently to one another. And becauſe this receiver was not (nor ought to be) long enough to receive the whole pipe, there was cemented on to the upper part of it a ſmaller receiver of white glaſs, of ſuch a length and bigneſs, that the upper end of the pipe might reach to the middle of its cavity, or thereabouts, and that the motions of the ſpringing water might have a convenient ſcope, and ſo be the better taken notice of.

See Plate IV. Fig 2. This figure was deſigned only to make ſome repreſentation of the difference, that would appear, if inſtead of making the fourth experiment with water, as in the foregoing figure, the trial was made with quickſilver.

THIS double receiver being cemented on to the engine, a little of the air was by one ſuck of the pump drawn out from it, by which the preſſure of the remaining air being weakened, it was neceſſary, that ſince the air included in the bottle had not its ſpring likewiſe weakened, it ſhould expand it ſelf, and conſequently impel up the water in the ſame bottle through the pipe, which it did ſo vigorouſly, as to make it ſtrike briſkly at firſt againſt that part of the top of the ſmaller receiver, which was juſt over the orifice of the pipe. But after it had a while made the water thus ſhoot up in a perpendicular line, as the ſpring of the air in the bottle grew by that air's dilatation to be weakened, the water would be impelled up leſs ſtrongly and leſs directly, till the air in the bottle being as much expanded as that in the receiver, the aſcent of the water would quite ceaſe, unleſs by pumping a little more air out of the receiver we renewed it again.

ABOUT the making of this experiment theſe particulars may be noted.

I. It is convenient, that the upper part of the pipe be made (as it eaſily may be at the flame of a lamp) very ſlender, that the water having but a very ſmall orifice to iſſue out at, may be ſpent but ſlowly, and thereby make the experiment laſt ſo much the longer.

II. YOU may, if you pleaſe, inſtead of making the upper part of the pipe ſlender, as was juſt now directed, cement on to it a top either of glaſs or braſs, conſiſting of three or more very ſlender pipes, with a pin-hole at the end of each, that one of theſe pointing directly upwards, and the others to the right hand and to the left, the water may ſpin out ſeveral ways at once, by which kind of branched pipes we have ſometimes imitated the *Jets d'eau* (as the French call them) and artificial fountains of gardens and grotto's.

III. IN regard that ſo ſhort a cylinder of water, as exceeded not the length of our glaſs pipe, could not make any conſiderable reſiſtance to the expanſion of the included air, it was thought and found ſafe enough to imploy inſtead of a ſtrong glaſs-bottle a much larger vial, without being ſollicitous about its ſhape, or that it ſhould be very ſtrong, and by this means we could make this pleaſant ſpectacle laſt a great while, eſpecially if we alſo made uſe of the expedient to be mentioned in the following note.

4. IF you find, that the included air has by expanding it ſelf too much weakened its

ſpring, whilſt there yet remains with it a good quantity of water in the bottle or vial, you may reinforce the preſſure of the air by only turning the ſtop-cock, and letting in what air you think fit to the exhauſted receiver: for upon the admiſſion of this new air, the air in the receiver will preſs upon the water in the pipe, and having driven it into the bottle again, will follow it thither, till the air in the bottle and that in the receiver have attained an equal ſpring, and then by pumping out a convenient quantity of the air contained in the latter, the air ſhut up in the former will be able to impel up the water as before, till the ſtagnant liquor be depreſſed to the lower orifice of the pipe, at which, when the air of the bottle can get out, the courſe of the water upwards muſt ceaſe.

THE Uſes I made of this new hydraulo-pneumatical fountain (for in it I aim not only at a ludicrous experiment) were principally theſe.

THE firſt was to make it the more probable, that if we had had convenient veſſels, we might by the preſſure of the air included in the bottle have raiſed water about fourteen times as high as we did quick-ſilver in the former experiment, ſince upon but a little weakening of the preſſure of the air in the double receiver, the air in the bottle was able to impel the water forcibly enough, and for a pretty while, to the top of a pipe of about a yard long, and a good deal higher. (But this is but a ſlight uſe.)

THE next thing therefore we deſign'd to ſhew by this experiment was, that in thoſe hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be ſet a moving as well by the meer dilatation of one of the parcels of the air, as by giving a new force by heat or compreſſion to the other, and whether this mechanical principle of motion may hereafter prove not altogether uſeleſs in engines, we refer to further conſideration.

ANOTHER uſe we made of this experiment was to ſhew ſomewhat relating to the ſpring of the air, which may be worth conſidering, though we ſhall now but barely mention it. If then, when ſome of the air had been pumped out of the receiver, we removed that double veſſel from the bottle, the external air would by its weight haſtily depreſs the water in the pipe, till having driven it to the very bottom, it got up in numerous bubbles through the water, and joined it ſelf with the air incumbent on that liquor: but that which was here obſervable was, that all the external air that was able to get into the bottle, did not do it ſuddenly, but after the firſt irruption we could perceive, that from time to time there would new portions of air leiſurely inſinuate themſelves through the pipe into the bottle, and emerge through the ſtagnant water in bubbles, that ſucceeded one another ſo ſlowly, as to beget ſome wonder, as if the ſpring of the included air having been once put out of its wonted conſtitution by its late expanſion, could not be reduced to it but by degrees by the weight of the atmoſphere, which was ſtill the ſame: or, rather, as if between the ſpring of the included and the preſſure of the external air counterballancing each other, there happen'd ſome ſuch thing as is obſerv'd in an ordinary pair of ſcales, of which one is too much depreſſed, where the motion (which was ſwift enough at firſt) becomes ſo much the ſlower, by how much the weights come nearer to the æquilibrium, which their equality diſpoſes them to reſt in.

BUT the chief uſe deſigned in this experiment was, to obſerve, whether the lines, made by the water in its effluxions, would be of the ſame figure, notwithſtanding the rarifaction of the air in the upper part of the receiver, as if the air had not been at all rarified: and for this purpoſe it is beſt to make one's obſervations towards the latter end of the experiment, becauſe then the receiver being moſt exhauſted, and conſequently having the leaſt of air left in it, the difference made by the change of the denſity of the *medium*, in which the beams of water (if I may ſo call them) move, is like (in caſe there be any) to be beſt diſcerned. And this convenience we had by our way of experimenting, that we could take notice of the lines deſcribed by the ſalient water, as the ejaculation of that liquor grew ſtill fainter and fainter. But though I afterwards invited Dr. *Wallis* to favour me with his opinion about the curve lines of the ſalient water, yet for want of an upper receiver large enough, even he profeſſed himſelf (as I had done) not ſatiſfied about them. Only he ſometimes (as I alſo did) obſerved the ſalient water to deſcribe part of a line perfectly enough parabolical, with which ſort of curves he has been particularly converſant.

THIS made me reſolve for further ſatiſfaction to attempt by another contrivance, (of whoſe ſucceſs, if I can procure the implements I need, your lordſhip may expect an account) what the figures will be not only of ſalient water, but mercury, and other liquors; and that when the receiver is much better exhauſted, than it was neceſſary it ſhould be in the foregoing experiment.

EXPERIMENT V.

About a way of ſpeedily breaking flat glaſſes, by the weight of the atmoſphere.

FOR the more eaſy underſtanding of ſome of the ſubſequent trials, it will be requiſite in this place to mention, among experiments about the ſpring of the air, the following phænomenon belonging to its weight.

THIS is one of thoſe, that is the moſt uſually ſhewn to ſtrangers, as a plain and eaſy proof, both that the weight of the incumbent air is conſiderable, and that the round figure of a receiver doth much more conduce to make an exhauſted glaſs ſupport that weight, than if the upper part of the receiver were flat.

TO make this experiment we provided a hoop or ring of braſs of a conſiderable thickneſs, whoſe height was 2 ½, or 3 inches, and the diameter of whoſe cavity as well at the upper as lower orifice (ſhould have been juſt

3 inches, but through the error of the workman) was 3 inches and $\frac{1}{10}$. To this hoop we ſucceſſively faſtened with cement divers round pieces of glaſs, ſuch as is uſed by glaſiers (to whoſe ſhops we ſent for it) to make panes for windows, and thereby made the braſs-ring with its glaſs-cover a kind of receiver, whoſe open orifice we carefully cemented on to the engine; and then we found, as we had conjectured, that uſually at the firſt exſuction (though ſometimes not till the ſecond) the glaſs-plate would be broken inwards with ſuch violence, as to be ſhattered into a great multitude of ſmall fragments, and (which was remarkable) the irruption of the external air driving the glaſs inwards did conſtantly make a loud clap, almoſt like the report of a piſtol. Which phænomenon, whether it may help us to diſcover the cauſe of that great noiſe, that is made upon the diſcharging of guns, (for the recoil ſeems to depend upon the dilatation and impulſe of the powder) I muſt not ſtay to conſider.

EXPERIMENT VI.

Shewing, that the breaking of glaſs-plates in the foregoing experiment, need not to be aſcribed to the fuga vacui.

THOUGH I long ſince informed you, that in the experiments I then preſented your lordſhip, it was not my purpoſe to deliver my own opinion, whether there be a vacuum, or no; and though I do not in this tract intend to declare my ſelf either way; yet, that I may on this occaſion alſo ſhew, that the preſſure of the air may ſuffice to account for divers phœnomena, which according to the vulgar philoſophers muſt be referred to nature's abhorrency of a vacuum, I will illuſtrate the foregoing experiment by another, the ſubſtance whereof is this.

THAT if, inſtead of the above mentioned braſs hoop, both whoſe orifices are of equal breadth, you imploy a hollow (but taller) piece of braſs, or (which is more eaſily made) of latten, ſhaped like a conus truncatus, or a ſugar-loaf, whoſe upper part is taken off parallel to the bottom; and if you make the two orifices of a breadth ſufficiently unequal, as if the larger being made as wide as that of our braſs-hoop, the ſtraiter were leſs than an inch in diameter; you will find, that if this piece of metal be made uſe of, as the other was in the foregoing experiment, the flat glaſs cemented on to the orifice, will be eaſily broken, as formerly when it is faſtened to the wider orifice; but if the ſtraiter orifice be turned upward, the glaſs that covers it, if it be of a due thickneſs (though no thicker than the former) will remain entire, notwithſtanding the withdrawing of the air from beneath it: which ſeems ſufficiently to argue, that it is not preciſely nature's abhorrency of a vacuum, that is the cauſe, why glaſſes are uſually broken in ſuch experiments, ſince whether the wider or the narrower orifice be uppermoſt, and covered, (the metalline part of the veſſel being the ſame, and only varying its poſture) the capacity of the exhauſted veſſel will be equal; and therefore nature ought to break the glaſs as well in one caſe as the other, which yet the experiment ſhews ſhe does not.

WHEREFORE this diverſity ſeems much better explicable by ſaying, that when the wider orifice is uppermoſt, the glaſs that covers it muſt ſerve for the baſis of a large atmoſpherical pillar, which by its great weight may eaſily force the reſiſtance of the glaſs: whereas when the ſmaller orifice is uppermoſt, there leans upon its cover but ſo ſlender a pillar of the atmoſphere, that the natural tenacity or mutual cohæſion of parts in the glaſs is not to be ſurmounted by a weight, that is no greater.

EXPERIMENT VII.

About a convenient way of breaking blown bladders by the ſpring of the air included in them.

THE foregoing experiments having ſufficiently manifeſted the ſtrength of the airs ſpring upon fluid bodies, I next thought fit to try, whether the force of a little included air would alſo upon conſiſtent and even ſolid bodies emulate the operations of the weight of the atmoſphere. In the proſecution of which enquiry we thought fit to make two ſorts of trials: the one, where the air is included in the bodies, on which its ſpring does work; and the other, where it is external to them. Of the firſt ſort are this ſeventh and the two following experiments; and of the ſecond ſort are ſome other trials, to be comprehended under the tenth experiment.

HAVING formerly mentioned to your lordſhip, that we were ſeveral times able (though ſometimes not without much difficulty) to make a blown bladder break with the ſpring of its own air; I ſhould not think it worth while to ſay any thing here about the ſame phœnomenon, but that (beſides that it ſeems odd enough, and is not unpleaſant to many ſpectators) it may deſerve not to be wholly neglected, becauſe a good way to break bladders in the much exhauſted receiver may ſometimes prove an uſeful expedient, eſpecially in ſuch caſes, where the experimenter (who ſometimes either is not ſkilful enough, or well enough furniſhed with accommodations to regulate the ingreſs of the air) would very ſuddenly ſupply the receiver with freſh air, when it has been much emptied, without danger of letting in too much air from without. Not to mention, that the air, included in the bladder to be broken, may be ſo mingled with ſteams, or imbued with divers qualities, as to be much fitter than common air for ſome particular purpoſes.

WE ſhall then for the affinity's ſake between this trial and the former, ſubjoin now the way, by which we ſeldom failed of breaking bladders in our emptied receivers. For this purpoſe, the blown bladder, that was to be burſt, having the neck very cloſely and ſtrongly tied, was kept a pretty while in the receiver, whilſt the air was pumping out, and then taken out again, that, now the fibres were ſtretched and relaxed, the capacity being leſſened by a new ligature that I ordered to be ſtrongly made near the neck, the bladder might be leſſen'd though the air were but the ſame, and the membrane being not ſo capable of yielding as before, upon the ſecond exhauſtion of the receiver the bladder in it would break, far more eaſily than otherwiſe, and perhaps be oddly enough lacerated.

WE ſometimes alſo varied this way of diſpoſing bladders to be burſt, by omitting the preparatory putting in of the bladder into the receiver, and only taking it in a little near the neck, that, the bladder having not been blown very full at firſt, the tenſion of the included air might be greater. But this laſt way is to be made uſe of, when the thing we deſire is, that the bladder by breaking at a certain time may part with its air, and not when 'tis only to give an inſtance of the force of the ſpring of uncompreſſed air againſt the ſides of the veſſel that contain it.

EXPERIMENT VIII.

About the lifting up a conſiderable weight by the bare ſpring of a little air included in a bladder.

YOU will eaſily believe, that the force imployed (in the foregoing experiment) by the air, to break the well blown bladders it is included in, is conſiderable, if I here add, that a ſmall quantity of air, which will not fill ¼ of a bladder, will not only ſerve to blow it quite up, but will manifeſtly ſwell it, though that effect be oppoſed not only by the reſiſtance of the bladder it ſelf, but by a conſiderable weight tied to the bottom of it, as in the following experiment.

WE took a middle ſized bladder (of a hog or ſheep) and having preſſed out the air, till there remained but a fourth or fifth part (by gueſs) we cauſed the neck to be very ſtrongly tied up again: alſo round about the oppoſite part of the bladder, within about an inch of the bottom, we ſo ſtrongly tied another ſtring, that it would not be made to ſlip off by a not inconſiderable weight we hung at it. Then faſtening the neck of the bladder to the turning key, we conveyed the bladder and the weight hanging at it into a large receiver, in which when it began to be pretty well exhauſted, the air within the bladder being freed from the wonted preſſure of the air without it, did by its own ſpring manifeſtly ſwell, and thereby notably ſhorten the bladder that contain'd it, and by conſequence viſibly lifted up the weight, (that reſiſted that change of figure) which exceeded fifteen pound of ſixteen ounces to the pound.

AFTER that we took a larger bladder, and having let out ſo much air, that it was left lank enough, we faſtened the two ends of it to the upper part of the receiver, (for which elſe it would have been too long) and tied a weight (but not the ſame) ſo as that it hung down from the middle of the bladder: then exhauſting the receiver as before, though the bladder, and this new weight which ſtretched it, reached ſo low, as that for a while we could ſcarce ſee, whether it hung in the air or no, yet at length we perceived the bladder to ſwell, and concluded, that it had lifted up its clog about an inch; which was confirmed by the return we permitted of the air into the receiver, upon which the bladder became more wrinkled than before, and the weight deſcended, which being taken off, and weighed in a ſtatera, amounted to about 28 pounds. We would have reiterated the experiment, but ſo heavy a weight having broken the bladder, we were diſcouraged from proceeding any further, eſpecially in regard of the difficulty of bringing by this contrivance the ſtrength of the air's ſpring to any exact computation; though it ſufficiently ſhews what I deſigned it ſhould, namely, that the ſpring of a little included air may be able even in ſo ſlight a contrivance to raiſe a great weight.

WHETHER this experiment may any way illuſtrate the motion of muſcles, made by inflation, contraction, &c. it belongs not to this place to conſider.

EXPERIMENT IX.

About the breaking of hermetically ſealed bubbles of glaſs by the bare ſpring of their own air.

I SHALL premiſe to the following trials an experiment, wherein uncompreſſed air is made by its own bare ſpring to break the ſolid body itſelf it is ſhut up in. And this I the rather ſet down before the ſubſequent trials, becauſe in our already publiſhed phyſico-mechanical experiments mention has been made of this trial, as of one, that we could not then make to ſucceed; we have ſince, employing ſmaller receivers, made it often enough proſperouſly, ſomewhat to the wonder of eminent virtuoſi, who confeſſed to me they had made frequent and divers attempts to perform the ſame thing, without ever ſucceeding in any of them.

BUT it will not be requiſite to multiply relations about this particular, and therefore I ſhall ſet down but this one, which I meet with among my looſe notes.

A large glaſs bubble hermetically ſealed being put into the receiver, and the air drawn out as much as in uſual operations, and ſomewhat more, though I told the company beforehand, that I had ſeveral times obſerved, that ſuch bubbles would not break immediately, but ſomewhile after the withdrawing the air from about them; yet this continued ſo long entire after we had left off pumping, that preſuming it had been blown too ſtrong, I began to deſpair of the experiments ſucceeding; when, whilſt we were providing ſomething elſe to put into the receiver, and, as I gueſſed, four minutes after the pump had been let alone, the bubble ſurprized us with its being broken with ſuch violence by the ſpring of the included air, that the fragments of it were daſhed every way againſt the ſides of the receiver, and broken ſo very ſmall, that when we came to take it up, the powder was by the by-ſtanders compared to the ſmall ſand wont to be imployed to dry papers, that have been newly writ upon with ink. The reaſon why the bubble broke ſo ſlowly I cannot now ſtay to propoſe, no more than to examine whether the difficulty of breaking veſſels of glaſs, no thicker than theſe bubbles, proceed from ſome weakening of the ſpring of impriſoned air, by its ſtretching a little the including glaſs, (for in another caſe we have obſerved this glaſs to be ſtretchable by the preſſure of air) or from hence, that 'twas very hard, as I have elſewhere mentioned, to avoid rarifying the air a little, and conſequently weakening its ſpring, by the heat, that was neceſſary to be imployed about the ſealing up the bubble.

EXPERIMENT X.

Containing two or three trials of the force of the ſpring of our air uncompreſſed upon ſtable and and even ſolid bodies, (whereto it is external.)

IN proſecution of the inquiry propoſed in the title, we made (among others) the following trials.

The First Trial.

I. We took the braſs-hoop, mentioned in the fifth experiment (whoſe diameter is ſomewhat above three inches) and having cauſed a glazier to cut ſome plates of glaſs, ſuch as are uſed for making the quarrels of windows, till he had brought them to a ſize, and a roundneſs fit to ſerve for covers to that braſs-hoop, we carefully faſtened one of them with cement to the upper orifice of the hoop or ring, and then cementing the lower orifice to the engine, ſo that the veſſel, compoſed of the metal and glaſs, ſerved for a ſmall receiver; we whelmed over it a large and ſtrong receiver, which we alſo faſtened on to the engine with cement after the uſual manner. By which contrivance it was neceſſary, that when the pump was ſet on work, the included receiver (of braſs and glaſs) ſhould have its air withdrawn; and yet the air in the larger receiver ſhould not be pumped out but by breaking through the glaſs, ſo that the internal air of the metalline receiver (as we may call it for diſtinction ſake) being pumped out, the glaſs plate, that made part of that receiver, muſt lie expoſed to the preſſure of the ambient air ſhut up in the other receiver, without having the former aſſiſtance of the now withdrawn air to reſiſt the preſſure: wherefore, as we expected, at the firſt or ſecond exſuction of the air, included in the ſmall metalline receiver, the glaſs-plate was, by the preſſure of the incumbent air contained in the great receiver, broken into an 100 pieces, which were beaten inwards into the cavity of the hoop.

The Second Trial.

II. This done, to ſhew, that there needed not the ſpring of ſo great a quantity of included air to break ſuch glaſſes, we took another roundiſh one, which, though wide enough at the orifice to cover the braſs ring and the new glaſs-plate that we had cemented on it, was yet ſo low, that we eſtimated it to hold but a ſixth part of what the large receiver, formerly imployed, is able to contain; and having whelmed this ſmaller veſſel, which was ſhaped like thoſe cups they call tumblers, over the metalline receiver, and well faſtened it to the engine with cement, we found, that though this external receiver had a great part of its cavity filled by the included one, yet when this internal one was exhauſted by an exſuction or two, the ſpring of the little air that remain'd, was able to break the plate into a multitude of fragments.

The Third Trial.

III. Because the glaſs-plates hitherto mentioned ſeemed not ſo thick, but that the preſſure of the included air might be able to give conſiderable inſtances of its force; inſtead of the metalline receivers hitherto employed, we took a ſquare bottle of glaſs, which we judged to be able to contain about a pint (or pound) of water, and which had been provided to keep ſubtle chymical liquors in, for which uſe we are not wont to chuſe weak ones. This we inverted, and applied to the engine as a receiver, over which we whelmed the large receiver formerly mentioned; and having cemented it on, as in the foregoing experiments, we ſet the pump on work to empty the internal receiver (or ſquare bottle) by which means the withdrawing of the air, and the figure of the veſſel (which was inconvenient for reſiſting) ſuffered the preſſure of the air included in the external receiver to cruſh the vial into a great number of pieces.

And to vary this experiment, as we did that of breaking the metalline receivers, we took another glaſs of the ſhape and about the bigneſs of the former, and having applied it to the engine as before, and covered it with a receiver, that was little higher than it ſelf, we found, that upon the exhauſtion of the air the ſecond ſquare glaſs was likewiſe broken into many fragments, ſome of which were of ſo great a thickneſs, as moved ſome wonder, that the bare preſſure of the air was able to break ſuch a veſſel, though probably the cracks, that reached to them, were begun in much weaker parts of the glaſs.

N. B. 1. The bottoms and the necks of both theſe ſquare bottles were entire enough; by which it ſeemed probable, that the veſſels had been broken by the preſſure of the air againſt the ſides, which were not only thinner than the parts above named, but expoſed a larger ſuperficies to the lateral preſſure of the air, than to the perpendicular.

2. We obſerv'd in one of the two laſt experiments, that the veſſel did not break preſently upon the laſt exſuction, that was made of the included air, but a conſiderable time after, which it ſeems was requiſite to allow the compreſſed parts of the glaſs time to change their places: and this phænomenon I therefore mention, becauſe the ſame thing, that here happened in the breaking a glaſs inwards by the ſpring of the air, I elſewhere obſerved to have happened in breaking a glaſs outwards by the ſame ſpring.

3. To confirm, that it is the ſpring of the external receiver's air, that is the agent in thoſe fractures of glaſſes, and to prevent or remove ſome ſcruples, we thought fit to make this variation in the experiment. We applied a plate of glaſs, just like thoſe formerly mentioned, to the braſs-hoop; but in the cementing of it on, we placed in the thickneſs of the cement

a ſmall pipe of glaſs about an inch long, whoſe cavity was not ſo big as that of a ſtraw, and which being left open at both the ends, might ſerve for a little channel, through which the air might paſs from the external receiver to the internal: over this we whelmed one of the ſmall receivers abovementioned, and then, though we ſet the pump on work, much longer than would have needed, if this little pipe had not been made uſe of, we found, as we expected, that the internal receiver continued entire, becauſe the air, whoſe ſpring ſhould have broken it, having liberty to paſs through the pipe, and conſequently to expand itſelf into the place deſerted by the air pumped out, did by that expanſion weaken its ſpring too much, to retain ſtrength enough to break the metalline (or internal) receiver.

BUT here it is to be noted, that either the pipe muſt be made bigger than that lately mentioned, or the exſuction of the air muſt not be made by the pump as nimbly as we can, or otherwiſe the plate of glaſs may be broken, notwithſtanding the pipe; becauſe the air contained in the external receiver having a force much greater than is neceſſary to break ſuch a plate, it may well happen (as I have ſometimes found it do) that if the air be haſtily drawn out of the internal receiver, that air, which ſhould ſucceed in its room, cannot get faſt enough out of that external receiver through ſo ſmall a pipe; and the air remaining in that external receiver will yet retain a ſpring ſtrong enough to break the glaſs. To illuſtrate which, I ſhall propoſe this experiment; that ſometimes, when I have at the flame of a lamp cauſed glaſs bubbles to be blown with exceeding ſlender ſtems, if they were nimbly removed out of the flame whilſt they were ignited, they would according to my conjecture, be either broken, if they cooled too faſt, or compreſſed inward, if they long enough retained the ſoftneſs they had given them by fuſion. For the air in the bubble being exceeding rarified and expanded, whilſt the glaſs is kept in the flame, and coming to cool haſtily when removed from thence, loſes upon refrigeration the ſpring the heat had given it; and ſo, if the external air cannot preſs it faſt enough through the too ſlender pipe, there will not get in air enough to reſiſt the preſſure of the atmoſphere; and therefore, if this preſſure find the bubble yet ſoft, it will preſs it a little inwards, and either flatten it, or make a dimple in it, though the orifice of the pipe be left open.

EXPERIMENT XI.

Shewing, that mercury will in tubes be raiſed by ſuction no higher than the weight of the atmoſphere is able to impel it up.

IT is ſufficiently known, that the common opinion of philoſophers, and eſpecially of thoſe which follow *Ariſtotle*, has long been, and ſtill is, that the cauſe of the aſcenſion of water upon ſuction, and particularly in thoſe pumps, where the water ſeems of its own accord to follow the riſing ſucker, is nature's abhorrency of a vacuum. Againſt this received opinion divers of the modern philoſophers have oppoſed themſelves. But as ſome of them were vacuiſts, and others pleniſts, they have explicated the aſcenſion of water in ſucking-pumps upon very different grounds; ſo that many ingenious men continue yet irreſolved in this noble controverſy. Wherefore though I have formerly made, and now renew a ſolemn profeſſion, that I do not in this treatiſe intend to declare either for or againſt the being of a vacuum; and though I have * elſewhere occaſionally acknowledged my ſelf not to acquieſce fully in what either the ancient or the modern philoſophers have taught about the adequate cauſe of ſuction; (in the aſſigning of which, I think, I have ſhewn them to have been ſomewhat deficient;) yet ſince I think ſome experiments, of importance to this controverſy, may be better made by the help of our engine, than they have been by any inſtrument I have yet heard of, I ſhall now add the trials I made, to ſhew both, that whether there be, or may be a vacuum or not, there is no need to have recourſe to a *fuga vacui* to explicate ſuction; and alſo that whatever other cauſes have by *Gaſſendus* and *Carteſius* been ingeniouſly propoſed to explicate ſuction, it ſeems to depend clearly upon the weight of the atmoſphere, or in ſome caſes upon the ſpring of the air; though I deny not, that other cauſes may contribute to that preſſure of the air, which I take to be the grand and immediate agent in theſe phænomena.

WE took a braſs pipe bended like a ſiphon, and fitted at the bigger end with a ſtop-cock, &c. as is delineated in the figure, (which inſtrument for brevity ſake, I often call an exhauſting, or ſucking ſiphon) and to the ſlender end of this we faſtned with good cement the upper end of a cylindrical pipe of glaſs, of about fifty inches long, and open at both ends, and having the lower end open into a glaſs of ſtagnant quick-ſilver, whoſe upper ſuperficies reached a pretty deal higher than the immerſed orifice of the glaſs cane. Theſe things being thus prepared, we cauſed the pump to be ſet on work, whereby the air being by degrees drawn out of the exhauſting ſiphon, and conſequently of the glaſs cane that opened into it; the ſtagnant mercury was proportionably impelled up into the glaſs-pipe, until it had attained to its due height, which exceeded not 30 inches. And then, though there remained in the upper part of the pipe above 20 inches unfilled with quick-ſilver, yet we could not by farther pumping raiſe that fluid metal any higher.

See Plate III. Fig. 2. and the annotations at the cloſe of this experiment.

BY which it ſeems manifeſt enough, that whatever many learned men have taught, or others do yet believe about the unlimited power, that nature would exerciſe, to prevent what they call a vacuum; yet this power has its bounds, and thoſe depend not ſo much upon the exigency of that principle, which the ſchoolmen call a *fuga vacui*, as upon the ſpecifick gravity of the liquor to be raiſed by ſuction. For confirmation of which, we ſubſtituted inſtead

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* The place here meant is a paſſage in the Author's *Examen* of Mr. *Hobb*'s *dialogue* about the air.

ſtead of the ſtagnant mercury a baſon of water; and though inſtead of the many ſucks we had fruitleſly imployed to raiſe the quick-ſilver above the lately mentioned height, we now imployed but one exſuction, (or leſs than a full one) which did but in part empty the exhauſting ſiphon: yet the water upon the opening of the ſtop-cock was not only impelled to the very top of the glaſs-cane, but likewiſe continued running for a good while through the exhauſting ſiphon, and thence fell upon the plate of the engine; ſo that it ſeemed an odd ſpectacle to thoſe, that knew not the reaſon of it, to ſee the water running very briſkly of its own accord, as they imagined, out of the ſhorter leg of a ſiphon; eſpecially that leg being perhaps not above a quarter ſo long as the other. And here I muſt not omit this conſiderable circumſtance, that though ſometimes in the Torricellian experiment, I have obſerved the mercury to ſtand at thirty inches, and now and then above it, yet the height of the mercury elevated in our glaſs-cane appeared not, when meaſured, to reach fully 29 inches and a quarter; which I thought it was not difficult to render a reaſon of, from the varying weight of the atmoſphere; and accordingly conſulting the baroſcope (that ſtood in another room) I found the atmoſphere to be at that time ſomewhat light, the quick-ſilver in it being in height but 29 inches and an eighth, which probably would have been the very height of the quick-ſilver raiſed by the engine, if it had had time by ſtanding to free it ſelf from bubbles.

FROM whence we may conclude, that ſuction will elevate liquors in pumps no higher than the weight of the atmoſphere is able to raiſe them, ſince the cloſeneſs requiſite in the pump of our engine to be ſtaunch makes it very unlikely, that by any ordinary pump a more accurate ſuction can be effected.

I have nothing to add about the related experiment but this one; that it may afford us a notable confirmation of the argument we formerly propoſed againſt them, that aſcribed the elevation and ſuſtentation of the quick-ſilver in the Torricellian experiment to a certain rarified air, which the more highly it is rarified, the greater power it acquires to attract quick-ſilver, and other contiguous bodies; for in our experiment, though by continuing to pump we can rarify or diſtend more and more the air in the exhauſting ſiphon, yet we were not able to raiſe the mercury above thirty inches (which exceeds not the height, to which the atmoſphere is able to elevate it) and this, though the ſtagnant mercury being expoſed to the free air, it cannot be pretended (as in ſome other caſes it may, though not ſatisfactorily, be done) that the mercury cannot be raiſed higher, without offering violence to the body incumbent on the ſtagnant mercury: for in the experiment we are conſidering, if nature ſhould raiſe the quick-ſilver higher and higher in the pipe, to ſucceed in the room of the air that is withdrawn, the formerly ſtagnant mercury, that would on this occaſion be raiſed, might be immediately ſucceeded by the free and undilated air, ſo that nature would be put to offer violence to the quickſilver only, which if ſhe were ſcrupulous to do, what ailed her to raiſe it (as ſhe did in our trial) againſt the inclinations of ſo ponderous a body, to above 29 inches high?

ANNOTATION.

THOUGH the exhauſting ſiphon, mentioned at the beginning of this experiment, may be eaſily enough conceived by an attentive inſpection of the figure; yet becauſe I frequently made uſe of it in pneumatical experiments, it will not be amiſs to intimate here once for all theſe three particulars about it. 1. That though the bending pipe itſelf may be for ſome uſes more conveniently made of glaſs than of metal, becauſe the tranſparency of the former may enable us to diſcover what paſſes in it; yet for the moſt part we chuſe to imploy pipes of the latter ſort, becauſe the others are ſo very ſubject to break. 2. That it is convenient to make the longer leg of the ſiphon a little larger at the bottom than the reſt of the pipe uſually needs to be, that it may the more commodiouſly admit the ſhank of a ſtop-cock, which is to be very carefully inſerted with cement; by ſeaſonably turning and returning of which ſtop-cock, the paſſage (for the air) between the engine and the veſſel to be exhauſted is to be opened and ſhut. 3. That though we ſometimes content our ſelves to apply immediately the braſs ſiphon itſelf to the engine, by faſtening with cement the external ſhank of the ſtop-cock to the orifice of the little pipe, through which the exſuction of the air is made; yet the bended pipe alone, if it be not almoſt conſtantly held, is ſo apt to be looſened by the motion of the engine, and the turning of the ſtop-cock, (which frequently occaſions leaks, and diſturbs the operation) that for the moſt part we make uſe of a ſiphon, conſiſting of a braſs pipe, and ſtop-cock, and a glaſs of 6, 8, or 10 inches in height, and of ſome ſuch ſhape (for it need not be the very ſame) as that repreſented in the figure: for by this means, though the exhauſtion is becauſe of this additional glaſs ſomewhat longer in making, yet it is more ſecurely and uninterruptedly carried on by reaſon of the ſtability, which the breadth of the lower orifice of the glaſs gives to the whole inſtrument. Beſides which, we have theſe other conveniences, that not only the ſiphon is hereby much lengthened, which in divers trials is very fit; but alſo, that we may commodiouſly place in the glaſſy part of this compounded ſiphon a gage, whereby to diſcern from time to time, how much the air is drawn out of the veſſel to be exhauſted.

See Plate III. Fig 2.

EXPERIMENT XII.

About the differing heights, whereto liquors will be elevated by ſuction, according to their ſeveral ſpecifick gravities.

IF, when I was making the foregoing experiment, I had been able to procure a pipe long enough, I had tried to what height I could raiſe water by ſuction, though I would have done it rather to ſatisfy others than my ſelf, who ſcarce doubted, but that as water is (bulk for bulk) about 14 times lighter than quick-ſilver: ſo it would have been raiſed by ſuction to about four or five and thirty foot, (which is 14 times as high as we were able to elevate the quick-ſilver) and no higher. But being not furniſhed for the trial I would have made, I thought fit to ſubſtitute another, which would carry the former experiment ſomewhat further. For whereas, in that we ſhewed how high the atmoſphere was able by its whole gravitation to raiſe quick-ſilver; and whereas likewiſe that, which appears in Monſieur *Paſchal*'s experiment, is, at what height the whole weight of the atmoſphere can ſuſtain a cylinder of water: by the way, that I thought on, it would appear (which hath not yet, that I know of, been ſhewn) how a part of the preſſure of the air would in perpendicular pipes raiſe not only the two mentioned liquors, but others alſo to heights anſwerable to the degree of preſſure, and proportionable to the ſpecifick gravities of the reſpective liquors.

Plate III. Fig 3.

To make this trial the more clear and free from exceptions, I cauſed to be made and inſerted to the ſhorter leg of the above mentioned exhauſting ſiphon a ſhort pipe; which branched itſelf equally to the right hand and the left, as the adjoining figure declares. In which contrivance I aimed at theſe two conveniences: one that I might exhauſt two glaſs-canes at the ſame time; and the other, to prevent its being ſurmiſed, that the engine was not equally applied to both the glaſſes to be exhauſted. This additional braſs-pipe being carefully cemented into the ſucking ſiphon, we did to each of its two branches take care to have well faſtened with the ſame cement a cylindrical glaſs of about 42 inches in length (that being ſomewhat near the height of our exhauſting ſiphon above the floor) the lower orifice of one of theſe two glaſſes being immerſed in a veſſel of ſtagnant mercury, and that of the other in a veſſel of water, where care was taken by thoſe I employed, that as the tubes were choſen near of a bigneſs, (which yet was not neceſſary) ſo the ſurfaces of the two different liquors ſhould be near of a height. This being done, we began to pump warily and ſlowly, till the water in one of the pipes was elevated to about 42 inches; and then meaſuring the height of the quick-ſilver in the other pipe above the ſurface of the ſtagnant quick-ſilver, we found it to be almoſt three inches; ſo that the water was about 14 times as high as the quick-ſilver. And to proſecute the experiment a little further, we very warily let in a little air to the exhauſting ſiphon, and had the pleaſure to ſee the two liquors proportionably deſcend, till turning the ſtop-cock, when the water was about 14 inches high, we thereby kept them from ſinking any lower, till we had meaſured the height of the quick-ſilver, which we found to be about one inch.

We tried alſo the proportion of theſe two liquors at other heights, but could not eaſily meaſure them ſo well as we did at thoſe newly mentioned; and therefore though there ſeemed to be ſome ſlight variation, yet we looked upon it but as what might be well imputed to the difficulty of making ſuch experiments exactly; and this diſpleaſed me not in theſe trials, that whereas it was obſerved, and ſomewhat wondered at, that the quick-ſilver for the moſt part ſeemed to be ſomewhat (though but a very little) higher then the proportion of 1 to 14 required, I had long before by particular trials found, that though 14 and 1 be the neareſt of ſmall integer numbers, that expreſs the proportion between the ſpecifick gravities of quick-ſilver and water, yet the former of thoſe fluids (or at leaſt that, which I made my trials with) is not quite ſo heavy as this proportion ſuppoſes, though I ſhall not here ſtay to determine preciſely the difference, having done it in another tract, where the method I employed in the inveſtigation of it is alſo ſet down.

The above-mentioned experiment, made by the help of our engine, as to quick-ſilver and water being confirmable by trials (to be by and by mentioned) made in other liquors, affords our hypotheſis two conſiderable advantages above the vulgar doctrine of the ſchools (for I do not apply what follows to all the pleniſts) who aſcribe the aſcenſion of liquors by ſuction to a traction made *ob fugam vacui*, as they are wont to ſpeak:

For firſt it is manifeſtly agreeable to our doctrine, that, ſince the air, according to it, is a fluid, that is not void of weight, it ſhould raiſe thoſe liquors, that are lighter, as water, higher then thoſe that are ponderous, as quick-ſilver; and that anſwerably to the diſparity of their weights. And ſecondly, there is no reaſon, why, if the air be withdrawn by ſuction from quick-ſilver and water, there ſhould be leſs left a vacuum above the one than above the other, in caſe either of them ſucceed not in the place deſerted by the air; and conſequently when the air is withdrawn out of both the forementioned glaſs-pipes, if there would be no vacuum in caſe no liquor ſhould ſucceed it, why does nature needleſly to prevent a vacuum make the water, that is an heavy body, aſcend contrary to its own nature, according to which it tends towards the center of the earth? And if the ſucceeding of a liquor be neceſſary to prevent a vacuum, how chance that nature does not elevate the quick-ſilver as well as the water; eſpecially ſince it is manifeſt by the foregoing experiment, that ſhe is able to raiſe that ponderous liquor above 26 inches higher than ſhe did in the experiment we are now diſcourſing of.

Perhaps it would not be amiſs to take notice, on this occaſion, that among other applications of this experiment it may be made ſomewhat uſeful to eſtimate the differing gravities of liquors; to which purpoſe I cauſed to be put under the bottom of the forementioned glaſs-pipes two veſſels, the one with freſh water, and the other with the like water impregnated with a good proportion of ſea-ſalt, that I had cauſed to be diſſolved in it, for want of ſea-water, which I would rather have employed. And I found, that when the freſh water was raiſed to about 42 inches, the ſaline ſolution had not fully reached to 40.

But though this difference were double

to that, which the proportion and gravity betwixt our ſea-water and freſh water would have required; yet to make the diſparity more evident, and alſo becauſe I would be able the better to gueſs at the proportion of the diſſolved ſalt, by making it as great as I could, I cauſed an unuſual brine to be made, by ſuffering ſea-ſalt to deliquate in the moiſt air. And having applied this liquor and freſh water to the two already mentioned pipes, and proceeded after the former manner, we found, that when the pure water was elevated to near 42 inches, the liquor of ſea-ſalt wanted about 7 inches and a quarter of that height; and when the water was made to ſubſide to the middle of its pipe, or thereabouts, the ſaline liquor in the other pipe was between 3 and 4 inches lower than it.

I would have tried the difference between theſe liquors and oil, but the coldneſs of the weather was unfavourable to ſuch a trial: but to ſhew a far greater diſparity than that would have done betwixt the height of liquors of unequal gravities, I took fair water, and a liquor made of the ſalt of pot-aſhes ſuffered to run in a ſellar *per deliquium*, (this being one of the ponderouſeſt liquors I ever prepared,) and having proceeded as in the former trials, I found, that when the common water was about 42 inches high, the newly mentioned ſolution wanted ſomewhat of 30 inches; and when the water was made to ſubſide to the middle of its pipe, or thereabouts, the deliquated liquor was between 6 and 7 inches lower than it.

I had ſome thoughts, when I applied my ſelf to make theſe trials, to examine how well we could by this new way compare the ſaltneſs of the waters of ſeveral ſeas, and thoſe alſo of ſalt-ſprings; and likewiſe whether, and (if any thing near) how far we might by this method determine the proportion of the more ſimple liquors, that may be mingled in compounded ones, as in the mixture of water and wine, vinegar and water, &c. but being not provided with inſtruments fit for ſuch nice trials, and a miſchance having impaired the glaſſes lately mentioned before the laſt trials were quite ended, and having ſoon after broken one of them, I laid aſide thoſe thoughts.

EXPERIMENT XIII.

About the heights, to which water and mercury may be raiſed, proportionably to their ſpecifick gravities, by the ſpring of the air.

IN proſecution of the parallel formerly begun, betwixt the effects of the weight of the atmoſphere, and the ſpring of included air, we thought fit, after the foregoing, to make the following experiment.

WE took a ſtrong glaſs-bottle, capable to hold above a pint of water, and having in the bottom of it lodged a convenient quantity of mercury, we poured on it a greater quantity of water, (becauſe this liquor was to be impelled up many times higher than the other,) and having provided two ſlender glaſs-pipes, each open at both ends, we ſo placed and faſtened them, by means of the cement, wherewith we choaked the upper part of the neck of the bottle, that the ſhorter of the pipes had its lower orifice immerſed beneath the ſurface of the quick-ſilver, and the longer pipe reached not quite ſo low as that ſurface, and ſo was immerſed but in the water, by which contrivance we avoided the neceſſity of having two diſtinct veſſels for our two ſtagnant liquors, which would have been inconvenient in regard of the ſlenderneſs of the upper part of our receiver. This done, we conveyed the bottle into a fitly ſhaped receiver, (formerly deſcribed at the firſt experiment,) and having begun to pump out the air, we took notice to what heights the quick-ſilver and water were impelled up in their reſpective tubes, on which we had before made marks from inch to inch with hard wax, (that they might not be removed by wet or rubbing,) and we obſerved, that when the quick-ſilver was impelled up to two inches, the water was raiſed to about eight and twenty; and when the quick-ſilver was about one inch high, the water was about fourteen. I ſay, about, partly becauſe ſome allowances muſt be made for the ſinking of the ſuperficies of the ſtagnant quick-ſilver, and the greater ſubſidence of that of the ſtagnant water, by reaſon of the liquors impelled into the two pipes; partly, becauſe that the breadth of the mark of wax was conſiderable, when the quick-ſilver was but about an inch high, and ſo made it difficult to diſcern the exact height of the metal, when the water was fallen down to fourteen inches: eſpecially in regard, that the quick-ſilver never aſcending ſo high as the neck of the bottle, (which the water left far beneath it,) the thickneſs of the receiver, and that of ſo ſtrong a bottle, made it difficult to diſcern ſo clearly the ſtation of the quick-ſilver as I could have wiſhed.

EXPERIMENT XIV.

About the heights anſwerable to their reſpective gravities, to which mercury and water will ſubſide, upon the withdrawing of the ſpring of the air.

FOR the further illuſtration of the doctrine propoſed in the laſt, and ſome of the foregoing experiments, about the raiſing and ſuſtentation of liquors in pipes by the preſſure of the air; I thought it not unfit to make the following trial, though it were eaſy to foreſee in this peculiar experiment a peculiar difficulty.

WE cauſed then to be conveyed into a fitly ſhaped receiver two pipes of glaſs very uneven in length, but each of them ſealed at one end: the ſhorter tube was filled with mercury, and inverted into a ſmall glaſs jarr, wherein a ſufficient quantity of that liquor had been before lodged: the longer pipe was filled with common water, and inverted into a larger glaſs, wherein likewiſe a fit proportion of the ſame liquor had been put.

THEN the receiver being cloſely cemented on to the engine, the air was pumped out for a pretty while before the mercury began to ſubſide; but when it was ſo far withdrawn, that its preſſure was no longer able to keep up a mercurial cylinder of that height, that liquid metal began to ſink; the water in the other tube, though this were three times as long, ſtill retaining its full height. But when the quick-ſilver was fallen ſo low, as to be but between three and four inches above the ſurface of the ſtagnant quick-ſilver, the water alſo began to ſubſide, but ſooner than according to the laws of meer ſtaticks it ought to have done, becauſe many aerial particles emerging from the body of the water to the upper part of the glaſs, did by their ſpring concur with the gravity of the water to depreſs this liquor. And ſo when the quick-ſilver was three inches above the ſtagnant mercury, the water in the other pipe was fallen divers inches beneath 42, and ſeveral inches beneath 28, when the mercury had ſubſided an inch lower. But this being no more than was to be expected, after we had cauſed the pumping to be a while continued, to free the water the better from the latitant air, we let in the external air; and having thereby impelled up again both the liquors into their pipes, and removed the receiver, we took out thoſe pipes, and inverting each of them again to let out the air, (for even that, which held the quick-ſilver had got a ſmall bubble, though inconſiderable in compariſon of the air that had got up out of the water,) we filled each of them with a little of the reſtagnant liquor belonging to it; and inverting each tube once more into its proper liquor, we repeated the experiment, and found it, as it ſeemed, to require more pumping than before to make the liquors begin to ſubſide; ſo that when the mercury was fallen to three inches, or two or one, the water ſubſided ſo near to the heights of 42, 28, or 14 inches, that we ſaw no ſufficient cauſe to hinder us from ſuppoſing, that the little differences, that appeared between the ſeveral heights of the quick-ſilver, and fourteen times as great heights of the water (which fell ſomewhat lower than its proportion in gravity required) proceeded from ſome aerial corpuſcles yet remaining, in ſpite of all we had done, in the water, and by their ſpring, though but faint, when once they had emerged to the upper part of the glaſs, furthering a little the depreſſion of it: not now to mention leſſer circumſtances, particularly, that the ſurface of the ſtagnant water did not inconſiderably riſe by the acceſſion of the water lately in the pipe; whereby the cylinder of water, raiſed above that ſurface, became by ſo much the ſhorter. However your lordſhip may, if you think fit, cauſe the experiment to be reiterated, which I could not ſo well do, by reaſon of a miſchance that befel the receiver.

EXPERIMENT XV.

About the greateſt height, to which water can be raiſed by attraction or ſucking pumps.

SINCE the making and the writing of the foregoing experiments, having met with an opportunity to borrow a place ſomewhat convenient to make a trial, to what height water may be raiſed by pumping; I thought not fit to neglect it. For though both by the conſideration of our hypotheſis, to whoſe truth ſo many phænomena bear witneſs; and though particularly by the conſequences deduceable from the three laſt recited experiments, I were kept from doubting what the event would be; yet I thought it worth while to make the trial.

I know what is ſaid to have been the complaint of ſome pump-makers. But I confeſs the phænomenon, it was grounded on, ſeemed not to me to be certainly enough delivered by a writer or two, that mention what they complained of; and their obſervation ſeems not to have been made determinately or carefully enough for a matter of this moment. Since that, which they complain of, ſeems to have been in general, that they could not by pumping raiſe water to what height they pleaſe, as the common opinion of philoſophers about nature's *fuga vacui* made them expect they might. And it may well have happened, that as they endeavoured only to raiſe it to the height their occaſions required, ſo all that their diſappointment manifeſted, was, that they could not raiſe it to that particular height: which did not determine, whether, if the pump had been a foot or a yard ſhorter, the water would then have been elevated to the upper part of it or no: but that which I chiefly conſider is, that theſe being but tradeſmen, that did not work according to the dictates of, or with deſign to ſatisfy a philoſophical curioſity, we may juſtly ſuſpect, that their pumps were not ſufficiently ſtanch, nor the operation critically enough performed and taken notice of.

WHEREFORE, partly becauſe a trial of ſuch moment ſeemed not to have yet been duly made by any; and partly becauſe the varying weight of the atmoſphere was not (that appears) known, nor (conſequently) taken into conſideration by the ingenious Monſieur *Paſchal* in his famous experiment, which yet is but analogous to this; and partly, becauſe ſome very late, as well as learned writers, have not acquieſced in his experiment, but do adhere to the old doctrine of the ſchools, which would have water raiſeable in pumps to any height, *ob fugam vacui*, (as they ſpeak,) I thought fit to make the beſt ſhift I could to make the trial, of which I now proceed to give your lordſhip an account.

THE place I borrowed for this purpoſe was a flat roof about 30 foot high from the ground, and with rails along the edges of it. The tube we made uſe of ſhould have been of glaſs, if we could have procured one long and ſtrong enough. But that being exceeding difficult, eſpecially for me, who was not near a glaſs-houſe, we were fain to cauſe a tin-man to make ſeveral pipes of above an inch bore, (for of a great length it was alledged they could not be made ſlenderer,) and as long as he could, of tin or latten, as they call thin plates of iron tinned over; and theſe being very carefully ſodered together made up one pipe, of about one or two and thirty foot long, which being tied to a pole, we tried with water whether it was ſtanch, and by the effluxions of that liquor finding where the leaks were, we cauſed them to be ſtopped with ſoder; and then for greater ſecurity, the whole pipe, eſpecially at the commiſſures, was diligently caſed over with our cloſe black cement, upon which plaiſter of Paris was ſtrewed to keep it from ſticking to their hands or cloaths, that ſhould manage the pipe. At the upper part of which was very carefully faſtened with the like cement a ſtrong pipe of glaſs, of between 2 and 3 foot in length, that we might ſee what ſhould happen at the top of the water. And to the upper part of this pipe was (with cement, and by the means of a ſhort elbow of tin) very cloſely faſtened another pipe of the ſame metal, conſiſting of two pieces, making a right angle with one another, whereof the upper part was parallel to the horizon, and the other, which was parallel to the glaſs-pipe, reached down to the engine, which was placed on the flat roof, and was to be with good cement ſollicitouſly faſtened to the lower end of this deſcending part of the pipe, whoſe horizontal leg was ſupported by a piece of wood, nailed to the abovementioned rails; as the tube alſo was kept from overmuch ſhaking by a board, faſtened to the ſame rails, and having a deep notch cut in it, for the tube to be inſerted into.

See Plate V. Fig. 1. This apparatus being made, and the whole tube with its pole erected along the wall, and faſtened with ſtrings and other helps, and the deſcending pipe being carefully cemented on to the engine, there was placed under the bottom of the long tube a convenient veſſel, whereinto ſo much water was poured, as reached a great way above the orifice of the pipe, and one was appointed to ſtand by to pour in more as need ſhould require; that the veſſel might be ſtill kept competently full.

After all this, the pump was ſet on work; but when the water had been raiſed to a great height, and conſequently had a great preſſure againſt the ſides of the tube, a ſmall leak or two was either diſcovered or made, which without moving the tube we cauſed to be well ſtopped by one, that was ſent up a ladder to apply ſtore of cement where it was requiſite.

Wherefore, at length we were able, after a pretty number of exſuctions, to raiſe the water to the middle of the glaſs-pipe abovementioned, but not without great ſtore of bubbles, made by the air formerly concealed in the pores of the water, and now emerging; which for a pretty while kept a kind of foam upon the ſurface of it, (freſh ones continually ſucceeding thoſe that broke.) And finding the engine and tube as ſtanch as could be well expected, I thought it a fit ſeaſon to try what was the utmoſt height, to which water could by ſuction be elevated; and therefore, though the pump ſeemed to have been plied enough already, yet for further ſatisfaction, when the water was within few inches of the top of the glaſs, I cauſed 20 exſuctions more to be nimbly made, to be ſure that the water ſhould be raiſed as high as by our pump it could be poſſibly. And having taken notice where the ſurface reſted, and cauſed a piece of cement to be ſtuck near it, (for we could not then come to reach it exactly,) and deſcending to the ground where the ſtagnant water ſtood, we cauſed a ſtring to be let down, with a weight hanging at the end of it, which we applied to a mark, that had been purpoſely made at that part of the metalline tube, which the ſuperficies of the ſtagnant water had reſted at, when the water was elevated to its full height: and the other end of the ſtring being, by him that let it down, applied to that part of the glaſs, as near as he could gueſs, where the upper part of the water reached, the weight was pulled up; and the length of the ſtring, and conſequently the height of the cylinder of water was meaſured, which amounted to 33 foot, and about 6 inches. Which done, I returned to my lodging, which was not far off, to look upon the baroſcope, to be informed of the preſent weight of the atmoſphere, which I found to be but moderate, the quickſilver ſtanding at 29 inches, and between 2 and 3 eights of an inch. This being taken notice of, it was not difficult to compare the ſucceſs of the experiment with our hypotheſis. For if we ſuppoſe the moſt received proportion in bulk between cylinders of quick-ſilver and of water of the ſame weight, namely that of 1 to 14, the height of the water ought to have been 34 foot and about 2 inches, which is about 8 inches greater than we found it. But then your lordſhip may be pleaſed to remember, that I formerly noted, before ever I made this experiment, that I did not allow the proportion betwixt mercury and water (at leaſt ſuch water as I made my trials with) to be altogether ſo great; and though in ordinary experiments, we may with very little inconvenience make uſe of that proportion to avoid fractions, yet in ſo tall a cylinder of water as ours was, the difference is too conſiderable to be neglected. If therefore, inſtead of making an inch of quick-ſilver equivalent to 14 inches of water, we abate but a quarter of an inch, which is but a 56 part of the height of the water, this abatement being repeated 29 times and a quarter, will amount to 7 inches, and above a quarter; which added to the former height of the water, namely 33 foot and 6 inches, will make up 34 foot and above an inch; ſo that the difference between the height of the mercury ſuſtained by the weight of the atmoſphere in the baroſcope, and that of the water raiſed and ſuſtained by the preſſure of the ſame atmoſphere in the long tube, did not appear to differ more than an inch or two from the proportion they ought to have had, according to the difference of their ſpecifick gravities. And though in our experiment the difference had been greater, provided it exceeded not 8 or 10 inches, it would not have been ſtrange; partly, becauſe of the difficulty of meaſuring all things ſo exactly in ſuch an experiment; partly, becauſe as waters are not all of the ſame weight, ſo a little diſparity of it in ſo long a cylinder may be conſiderable; and partly, and perhaps chiefly, becauſe the air flying out of the bubbles, that roſe out of ſo great a quantity of water, and breaking at the top of it, and ſo near that of the tube, might by its ſpring, though but very weak, aſſiſting the weight of ſo much water, ſomewhat (though not much) hinder the utmoſt elevation of that liquor. But our experiment did not make it needful for me to inſiſt on theſe conſiderations; and the inconſiderable difference, that was betwixt the height of the water we found, and that which might have been wiſhed, did rather countenance, than at all diſfavour the thing to be made out by our experiment, ſince by no pumping we could raiſe the water quite ſo high (though I confeſs it wanted but very little) as the weight of the atmoſphere was able to keep up a cylinder of mercury proportionable to it in height, and equivalent in weight: and yet I preſume, your lordſhip will eaſily grant, that there was at leaſt as much care uſed in this experiment, to keep the things imployed about it tight, as has been wont to be uſed by tradeſmen in their pumps, where it is not ſo eaſy either to prevent a little inſinuation of the air, or to diſcern it.

IT is not that I am ſure, that even all our care would have kept the water for any long time at its full height; but that the air was ſufficiently exhauſted for our purpoſe, when we determined the height of the water, I was induced to conclude by theſe circumſtances.

I. As well the conſtruction of the engine, as the many formerly related experiments, that have been ſucceſsfully tried with it, ſhew, that it is not like it ſhould be inferior in cloſeneſs to the great water-pumps, made by ordinary tradeſmen: and particularly the XIth experiment foregoing manifeſts, that by this pump quick-ſilver was raiſed to as great a height, as the atmoſphere is able to ſupport in the Torricellian experiment.

II. THE ſtanchneſs of the pipe appeared by the diminution (as to number) of bubbles, that appeared at the top of the water, and by their ſize too; for when there was a leak, (though but ſo very ſmall, that the water could not get out at it in the tube) it might uſually be taken notice of by the attentive ear of him, that ſtood to watch upon the ladder, erected by the ſide of the tube; and the air, that got in, did eaſily diſcover itſelf to the eye by large bubbles, manifeſtly differing from thoſe, that came from the aerial particles belonging to the water; and if the leak were not ſo very ſmall, the air that got in would ſuddenly lift up the water above it, and perhaps fill with it the deſcending pipe.

III. THOUGH there had been ſome imperceptible leak, yet that would not have hindred the ſucceſs of the experiment for the main. For in leaks, that have been but ſmall, though manifeſt enough, we have often, by cauſing the pump to be plied leſs nimbly than it now was, been able to proſecute our trials; becauſe the pump carried off ſtill more air than could get in at a leak that was no greater.

IV. AND that little or no intruding air was left in the upper part of our tube, was evident by thoſe marks, whereby it was eaſy for them, that are well acquainted with the pump, to eſtimate what air is left in the veſſel it ſhould exhauſt; and particularly towards the end of our operation I obſerved, that when the ſucker was depreſſed, there came out of the water, that covered the pump, ſo very few bubbles, that they might be imputed to the air afforded by the bubbles, ſpringing from the water in the tube; whereas if any adventitious air had got into that cylinder of water, it would have appeared in the water that covered the pump.

V. LASTLY, it were very ſtrange, that if the water was but caſually hindred by ſome leak from aſcending any higher, it ſhould be ſo eaſy to raiſe it to the very number of feet that our hypotheſis requires, and yet we ſhould be unable by obſtinate pumping to raiſe it one foot higher.

N. B. 1. As ſoon as we had made our experiment, and thereby found, that what was requiſite to it was in order; I ſent to give notice of it to Dr. *Wallis* and Dr. *Wren*, as perſons, whoſe curioſity makes them as well delighted with ſuch trials, as their deep knowledge makes them moſt competent judges of them. But before they could be found, and come, it being grown ſomewhat late and windy, I, that was not very well, and had tired myſelf with going up and down, could not ſtay with them ſo long as I intended, but leaving the reſt of the repeated experiments to be ſhewn them by *I. M.* (who had been very induſtrious in fitting and erecting the tube) they and their learned friend (whom they brought with them) Dr. *Millington*, told me a while after, that they alſo had found the greateſt height, to which they could raiſe the water, to be 33 foot and an half.

2. WHEN the water began firſt to appear in the glaſs, the bubbles would be, as I had foretold, exceeding numerous, ſo as to make a froth of near a foot high, if the water were newly brought, and had never been raiſed in the tube before. But if the pumping were long continued, the number and height (or at leaſt one of the two) of the aggregate of bubbles, would (as there remained fewer and fewer aerial particles in the water) be leſſer and leſſer; but their emerging did never, that I remember, wholly ceaſe.

3. AT the beginning alſo there would appear great vibrations of the water in the upper part of the tube; the riſing and the falling amounting ſometimes to a foot, or near half a yard: but theſe grew leſſer and leſſer, as thoſe of the quick-ſilver in the Torricellian experiment uſe to do.

4. ONE may uſe an ordinary pail to hold the ſtagnant water; but we rather employed a veſſel of earth, made for another purpoſe, ſomewhat ſlender, and of a cylindrical ſhape, becauſe in a narrow veſſel it is more eaſy to gueſs by the riſing and falling of the liquor, how the pump is plied, and to perceive even ſmaller leaks.

5. I muſt not forget to take notice, that though the newly named gentlemen came to me (when they had ſeen the experiment tried) within leſs than an hour after the time I had looked upon the baroſcope, and obſerved the quick-ſilver to ſtand ſomewhat beneath 29 inches, and three eighths; yet when preſently

upon their return I conſulted the ſame inſtrument again, the mercury appeared to be ſenſibly riſen, being ſomewhat (though but very little) above nine and twenty inches, and three eighths; and five or ſix hours after (at bed-time) I found it to be yet more conſiderably riſen. Which may keep your lordſhip from wondering at what I intimated a little above, touching Monſieur *Paſchal*'s experiment, as well as touching the diſappointment of the pump-makers endeavours. For it is not only poſſible, that (as I have elſewhere noted) water may be raiſed in the ſame pump, though we ſuppoſe it ſtill equally ſtanch, higher at one time than at another: but it was contingent, that, in Monſieur *Paſchal*'s noble attempt to imitate the Torricellian experiment with water inſtead of quickſilver, the proportion betwixt the heights of thoſe two liquors in their reſpective tubes anſwered ſo well to their ſpecifick gravities. For, the varying weight of the atmoſphere being not then, that appears, known, or conſequently taken into conſideration; if Monſieur *Paſchal* having tried the Torricellian experiment, when the air was for inſtance very heavy, had tried his own experiment, when the atmoſphere had been as light as I have often enough obſerved it to be, he might have found his cylinder of water to have been half a yard or two foot ſhorter than the formerly meaſured height of the quick-ſilver would have required.

I have now no more to add about this fifteenth experiment, but that it may ſerve for a ſufficient confirmation of what I note in another treatiſe, againſt thoſe hydraulical and pneumatical writers, who pretend to teach ways of making water paſs by inflected pipes, and by the help of ſuction, from one ſide of a mountain to the other, be the mountain never ſo high. For, if the water be to aſcend as it were ſpontaneouſly above 35 or 36 foot, a ſucking pump will not ordinarily, at leaſt here in *England*, be able to raiſe it.

AND now I ſpeak of mountains, it will not be altogether impertinent to add, that if it had not been for unſeaſonable weather, I had thought fit to make the foregoing eleventh experiment (of elevating mercury by ſuction) to be tried at the top of an hill, not far from the place I then was at. For by what has been already delivered, it appears, that we might have eſtimated the height, to which the water may be there elevated by ſuction, without repeating the experiment with a thirty five foot tube, (which we could not hope for conveniency to do) by the utmoſt height, to which our engine could have raiſed mercury: and it may be of ſome uſe to be able from experiments to make ſome eſtimate (for it can ſcarce be an accurate one) how much it may be expected, that pumps ſhall (*cæteris paribus*) loſe of their power of elevating water by ſuction, by being employed at the top of an hill, inſtead of being ſo at the bottom, or on a plain. Remembering always what I lately intimated, that even in the ſame place liquors will be brought to aſcend by ſuction to a greater or leſs height at one time than another, according to the varying gravity of the atmoſphere.

EXPERIMENT XVI.

About the bending of a ſpringy body in the exhauſted receiver.

THE cauſe of the motion of reſtitution in bodies, and conſequently of that, which makes ſome of them ſpringy, which far the greater part of them are not, has been ingeniouſly attempted by ſome modern corpuſcularians, and eſpecially Carteſians. But ſince divers learned and judicious men do ſtill look upon the cauſe of elaſticity, as a thing, that needs to be yet farther enquired into; and becauſe I am not myſelf ſo well ſatisfied as to blame their curioſity, I held it not unfit to examine by the help of our engine their conjecture, who imagine, that the air may have a great ſtroke in the making of bodies ſpringy; and this I the rather did, becauſe I had * elſewhere ſhewn, that there is no need to aſſert, that in all bodies, that have it, the elaſtical power flows immediately from the form, but that in divers of them it depends upon the mechanical ſtructure of the body.

TO make ſome trial therefore, whether the air have any great intereſt in the motion of reſtitution, we took a piece of whalebone of a convenient bigneſs and length; and having faſtened one end of it in a hole made in a thick and heavy trencher, to be placed on the plate of the engine, we tied to the other end a weight, whereby the whalebone was moderately bent, the weight reaching down ſo near to a body placed in a level poſition under it, that if the ſpring were but a little weakened, the weight muſt either lean upon, or at leaſt touch the horizontal plain: or if on the other ſide the ſpring ſhould grow ſenſibly ſtronger, it might be eaſily perceived by the diſtance of the weight, which was ſo near the plain, that a little increaſe of it muſt be viſible.

THIS done, we conveyed theſe things into the receiver, and ordered thoſe that pumped to ſhake it as little as they could, that the weight might not knock againſt the body that lay under it, or ſo ſhake it, as to hinder us from diſcerning, whether or no it were depreſſed by the bare withdrawing of the air.

AND when the air had been well pumped out, I watched attentively, whether any notable change in the diſtance of the weight from the almoſt contiguous plain would be produced upon its being let in again: for the weight was then at reſt, and the returning air flowing in much more ſpeedily than it could before be drawn out, I thought this the likelieſt time to diſcover, whether the abſence of the air had ſenſibly altered the ſpring of the whalebone. But though the experiment were made more than once, I could ſatisfy myſelf only in this, that the depreſſion or elevation of the weight, that was due to the true and meer change of the ſpring, was not very conſiderable, ſince I did not think my ſelf ſure, that I perceived any at all: for though it be true, that ſometimes, when the receiver was well exhauſted, the weight ſeemed to be a little depreſſed, yet that I thought was very little, if any thing more than what might be aſcribed to the abſence of the air, not conſidered as a body, that had any thing to do directly with the ſpring, but as a body, that had ſome (though but a little) weight; upon which account it made the medium, wherein the experiment was tried, contribute to ſupport the weight, that bent the ſpring; which weight, when the air was abſent, muſt (being now in a lighter medium) have its gravitation increaſed by as much weight, as a quantity of the exhauſted air, equal to it in bulk, could amount to. But this experiment being tried only with whalebone, and in a receiver not very great, may deſerve to be further tried in taller glaſſes, with ſprings of other kinds, and by the motions of a watch, and other more artificial contrivances.

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* In notes about the hiſtory of elaſticity.

EXPERIMENT XVII.

About the making of mercurial, and other gages, whereby to eſtimate how the receiver is exhauſted.

BECAUSE the air being inviſible, it is not always eaſy to know, whether it be ſufficiently pumped out of the receiver that was to be exhauſted; we thought it would be very convenient to have ſome inſtrument within the receiver, that might ſerve for a gage, or ſtandard, whereby to judge whether or no it were ſufficiently exhauſted.

To this purpoſe divers expedients were thought on, and ſome of them put in practice; which, though not equally commodious, may yet all of them be uſefully imployed, one on this occaſion, and another on that.

The firſt (if I miſremember not) that I propoſed, was a bladder, (which may be greater or leſs, according to the ſize of the veſſel it is to ſerve for) to be very ſtrongly tied at the neck, after having had only ſo much air left in the folds of it, as may ſerve to blow up the bladder to its full dimenſions, when the receiver is very well exhauſted, and not before. But though your lordſhip will hereafter find, that I yet make uſe of ſmall bladders on certain occaſions, in which they are peculiarly convenient, yet in many caſes they do, when the glaſſes are well exhauſted, take up too much room in them, and hinder the objects, included in the receiver, from being obſerved from all the ſides of it.

Another ſort of gage was made with quick-ſilver, poured into a very ſhort pipe, which was afterwards inverted into a little glaſs of ſtagnant quick-ſilver, according to the manner of the Torricellian experiment. For this pipe being but a very few inches long, the mercury in it would not begin to deſcend, till a very great proportion of air was pumped out of the receiver; becauſe till then the ſpring of the remaining air would be ſtrong enough to be able to keep up ſo ſhort a cylinder of mercury. And this kind of gage is no bad one. But becauſe, to omit ſome other little inconveniences, it cannot eaſily be ſuſpended, (which in divers experiments 'tis fit the gage ſhould be) and the mercury in it is apt to be too much ſhaken by the motion of the engine, there was another kind of gage by ſome ingenious man (whoever he were) ſubſtituted in its place, conſiſting of a kind of ſiphon, whoſe ſhorter leg hath belonging to it a large bubble of glaſs, moſt commonly made uſe of at an illuſtrious meeting of virtuoſi; where your lordſhip having ſeen it, I ſhall not need to deſcribe it more particularly.

But none of the gages I had formerly uſed, nor even this laſt, having the conveniences, that ſome of my experiments require; I was fain to deviſe another, which is that I moſt make uſe of, as having advantages, ſome or other of which each of the gages already mentioned wants; for even that with ſpirit of wine, not to mention leſſer diſadvantages, hath a bubble too great to let it be uſeful in veſſels ſo ſlender, as for ſome purpoſes I divers times employ; and this ſhort cylinder of ſo light a liquor as ſpirit of wine makes the ſubſidence of the liquor be indeed a good ſign, that the receiver is well exhauſted, but gives us not an account what quantity of air may be in the receiver, 'till it be arrived at that great meaſure of rarefaction; and the ſame liquor, being upon a very ſmall leak (ſuch as would not be prejudicial to many experiments) impelled up to the top of the gage, we cannot afterwards by this inſtrument take any meaſure of the air, that gets in at the leak. But now there are divers experiments, where I deſire to ſee the phænomena that will happen, not only (or perhaps not at all) upon the uttermoſt exhauſtion of the air, but when the preſſure of it is withdrawn to ſuch or ſuch a meaſure, and alſo when the air is gradually readmitted.

See Plate III. Fig. 4.

To make the gage we are ſpeaking of, take a very ſlender and cylindrical pipe of glaſs, of ſix, eight, ten, or more inches in length, and not ſo big as a gooſe-quill (but ſuch as we employ for the ſtems of ſealed weather-glaſſes;) and having at the flame of a lamp melted it, but not too near the middle, to make of it by bending it a ſiphon, whoſe two legs are to be not only parallel to one another, but as little diſtant any where from one another as conveniently may be. In one (which is uſually the longer) of theſe legs, there is to be left at the top, either half an inch, or a whole inch, or more or leſs than either (according to the length of the gage, or the ſcope of the experimenter) of air in its natural ſtate, neither rarified, nor condenſed; the reſt of the longer leg, and as great a part of the ſhorter as ſhall be thought fit, being to be filled with quick-ſilver. This done, there may be marks placed at the outſide of the longer, or ſealed, leg, whereby to meaſure the expanſion of the air included in the ſame leg; and theſe marks may be either little glaſs knubs, about the bigneſs of pins heads, faſtened by the help of a lamp at certain diſtances to the longer leg of the ſiphon, or elſe the diviſions of an inch made on a liſt of paper, and paſted on either to the ſiphon it ſelf, or to the ſlender frame, which on ſome occaſions we faſten the gage to.

THIS inſtrument being conveyed into a receiver (which for expedition ſake we chuſe as ſmall as will ſerve the turn) the air is to be very diligently pumped out, and then notice is to be taken, to what part of the gage the mercury is depreſſed, that we may know, when we ſhall afterwards ſee the mercury driven ſo far, that the receiver, the gage is placed in, is well exhauſted. And if it be much deſired to know more accurately (for one may arrive pretty near the truth by gueſs) what ſtations of the mercury in the gage are anſwerable to the degrees of the rarefaction of the air in the receiver; that may be compaſſed either by calculation (which is not ſo eaſy, and ſuppoſes ſome hypotheſes) or, though not without ſome trouble, by letting in the water as often as is neceſſary, into a receiver, whoſe intire capacity is firſt meaſured, and in which there may be marks made to ſhew, when the water to be let in ſhall fill a fourth part, or half, or three quarters, &c. of the cavity. For if (for inſtance) when the quick-ſilver in the gage is depreſſed to ſuch a mark, you let in the water, and that liquor appears to fill a fourth part of the receiver, you may conclude, that about a fourth part of the air was pump'd out, or that a fourth part of the ſpring, that the whole included air had, was loſt by the exhauſtion, when the quick-ſilver in the gage was at the mark above mentioned. And if the admitted water do conſiderably either fall ſhort of, or exceed the quantity you expected, you may the next time let in the water either after the mercury has a little paſſed the former mark, or a little before it is arrived at it. And when once you have this way obtained one pretty long and accurate gage, you will not need to take ſo much pains to make others, ſince you may divide them by the help of that one; for this being placed with any other in a ſmall receiver, when the mercury in the ſtandard-gage (if I may ſo call it) is depreſſed to any of the determinate diviſions obtained by obſervation, you may thence conclude, how much the air in the receiver is rarified, and conſequently by taking notice of the place, where the mercury reſts in the other gage, you may determine what degree of exhauſtion in a receiver is denoted by that ſtation of the mercury in this gage.

PERHAPS I need not tell your lordſhip, that the ground of this contrivance was, that whereas in divers other gages, when the pump came to be obſtinately plied, the expanſion of the included air would be ſo great, that it would either drive out the liquor, eſpecially if it were light, or in part make an eſcape through it; I judged, that in ſuch an inſtrument, as that newly deſcribed, thoſe inconveniencies would be avoided, becauſe that the more the air ſhould come to be dilated, the greater weight of quick-ſilver it would in the ſhorter leg have to raiſe, which would ſufficiently hinder it from making that heavy liquor run over; and the ſame ponderouſneſs of the liquor, together with the ſlenderneſs of the pipe, would likewiſe hinder the included air from getting through in bubbles.

N.B. 1. FOR moſt experiments, where exact meaſures are not required, it will not be ſo neceſſary to mark the gage at any other ſtation of the quick-ſilver than that, which it is brought to by the exhauſtion of the receiver; for by that alone we may know, when the air is well pumped out of the receiver, wherein the gage is included: and when one is a little uſed to ſome particular gage, one may by the ſubſidence of the mercury gueſs at the degree of the air's rarefaction, ſo near as may ſerve the turn in ſuch experiments. But when this inſtrument is to be uſed about nice trials, where it may be thought requiſite to have it divided according to one of the ways formerly propoſed, it will on divers occaſions be more ſecure (in caſe the maker of the gage has ſkill to do it) to put to the diviſions rather by little knubs of glaſs, than by paper; becauſe this will on ſuch occaſions be in danger either to be rubbed off, or wetted. And if glaſs-marks be uſed, it will be convenient, that every fifth, or tenth, or ſuch ordinal number as ſhall be judged fit, be made of glaſs of a differing colour, for diſtinction ſake, and the more eaſy reckoning. We ſometimes for a need apply, inſtead of theſe glaſs-knubs, little marks of hard ſealing wax, which will not be injured by moiſture, as thoſe papers will, that are paſted on: but theſe of wax, though in many caſes uſeful, are not comparable to the other in all; ſince if they be very ſmall, they are eaſily rubbed off, and if large, they make not the diviſion exact enough, and often hide the true place of the quick-ſilver.

2. I ſhall here about the mercurial gages add only this hint, that what I propoſed to myſelf in that contrivance was not only to eſtimate the air pumped out of the receiver, or that remaining in it; but alſo, by the help of this inſtrument (as elſewhere by another experiment) to meaſure (ſomewhat near) the ſtrength of the ſpring of rarefied air, according to its ſeveral degrees of rarefaction; and by this obſervation, in concurrence with other things, I hoped we might (according to what I have elſewhere inſinuated) be aſſiſted to eſtimate, by the cylinder of mercury raiſed in the open leg, the expanſion of the air included in the ſealed leg: but of theſe things I deſigned in this place to give but an intimation.

3. THAT leg of the gage, that includes the air, may be ſealed up either at the beginning, before the pipe be bent into a ſiphon, or (which is much better) after the following manner. Before you bend the pipe, draw out the end of it, which you mean to ſeal, to a ſhort and very ſlender thread; then having made the pipe a ſiphon, pour into the leg, which is to remain open, as much quick-ſilver as you ſhall judge convenient, which will riſe to an equal height in the other leg; out of which by gently inclining the ſiphon, you may pour out the ſuperfluous mercury, (if there be any,) and when you ſee, that there is an inch, or half an inch (or what part you deſigned to leave for air) unfilled with mercury, next to the end that is to be cloſed; and that the reſt of that leg, and as much (as you think fit)

ſit) of the other is full of quick-ſilver, you may, by keeping the ſiphon in the ſame poſture, and warily applying the ſlender apex above-mentioned to the upper part of the flame of a lamp, blown horizontal, eaſily ſeal up that apex without cracking, or prejudicing the open leg, or conſiderably injuring the air hole, that was to be ſealed up in the other. And this ſealing of one leg muſt (as it is evident) keep the mercury ſuſpended in it, though it be higher by divers inches than that in the open leg, till the withdrawing of the external air enable the included, by expanding itſelf, to depreſs the mercury in the ſealed leg, and raiſe it in the open.

4. HOW the length of theſe mercurial gages is to be varied, according to the bigneſs and ſhape of the ſlender receivers they are to be employed in; and how they may eaſily be made either to ſtand upright at the bottom of the receiver, or be kept hanging in the middle, or near the top of it (as occaſion may require;) and how the open end may be made to ſecure the mercury, in caſes where that is needful, belongs not ſo properly to this treatiſe, as to the ſecond part of the Continuation; where, if ever I trouble your lordſhip with it, the uſefulneſs of this ſort of gages, and the circumſtances, that may advantage them, will beſt appear.

5. THERE being ſome experiments, wherein it is not deſired, that the receiver ſhould be near exhauſted, but rather that the degrees of the air's rarefaction, which ought not to be very great, ſhould be well meaſured; we may in ſuch caſes make uſe of gages ſhaped like thoſe hitherto deſcribed, but made as long as the receiver will well admit, and furniſhed inſtead of quick-ſilver either with ſpirit of wine coloured with cocheneal, or elſe with the tincture of red roſe-leaves, drawn only with common water, made ſharp by a little either of the oil, or ſpirit of vitriol, or of common ſalt. For the lightneſs of theſe liquors in compariſon of quick-ſilver will allow the expanſions of the air included in the gage to be very manifeſt, and notable enough, though not half, or perhaps a quarter of the air be pumped out of the receiver.

6. YOU may alſo in ſuch caſes as theſe, where the receiver is large enough, and is not to be quite exhauſted, make uſe of a mercurial gage, differing from thoſe above deſcribed only in this, that the ſhorter leg need not to be above an inch, or half an inch long, before it expand itſelf into a bubble of about half an inch, or an inch in diameter, and having at the upper part a very ſhort and ſlender unſealed pipe, at which the air may get in and out: by which contrivance you may have this convenience, that you need not include ſo much air, as otherwiſe would be requiſite, at the top of the longer leg, becauſe the mercury in the ſhorter cannot, by reaſon of the breadth of the bubble, whereinto the expanſion of the air drives it, be conſiderably raiſed: upon which account it becomes more eaſy to eſtimate by the eye the degrees of the included air's rarefaction, which may be done almoſt as eaſily, as if there were water inſtead of mercury, provided it be remembered, that quick-ſilver, by reaſon of its ponderouſneſs, does far more aſſiſt the dilatation of the air, than ſo much water would do.

EXPERIMENT XVIII.

About an eaſy way to make the preſſure of the air ſenſible to the touch of thoſe that doubt of it.

THOUGH ſeveral of our experiments ſufficiently manifeſt to the ſkilful, that the preſſure of the air is very conſiderable; yet becauſe ſome of them require peculiar glaſſes, and other inſtruments, which are not always at hand, and becauſe there are many that think it ſurer to eſtimate the force of preſſure by what they immediately feel, than by any other way; I was invited for the ſake of ſuch to employ an eaſy experiment, which uſually proved convincing, becauſe it operated on that ſenſe, whereon they chiefly relied.

I cauſed then to be made a hollow (but ſtrong) piece of braſs, not above two or three inches high, (that it might be in a trice exhauſted,) and open at both ends; whoſe orifices were circular and parallel, but not equal, (the inſtrument being made tapering, ſo that it might be repreſented by an excavated *conus truncatus*, or a gigg, with the lower part cut tranſverſly off.) This piece of braſs being cemented on, as if it were a ſmall receiver to the engine, the perſon, that would not believe the preſſure of the air to be near ſo conſiderable as was repreſented, was bidden to lay the palm of his hand upon the upper orifice; and being ordered to lean a little upon it, that ſo the lower part of his hand might prove a cloſe cover to the orifice, one exſuction of the air was made by the help of the pump: and then upon the withdrawing of the greateſt part of the preſſure of the internal air, that before counterballanced that of the external, the hand being left alone to ſupport the weight of the ambient air, would be preſſed inwards ſo forceably, that though the ſtronger ſort of men were able (though not without much ado) to take off their hands, yet the weaker ſort of triers could not do it, eſpecially if by a ſecond ſuck the little receiver were better exhauſted, but were fain to ſtay for the return of the air into the receiver to aſſiſt them.

THIS experiment being deſigned rather to convince than to puniſh thoſe that were to make it, we took care, not only that the braſs ſhould be ſo thick, and the orifices ſo ſmooth, that no ſharpneſs nor roughneſs of the metal ſhould offend the hand; but alſo that the narrower orifice (which was the oftneſt made uſe of) ſhould be but about an inch and a quarter in diameter. But if any were deſirous of a more ſenſible conviction, it was very eaſy to give it him by making the larger orifice the uppermoſt, which was the reaſon why the inſtrument was, as we formerly noted, made tapering. But yet this larger orifice ought not to exceed two inches, or two inches and a half in wideneſs, leaſt the great weight of the air endanger the breaking or conſiderably hurting the hand of the experimenter. Which caution I am put in mind of giving, by remembering that I once much endangered my own hand, through the miſtake of him that managed the pump, who unawares to me ſet it on work, when, for another purpoſe, I had laid my hand upon the orifice of an inſtrument of too great a diameter.

The famous experiment of Torricellius, *mentioned in the 17th of our already publiſhed trials, is of that nobleneſs and importance, that though divers learned men have (but upon very differing principles) diſcourſed of it in print, which gives me the leſs mind to inſiſt long upon it here, yet I ſhall not ſcruple to ſubjoin ſome notes concerning trials that I made, (though for want of opportunity I could not repeat them according to my cuſtom,) which I had not met with in others, and which may ſerve to confirm the hypotheſis made uſe of in this Continuation, and the treatiſe it belongs to.*

EXPERIMENT XIX.

About the ſubſidence of mercury in the tube of the Torricellian experiment to the level of the ſtagnant mercury.

A BAROSCOPE being included in a receiver, made of a long bolt head with the lower part of the ball cut circularly off, upon the firſt exſuction of the air, the quick-ſilver that before ſtood at 29 inches, (the atmoſphere appearing then by a conſtant baroſcope very light,) would fall ſo low as to reſt at 9 or 10 inches, (for once I meaſured the ſubſidence beneath its former elevation,) and in about three ſucks more it would be brought quite down to the level of the ſtagnant quick-ſilver, and ſomewhat below, (as it is the property of quick-ſilver, quite contrary to water, to riſe leſs in a ſlender pipe than in a wide.) The air being let into the receiver, the quick-ſilver would be impelled up ſlowlier or faſter, as we pleaſed, to the former height of 29 inches, or thereabouts.

N.B. 1. THAT if the air were ſuffered to go haſtily out of the receiver, the mercury would, by virtue of the accelerated motion acquired in its deſcent, at the very firſt ſuck deſcend, till it reached within an inch or two of the ſtagnant mercury, though it would preſently after a few riſings and fallings ſettle at the height of 9 or 10 inches, till the next ſuck brought it down lower.

2. IF when the mercury was reimpelled up to its due height, thoſe that managed the pump did, inſtead of rarefying the air, a little compreſs it, the quick-ſilver would by the compreſſed air be eaſily made to riſe an inch or more above the former ſtandard of 29 inches. Which circumſtance I mention, not as a new thing, but to confirm (what ſome think ſtrange) a paſſage printed, in *New Experiments*, Exper. XVII. where I mention, that if the air in the receiver, inſtead of being rareſyed in the engine, were a little compreſſed by it, the preſſure of the included air, being ſomewhat increaſed by having its ſpring thus bent, would ſuſtain the mercury in the Torricellian tube at a greater than the wonted height.

AND to confirm another paſſage in the ſame page, where I obſerved, that if the preſſure of the air upon the ſtagnant mercury be not ſo great as it is wont to be, the mercury will begin to ſubſide in a (filled and inverted) tube, which wants of the uſual height; we took a glaſs cane, (ſealed at one end,) much ſhorter than the due length, and having filled it with mercury, and inverted it into a glaſs full of ſtagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requiſite height, remained totally ſuſpended, but upon the firſt or ſecond ſuck it would ſubſide, and in two or three ſucks more it would fall to the level of the ſtagnant mercury, or a little below it. Upon the letting in of the air it would be impelled to the very top of the tube, bating an aerial bubble, which ſeemed to come from the mercury itſelf, and was ſo little, as not to be at all diſcernable, ſave to a very attentive eye.

Experiment XVII. THIS experiment I ſhould not think fit here to relate, ſince I formerly acquainted your lordſhip with the ſubſidence of the mercury upon the withdrawing of the air from the receiver; were it not, that, in the mention of that trial, I remember I confeſſed to you, that I could not ſo free the great receiver I then uſed from air, but that the little, that remained or leaked in, made me unable to bring the mercury in the tube totally to ſubſide, or fall much nearer than within an inch of the ſurface of the ſtagnant mercury, with which in our preſent trials that in the tube was brought to a level.

EXPERIMENT XX.

Shewing that in tubes open at both ends, when no fuga vacui *can be pretended, the weight of water will raiſe quick-ſilver no higher in ſlender than in large pipes.*

BECAUSE I find it, even by learned and very late writers, urged as a clear and cogent argument againſt thoſe, that aſcribe the phænomena of the Torricellian experiment to the weight of the external air; that it is impoſſible, that the air, though it were granted to be a heavy body, could ſuſtain the quick-ſilver at the ſame height in tubes of very differing bigneſs, ſince the ſame air cannot equally counterpoiſe mercurial cylinders of ſuch unequal weights: and becauſe this objection is wont very much to puzzle thoſe, that are not well acquainted with the hydroſtaticks, I preſume your lordſhip will allow me, till I can ſhew you ſome hydroſtatical papers, by which the objection may appear to be but ill grounded upon the true theorems of that art, to annex the tranſcripts of a couple of experiments, (that I once made to remove this, ſuppoſedly inſuperable, difficulty,) juſt as I find them regiſtered in my note-books.

The FIRST TRYAL, Sept 2, 1662.

WE took a very large glaſs-tube, hermetically ſealed at one end, and about two foot and a half in length. Into this we poured quick-ſilver to the height of three or four fingers. Then we took a couple of cylindrical pipes of very unequal ſizes, the wider being as big again as the ſlenderer, and open at both ends. The lower ends of theſe two pipes we thruſt into the quick-ſilver, and faſtened them near their upper ends to the tube with ſtrings, that they might not be lifted up, nor moved out of their poſture, in which the convex ſurface of the mercury in both the pipes ſeemed to lie almoſt in a level, the tube alſo itſelf being placed upright in a frame. This done, by the help of a funnel we poured in water by degrees at the top of the tube, and obſerved, that as the water gravitated more and more upon the ſtagnant mercury, ſo the included mercury roſe equally in both the pipes, until the tube being almoſt filled with water, the mercury appeared to be impelled up to, and ſuſtained at as great a height in the big tube, as in the leſſer, being in either raiſed about two inches above the ſurface of the ſtagnant quick-ſilver.

N. B. 1. HAVING cauſed about half the water (having no conveniency to withdraw any more) in the tube to be ſucked out at the top, we obſerved the quick-ſilver in both the tubes to ſubſide uniformly, and to re-aſcend alike upon the re-affuſion of the water.

2. WE endeavoured to try the experiment (for their ſake, who have not the conveniency to have ſuch tubes purpoſely made) in a wooden veſſel, into which, when it was filled with water, we let down a flat glaſs furniſhed with ſtagnant mercury, whereinto the ends of the two pipes were immerſed. But the opacouſneſs of the cylinder (which reduced us to ſee only from the top the reflection of the ſtagnant mercury,) and other impediments, diſabled us to perceive the motions and ſtations of the mercury in the pipes, though we once made uſe of a candle the better to diſcern them.

The SECOND TRYAL.

WE took a very wide tube of glaſs, of about a foot long, and into it poured a convenient quantity of quick-ſilver. We took alſo two pipes of about equal length, and of that diſparity in bigneſs, that we newly mentioned, (thoſe pipes lately deſcribed, being indeed cut off from theſe we are now to ſpeak of,) and theſe being filled with quick-ſilver, after the manner of the Torricellian experiment, were by a certain contrivance let down into the tube, and unſtopped under the ſurface of the ſtagnant mercury, and then the quick-ſilver in the pipes falling down to its wonted ſtation, and reſting there, we poured into the tube about a foot height (by gueſs) of water, whereupon the quick-ſilver, as it before ſtood, as it were, in a level in both the pipes, ſo it was, for ought appeared to us, equally impelled up beyond its wonted ſtation, and ſuſtained there, both in the ſlender and in the bigger pipe, and upon the withdrawing of ſome of the water, it began to ſubſide alike, as to ſenſe, in them both, falling no lower in the bigger than in the ſlenderer. And water being a ſecond time poured down into the tube, the mercury did in both pipes riſe uniformly as before. By which, and the former experiment, it ſufficiently appeared, that a gravitating liquor, as air or water, may impel, or keep up mercury to the ſame height in tubes, that are of very differing capacities; and that liquors ballance each other according to their altitude, and not barely according to their weight. For in this laſt experiment, the additional cylinder of one inch of mercury, was manifeſtly raiſed and kept up, by the water incumbent on the ſtagnant mercury, the other cauſe, whatever it were, of the mercury's ſuſpenſion, being able to ſuſtain but a cylinder ſhorter by an inch. And the ſame parcel of water did counterpoiſe in the differing pipes two mercurial cylinders, which though but of the ſame altitude, (namely about an inch) were of very unequal weight.

EXPERIMENT XXI.

Of the heights, at which pure mercury, and mercury amalgamed with tin, will ſtand in barometers.

CONSIDERING with myſelf, that if the ſuſtentation of the quick-ſilver in the Torricellian experiment at a certain height depends upon the æquilibrium, which a liquor of that ſpecifick gravity does at ſuch a height attain to with the external air; if that peculiar and determinate gravity of the quick-ſilver be altered, the height of it, requiſite to an æquilibrium with the atmoſphere, muſt be altered too: conſidering this, I ſay, I thought it might ſomewhat confirm the hypotheſis hitherto made uſe of, if a phænomenon ſo agreeable to it were actually exhibited. This I ſuppoſed performable two differing ways, namely by mixing, or as chymiſts ſpeak, amalgamating mercury either with gold, to make it a mixture more heavy, or with ſome other metal, that might make it more light than mercury alone is. But the former of thoſe two ways I forbore to proſecute, being where I then was unfurniſhed with a ſufficient quantity of refined gold, for that which is coined is generally allayed with ſilver, or copper, or both; and therefore amalgamating mercury with a convenient proportion of pure tin, or, as the tradeſmen call it, block-tin, that the mixture might not be too thick to be readily poured out into a glaſs-tube, and to ſubſide in it, we filled with this amalgam a cylindrical pipe, ſealed at one end, and of a fit length, and then inverted it into a little glaſs furniſhed with the like mixture. Of which tryal the event was, that the amalgam did not fall down to 29, nor even to 30 inches, but ſtopped at 31 above the ſurface of the ſtagnant mixture.

N. B. 1. THAT though one may expect, that the event of the experiment would be the more conſiderable, the greater the quantity is, that is mingled of the light metal, yet care muſt be taken, that the amalgam be not made too thick, leſt part of it ſtick here and there (as we did to our trouble find it apt to do) to the inſide of the pipe, by which means ſome aerial corpuſcles will meet with ſuch convenient receptacles, as to make it very difficult, if not almoſt impoſſible, to free the tube quite from air.

2. It may perhaps be worth while to try, whether by comparing the height of the amalgam, to what it ought to be upon the ſcore of the ſpecifick gravities of the mercury, and the tin, mingled in a known proportion in the amalgam, any diſcovery may be made, whether thoſe two metals do penetrate one another after ſuch a manner (for there is no ſtrict penetration of dimenſions among bodies) as copper and tin have, as I elſewhere note, been by ſome chemiſts obſerved to do, when being melted down together, they make up a more cloſe and ſpecifically ponderous body, than their reſpective weights ſeemed to require.

3. That by comparing this 21ſt experiment with the 18th of thoſe formerly publiſhed, it may appear, that the height of the liquor, ſuſpended in the Torricellian experiment, depends ſo much upon its æquilibrium with the outward air, that it may be varied by a change of gravity in either of the two bodies that counterballance each other, whether the change be of weight in the atmoſphere, or of ſpecifick gravity in the ſuſpended liquor.

ADVERTISEMENT.

I Should here acquaint your lordſhip with what I have ſince tried, in reference to the 18th of the printed experiments, where I mention, that I obſerved, by long keeping the ſame inſtrument, with which I once made the Torricellian experiment, in the ſame place, that the height of the ſuſpended mercury would vary according as the weight of the atmoſphere happened to change. But though about the barometer (as others have by their imitation, allowed me to call the inſtrument hitherto mentioned, put into a frame) I made in the year 1660 ſeveral obſervations, that would not perhaps be impertinent in this place, yet having long ſince left them with a friend, who lives far off, and not having them now in my power, I muſt beg your lordſhip's permiſſion, to reſerve them for a part of the appendix, which I doubt I ſhall be engaged to add to this epiſtle. And in the mean time, I ſhall not forbear to preſent your lordſhip thoſe other papers, that I have by me, relating to the barometer; ſome of which will, I preſume, ſufficiently confirm my lately mentioned conjecture, about the cauſe of the variation obſerved in the height of the ſuſpended mercury.

EXPERIMENT XXII.

Wherein is propoſed a way of making barometers, that may be tranſported even to diſtant countries.

THINKING it a deſireable thing (as I have elſewhere intimated) to be able to compare together, by the help of barometers, the weight of the atmoſphere at the ſame time, not only in differing parts of the ſame country, as of *England*, but in differing regions of the world; I could not but foreſee, that it would be very difficult to accompliſh my deſire without altering the form of the barometers I had hitherto made uſe of. For as theſe be unfit to be tranſported far, becauſe that ſtagnant mercury would be ſo apt to ſpill; ſo the procuring them to be made in the places, where they are to be uſed, though it be no bad expedient, and ſuch as I have divers times made uſe of, is liable to this inconvenience; that, beſides that few will take the pains, and have the ſkill, requiſite to make baroſcopes well, though they be ſufficiently furniſhed with glaſſes and mercury for that purpoſe; beſides this, I ſay, except men be more than ordinarily diligent and ſkilful, (and perhaps though they be) it will be very difficult to be ſure, that the baroſcope newly made in a remote country is as good (and but as good) as that, which a man makes uſe of in this; in regard that at the making of the former, they are ſuppoſed to have no other baroſcope to compare it with; and to be ſure, they have not the ſame, with which it is to be compared here.

Being by theſe conſiderations invited to attempt the making of portable or travelling baroſcopes (if I may ſo call them) I thought it requiſite to endeavour theſe three things: the firſt, to make the veſſel, that ſhould contain both the ſuſtained and the ſtagnant mercury, all of one piece of glaſs, of a like bigneſs: the next, to place this veſſel, when filled, in ſuch a frame, as may be eaſy to be tranſported, and yet in a reaſonable meaſure defend the glaſs from external violence, no part of it ſtanding quite out of the frame, as in all other baroſcopes: and the third, ſo to order the veſſel, that it may not be ſubject to be eaſily broken by the violent motion of the mercury contained in it.

The firſt of theſe will not ſeem practicable to thoſe, that imagine (without any warrant from the hydroſtaticks,) that it is as well neceſſary as uſual, that the ſtagnant mercury ſhould have a veſſel much wider than the tube, wherein the mercurial cylinder is ſuſtained; but to us the difficulty ſeemed much leſs to make the glaſs-part of our tube of one piece, and of a convenient ſhape, than afterwards to fill it.

But to do both, we took a glaſs cylinder, ſealed at one end, and of a convenient length (as about four or five foot) and cauſed it by the flame of a lamp to be ſo bent, that, to thoſe, that did not take notice it was ſealed at one end, it ſeemed to be a ſiphon of very unequal legs, the one being three or four times longer than the other; by virtue of which figure the ſhorter leg may ſerve inſtead of the diſtinct veſſel uſually employed to contain the ſtagnant mercury. To fill this, which is not eaſy, one may proceed after this manner. Take a ſmall funnel of glaſs, with a long and ſlender ſhank, ſo that it may reach three or four inches, or further, into the ſhorter leg of our barometrical ſiphon (if I may ſo call it;) and by this funnel pour into this ſhorter leg as much mercury as may reach about two or three inches in both legs; then ſtopping the orifice with your finger, and ſlowly inclining the tube, the mercury in the longer leg will gently fall to the ſealed end; and the air, that was there before, will paſs by it, and ſo make it room. The mercury in the ſhorter leg (which leg ought to be held uppermoſt) will by the ſame inclination of the tube fall towards the orifice; but, being by the finger that ſtops that, kept from falling out, if you do ſlowly re-erect the glaſs, and then make it ſtoop again as much as before, the mercury will paſs out of the ſhorter leg into the longer, and join with that which was there before; and if all the mercury do not ſo paſs, the orifice is to be ſtopped again with your finger, and the tube inclined as formerly. This done, the tube is to be erected, and by the help of the funnel more mercury is to be poured in, and the foregoing proceſs of ſtopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury pour'd into the ſhorter leg be brought to join with that in the longer; and then the open leg is to be furniſhed with freſh mercury, obſerving this, that the nearer the longer leg comes to the being filled, the leſs you muſt raiſe it from time to time, when you pour mercury into the ſhorter; as alſo, that when you ſee the longer leg quite full of mercury (though there be but little in the ſhorter) you need not pour in any more, if the longer do much exceed a yard; becauſe upon the reſtoring of the tube to an erected poſture, there will ſubſide from the taller leg into the other a pretty quantity of mercury, by reaſon of the ſpace at the ſealed end, which will be deſerted by the mercury that was there. But becauſe it is difficult by this way, as well as by that practiced already, to fill a tube with mercury without leaving any viſible bubbles; to free it from ſuch (if any happen to be) you muſt once more ſtop the orifice with your finger, and incline, and re-erect the tube divers times, till you have thereby brought moſt of the ſmaller bubbles into one greater; (which you may if you pleaſe increaſe, by letting in a little air:) for by making this great bubble paſs leiſurely two or three times from one end of the tube to the other, it will in its paſſage as it were lick up all the ſmall bubbles, and unite them to itſelf; which may afterwards by one inclination more of the tube be made to paſs into the ſhorter leg, and thence into the free air.

But there is another ſort of funnels, which if one have the ſkill and conveniency to make (as *I. M.* eaſily doth) one may very expeditiouſly fill the bended tubes of our portable barometers. For if you make the ſlender part of the funnel not ſtreight but bended, in the form of an obtuſe angle, and of ſuch a length, that the part, which is to go into the ſhorter leg of our ſiphon, may reach to the flexure (of the ſiphon;) then you may, by ſo holding the tube, that the ſealed end be ſomewhat lower than the other, and by pouring in mercury at the obtuſe end of the angular funnel, eaſily make it run over the flexure into the longer leg of the ſiphon; provided you do now and then, as occaſion requires, erect a little and ſhake the tube, to help the mercury to get by the air, and expel it.

By ſuch ways as theſe we have found by experience, that it is poſſible (though not eaſy) to do in ſuch a bended glaſs, as our purpoſe requires, what, beſides a very late learned writer, the diligent *Merſennus* himſelf, admoniſhes his reader, that it is not a practicable thing to do in the ordinary glaſſes of the Torricellian experiment, viz. to free the mercury of a ſtreight tube from air and bubbles, ſo as to be able by inclining the glaſs to make the liquor aſcend to the very top.

The firſt of our three above-mentioned ſcopes being thus attained, it was not difficult to compaſs the ſecond, by the help of a ſolid piece of wood, which is to be ſomewhat longer than the tube, and a good deal broader in the lower part than in the upper, that it may receive the ſhorter leg of the ſiphon. In ſuch a piece of wood, which was about an inch thick, we cauſed to be made a gutter or channel, of ſuch a depth and ſhape, that our ſiphon might be placed in it ſo deep, that a flat piece of wood (like a plained lath) might be laid upon it, without at all preſſing upon, or ſo much as touching the glaſs; ſo that this piece of wood may ſerve for a cover to defend the glaſs, to be put on when the inſtrument is to be tranſported, and taken off again, when it is to be hung up to make obſervations with; the channel-piece of wood ſerving both for a part of a caſe, and for an entire frame; which may for ſome uſes be a little more commodious, if the cover be joined (as it may eaſily be) to the reſt of the frame, by two or three little hinges and a haſp, by whoſe help the caſe may be readily opened and ſhut at pleaſure.

The third thing we propoſed to our ſelves is nothing near ſo eaſy as the ſecond; nor have we yet had opportunity, to try, whether the way we made uſe of will hold, if the barometer be tranſported into very remote parts, though by ſmaller removes we found cauſe to hope that it will ſucceed in greater.

The grand difficulty to be obviated was this; that though it were eaſy to hinder the ſpilling of the mercury, by ſtopping the orifice of the ſhorter leg of our ſiphon, yet that would not ſerve the turn; for the upper part of the tube being deſtitute of air, if the mercury be by the motion of the inſtrument put to vibrate, it will be apt (for want of meeting with any air in the upper part of the tube to check its motions) to hit ſo violently againſt the top of the glaſs as to beat it out, or to crack ſome of the neighbouring parts.

To obviate this great inconvenience our way is, to incline the tube, till the mercury be impelled to the very top of it, and yet there will remain a competent quantity in the ſhorter leg of the glaſs, if that be not at firſt made too ſhort. This done, the remaining part of the ſhorter leg is to be quite filled up either with water or mercury, and the orifice of it is to be very carefully and firmly ſtopped, for which purpoſe we uſe our ſtrong black cement: for by this means the mercury in the longer leg, having no room to play, cannot ſtrike with violence as before, againſt the top of the glaſs. But though by many times ſucceſſively ſhaking the baroſcope we did not perceive, that it was very like to be prejudiced by the

the ſhakes it muſt neceſſarily endure in tranſportation to remote places, if due care be had of it by the way, yet till further trial have been made I ſhall not pretend to be certain of the event. But thus much of conveniency we have already found in this contrivance, that we ſent it ſome miles off, to the top of a hill, and had it brought home ſafe again, the phænomena at the top and bottom of the hill being anſwerable to what we might have expected, if we had employed another baroſcope.

See the whole baroſcope Plate V. Fig. 2.

When the inſtrument is to be ſent away, the height of the mercurial cylinder (to be meaſured from the ſurface of the ſtagnant mercury in the ſhorter leg) being taken for that place, day, and hour, and compared (if it may be) with that of another good baroſcope, which is to continue in that place; as much of the gutter as is unfilled by the glaſs may be well ſtuffed with cotton, or ſome ſuch thing, to keep the glaſs the more firm in its poſture; and that the tube be not ſhaken or preſſed againſt the wood, ſome of the ſame matter may be put between the reſt of the frame and the cover, which ought to be well bound together. And when the inſtrument is arrived at the remote place, where it is to be employed, (for if it be to be ſent but a little way, it may be carried ſafely without uſing any adventitious liquor) the water, that is added, may be taken off again, by ſoaking it up with pieces of ſpunge, linnen, &c. but if inſtead of water you put in mercury, as it ought to have been put in by weight, ſo it is to be taken out, till you have juſt the weight that was put in: and it is not difficult to take out the mercury by degrees, by the help of a ſmall glaſs-pipe, ſince you may either ſuck up little by little as much as remains of the additional mercury, when by erecting the barometer, and warily unſtopping the orifice of the lower leg, as much mercury as will of itſelf flow out is effluxed; or elſe you may take out the ſuperfluous mercury, by thruſting the lower end of the pipe into that liquor, and when it has taken in enough, ſtopping the upper end cloſe with your finger, to keep it from falling back again when you remove the pipe.

N.B. If it ſhould happen in a long voyage, that by the numerous ſhakings of the inſtrument, there ſhould from the additional water or mercury in the ſhorter leg get up into the longer any little aerial bubble, which ſeems the only, but I hope not likely, danger in this contrivance, he that is to uſe the inſtrument, at the end of the voyage may, if he be ſkilful, free the mercury from it by the ſame way, that we lately preſcribed to free it from air, when the inſtrument was firſt filled.

I preſume I need not tell your lordſhip, that the chief uſe of his travelling baroſcope is, that he that uſes it in a remote part, keepin ga diary of the heights of the mercury, by comparing theſe heights with thoſe, at which the mercury ſtood at the ſame times in the barometer that was not removed, the agreement or difference of the weight of the atmoſphere in diſtant places may be obſerved. To which this may be added, the conveniency, which the ſtructure of theſe inſtruments gives them to be ſecurely let down into wells or mines, and to be drawn up to the top of towers, and ſteeples, and other elevated places: not here to conſider, whether by a convenient addition, theſe, as well as ſome other barometers, may not be made to diſcover even very minute alterations of the atmoſphere's preſſure.

Whether this travelling baroſcope, being furniſhed at its upper end with a very good ball and ſocket, and at the lower end with a great weight (which way of keeping things ſteady in a ſhip has been happily uſed by the Royal Society on another occaſion,) whether, I ſay, our inſtrument may by this contrivance, or ſome other that might be ſuggeſted to the ſame purpoſe, be made any thing ſerviceable at ſea, notwithſtanding the differing motions of the ſhip, I have had no opportunity to try: but whether it may or may not be uſeful in ſpite of the rolling of the ſhip, it may at leaſt be made uſe of in flat calms (which divers times happen in long voyages, eſpecially to the *Eaſt Indies*, and to *Africk*) and then the inſtrument, which at other times may lie by without being at all cumberſome, may be made uſe of, as long as the calm laſts, to acquaint the obſerver with the weight of the atmoſphere in the climate where he is, and that upon the ſea: which may give ſome welcome information to the curioſity of ſpeculative naturaliſts, and perhaps prove either more directly, or in its conſequences, of ſome uſe to navigators themſelves, as by enabling them by its ſudden changes to foretel the end of the calm. Beſides that, having one of theſe inſtruments ready at hand, whereever they ſet foot on ſhore, though it be but upon a ſmall iſland, or a rock, they can preſently and eaſily take notice of the gravity of the atmoſphere in that place; which whether or no, if compared with other obſervations, it may in time prove not altogether uſeleſs to the gueſſing whereabouts they are, and the foreſeeing ſome approaching changes of weather, I leave to future experience, if it ſhall be thought worth the making, to determine.

Besides the ordinary baroſcope, and this travelling one, I have employed two or three other inſtruments of quite differing kinds, to diſcover the varying gravities of the atmoſphere; but though they have hitherto ſucceeded well, for the main, yet being willing to make further obſervations about them, I reſerve one of them for another opportunity, and think fit to leave the other in a tract it belongs to.

A Postscript Advertisement.

SINCE the writing of the foregoing and the following experiments about the travelling baroſcope, having had occaſion to make one at a place about fifty miles diſtant from that where I was when I writ them, I took notice, that the mercury in the travelling baroſcope was not by ¼ of an inch ſo high as that in another baroſcope made the ordinary way; and yet it was not eaſy to perceive, that the former had been leſs carefully filled than the latter. So that I yet know not well to what cauſe to impute the difference, unleſs it ſhould perhaps depend upon this circumſtance; that the pipe, whereof the travelling baroſcope was made, was very ſlender, and much more ſo, than the tube of the other; and I have already elſewhere obſerved, that mercury, contrary to what happens in water, is leſs apt to riſe in very ſlender pipes. And though I remember, that, at the place where I writ the experiment, to which this poſtſcript belongs, in the tube, I then employed to make the travelling baroſcope, the mercury aſcended as high as in a noted one made the common way; yet not being in the other place furniſhed with a tube, long and big enough, I think myſelf obliged, till I can clear the doubt by further trial, to give your lordſhip this advertiſement, leſt either the cauſe already ſuſpected, or ſome other unheeded thing, may in ſome caſes make theſe travelling baroſcopes ſomewhat differing from others. But though they ſhould prove to be ſo, yet it would not follow, that they cannot be made ſerviceable: for keeping a pretty while that inſtrument, which ſuggeſted the ſcruple to me, juſt by the other, with which I had compared it, and carefully taking notice of the reſpective heights, at which the mercury reſted in both; I obſerved, that when it roſe or fell in the other barometer, it did alſo riſe and fall in the portable one; and when it reſted at its firſt ſtation in the former, it did ſo in the latter; and though there ſeemed to be an inequality in the quantity of the aſcent, and ſubſidence of the mercury in the two inſtruments, yet that ſeemed to be accountable for, by ſome circumſtances, eſpecially the very unequal breadth of the veſſel, that contained the ſtagnant mercury in the other barometer, and that ſhorter leg, which anſwered to that veſſel in the travelling barometer. But till the formerly propoſed ſcruple be by further obſervations removed, the ſafeſt way will be to make the barometer to be ſent to remote places, as like as may be (in bigneſs, and length of the tube) to another portable one kept at home; that ſo, when they are once adjuſted, the collations may be made betwixt two inſtruments of the ſame kind, whereof that, which is kept at home, may alſo, if it be thought fit, be compared, when the obſervations are made, with a baroſcope, made the ordinary way.

EXPERIMENT XXIII.

Confirming, that mercury in a barometer will be kept ſuſpended higher at the top, than at the bottom of a hill. On which occaſion ſomething is noted about the height of mountains, eſpecially the pic of Tenariff.

TO give your lordſhip ſome inſtance (till I can preſent you with a noble one) of the uſe of our travelling barometer, I ſhall now add; that when I writ the foregoing experiment, chancing to be within two or three miles of a hill, which, though not high, was the leaſt low in that country, I thought our inſtrument might be ſafely, and not altogether uſeleſly, carried on horſe-back to the top of it, which was too remote from the bottom to be conveniently reached by me on foot in the midſt of winter. This trial therefore I reſolved to make, becauſe, though I formerly told you of a conſiderable one, that had been made in *France* by ſome eminent virtuoſo of that country; yet I was willing, not only to have a proof, how ſafely our baroſcope might be tranſported, but to confirm to your lordſhip upon our own obſervation, made in another region, ſo conſiderable an argument, as theſe kind of experiments afford to our hypotheſis.

AND though when I came to try the experiment, I happened to have an indiſpoſition, that forbid me to do it all myſelf; yet having carefully marked on the edge of the frame the height, to which the ſuſpended quick-ſilver reached, and compared it with a good baroſcope made the ordinary way, I committed our inſtrument to a couple of ſervants, that I had often employed about pneumatical and mercurial experiments, giving them particular inſtructions what to do. And the inſtrument being ſuch as might be ſafely carried on horſeback, I had in two or three hours an account brought me back, the ſum of which was; that they found the ſuſpended mercury fall a little as they aſcended the hill, at whoſe top they gave the liquor leave to ſettle, and carefully took notice, by a mark, of the place it reſted at; which was, as I afterwards found, ¼ of an inch, or ſomewhat better beneath the mark I had made; and this notwithſtanding the hill was not high, and the air and wind ſeemed to them to be much colder at the top of it, than beneath. But though, as they deſcended more and more, they obſerved the mercury to riſe again higher and higher, (as being preſſed againſt by a taller column of the atmoſphere,) and though conſequently the experiment agreed very well with our hypotheſis, and may ſerve for a confirmation of it; yet by reaſon of the ſmall height of the mountain, the decrement of the height of the mercurial cylinder was not not ſo conſiderable, but that I ſhould perhaps have omitted the mention of this trial, if it did not ſhew, that our travelling baroſcopes may be fit to be employed about ſuch experiments. And therefore, when I can recover ſome of my ſcattered papers, I ſhall by way of appendix ſubjoin to this ſome other obſervations, that I procured to be made by ingenious men, who had the opportunity of living near higher mountains.

SOME further trials I have recommended to be hereafter made by ſome other inquiſitive perſons; and to make them the more inſtructive, I could wiſh, that others would do what I ſhould have done, if opportunity had befriended me. For I deſigned to make the experiment at the bottom, the top, and the intermediate part of the whole, at three differing conſtitutions of air, viz. when it ſhould appear by a good ordinary baroſcope, that the atmoſphere was very heavy, when it ſhould be found to be very light, and when it ſhould have a moderate degree of gravity: and I hoped, that if ſagacious experiments ſhould make theſe diverſified obſervations on diſtant and unequal hills, good hints may reſult from the collations, that may be made of the varying decrements of the mercurial cylinder's height, according to the differing gravities of the atmoſphere at ſeveral times, and the differing heights of the hills and ſtations where the obſervations ſhould be made.

I alſo endeavoured to get a baroſcope carried down to the bottoms of deep mines; partly, to try whether the atmoſpherical pillar being longer there than at the top, the mercury in the tube would not be impelled up higher; and partly, in order to other diſcoveries. But ſome impediments in the ſtructure of thoſe mines made it not very practicable to employ barometers there; which yet makes me not deſpair of ſucceſs in ſome other mines, where the ſhafts or pits are ſunk more perpendicularly.

PERHAPS I told your lordſhip already by word of mouth, that I have been ſollicitouſly endeavouring to get the Torricellian experiment tried upon the pic of *Teneriff*; but hitherto I have had no account of the ſucceſs of my endeavours; for which I am the more concerned, becauſe of the eminent (if not matchleſs) height of that mountain, of which you may receive ſome ſatisfaction, by what I am going to ſubjoin about it.

An Appendix about the height of Mountains.

FORASMUCH as on the one hand, not only *Kepler*, but divers other modern writers of note, do endeavour to ſtraiten the atmoſphere, and make it lower by half than the leaſt height, to which, according to our eſtimation, it ſhould reach; and to countenance their opinion, will not allow the clouds to be often above a mile high, nor even the higheſt mountains to exceed two miles. And foraſmuch as, on the other ſide, other learned men ſeem to make the clouds and the mountains of a ſtupendous height; we, who take a middle way of eſtimating the height of the one and the other, hold it not unfit to ſubjoin on this occaſion ſome uncommon obſervations, in favour of our opinion, that we have obtained from inquiſitive travellers.

BUT firſt I will ſubjoin a paſſage I have ſome where met with in *Ricciolus*'s *Almigeſtum novum*, where he, if I well remember, relates, that the rector *Metenſis*, as he calls him, of the Jeſuits college affirmed to him ſome years ſince, that he had meaſured the height of many clouds, without having found any of them higher than 5000 paces: which argues, that he met with ſome ſo high, though indeed the height of clouds muſt needs be very various, according the gravity or lightneſs, denſity or thinneſs, reſt or agitation of the air, and the condition of the vapours and exhalations they conſiſt of. And if either that be true, which we have formerly had occaſion to mention concerning *Maignan*'s obſervation; or if it be true, that ſublunary comets, (for I ſpeak not of celeſtial ones) are generated of exhalations of the terreſtrial globe, we may well conjecture, that the atmoſphere, (eſpecially if its height be not uniform,) and even clouds, eſpecially thoſe that have moſt fumes, and feweſt vapours, may reach much higher than *Cardan*, *Kepler*, and others have defined.

BUT of the height of clouds, which we have ſometimes attempted to take geometrically, we may have elſewhere occaſion to ſpeak again; and therefore I ſhall now proceed to what I have to ſay concerning the height of mountains. Which being an enquiry curious and difficult enough in itſelf, and of ſome importance in the diſquiſition about the height of the atmoſphere, it being evident, that that muſt reach at leaſt as high as the tops of mountains, upon whoſe tops men can live; I hope it will not be unacceptable to your lordſhip, if having a while ſince, as I was intimating, had the opportunity to diſcourſe with ſome credible perſons, that have been upon the top of exceeding high mountains, particularly of the pic of *Teneriff*, and eſpecially with one gentleman, who was a few days before brought to ſatisfy the curioſity of our inquiſitive and diſcerning monarch, by giving him an account of his journey, I acquaint you with thoſe of the particulars, which I learned from thence, that are the moſt pertinent to our preſent purpoſe. Firſt then, whereas divers late mathematicians will not allow above two miles or half a german league, and ſome of them not half ſo much, to the height of the higheſt mountain; the mountain we ſpeak of, in the iſland of *Teneriff*, one of the Canaries or Fortunate Iſlands, is ſo high, that though perhaps I think thoſe travellers I have taken notice of ſpeak with the moſt when they write, that the top of this mountain is be ſeen at ſea, four degrees off, i. e. at leaſt threeſcore german leagues; yet having aſked the ingenious gentleman lately mentioned, Mr. *Sydenham*, from what diſtance the top of the ſugar-loaf, or higheſt part of the hill, ſo called from its figure, could be ſeen at ſea, according to the common opinion of ſeamen; he anſwered, that that diſtance was wont to be reckoned 60 ſea-leagues, of three miles to a league; adding, that he himſelf had ſeen it about 40 leagues off, and yet it appeared exceeding high, and like a bluiſh pyramid, manifeſtly a great deal higher than the clouds. And what he related to me about the diſtance was afterwards confirmed by the anſwers I received from obſerving men of differing nations, who had ſailed that way; and particularly by a noble virtuoſo, ſkilled in the mathematicks, who was then admiral of a brave Engliſh fleet. And the abovementioned gentleman Mr. *S.* alſo told me, that ſometimes men could from thence ſee the iſland of *Madera*, though diſtant from it 70 leagues; and that the great Canary, though 18 leagues off, ſeemed to be very near them that were on the top of the ſugar-loaf, as if they might leap down upon it. Thus far Mr. *Sydenham*, by whoſe relation it appears, that this pic muſt be far higher than *Kepler* and others allow mountains to be; for elſe it could not be ſeen at ſea, from ſo great a diſtance. And the learnd *Ricciolus*, ſuppoſing it to be, (as ſome navigators report it to be,) diſcoverable at ſea four degrees off, calculates its height meaſured by a perpendicular line, and allowing too for refraction, to amount to ten miles, which altitude alſo the accurate *Snellius* aſſigns it. But I fear this learned man may have been ſomewhat miſ-informed by the navigators he relies on, or elſe that the way of allowing for refractions is not yet reduced to a ſufficient certainty. For I do not find by thoſe, who have purpoſely gone to the top of it, that the mountain is ſo high as his calculation makes it. And whereas the ſame eminent writer reſolutely pronounces, that the height of mount *Caucaſus*, deduction being made for refraction, is 51 Bolognian miles, which are conſiderably greater than the Roman miles; I doubt not here likewiſe, though I queſtion not his ſupputations, if you grant him the grounds of them, he makes this mountain far higher than indeed it is. For the paſſage of *Ariſtotle*, on which he founds his opinion, is obſcure enough; and *Ariſtotle* himſelf does ſometimes take up reports upon hearſay, without over-ſtrictly examining their truth or probability; whereas all the navigators and travellers I have hitherto met with, (and your lordſhip knows, that I have, upon a publick account, the opportunity of meeting often with ſuch men,) do almoſt unanimouſly agree, that the pic of *Teneriff* is the higheſt mountain hitherto known in the world; and yet that is ſo far from being 15 leagues high, as ſome eminent, and even late writers would perſwade us, that it is ſcarce a ſeventh part ſo high as *Ricciolus* computes mount *Caucaſus* to be. For having aſked Mr. *Sydenham*, and others, what was the eſtimate made by the moſt knowing perſons of the iſland of the height of the hill, he told me, that his guides accounted it to be one and twenty miles high from the town called *L'oretava*, ſeated on the lower part of the hill, from which town to the ſea there is three miles of way always deſcending. But in regard that the way, which amounted to 21 miles in length, is, as other ways, whereby ſteep places are wont to be aſcended, made to wind and turn for the conveniency of travellers; I can ſcarce deduct leſs than two thirds for the crookedneſs of the way: and accordingly having aſked him, whether the perpendicular height of it had been accurately taken by any with mathematical inſtruments, he anſwered, that he could ſay nothing to that upon his own knowledge, but that a ſeaman with great confidence affirmed himſelf to have accurately enough meaſured it by obſervations made in a ſhip, and to have found the perpendicular height of the hill to be about ſeven miles. Which eſtimate agrees well enough with the calculations of *Ricciolus* and *Snellius*, if we leſſen the diſtance, from which the top of the hill is to be diſcovered, from 60 German leagues of four miles to a league, to the like number of common leagues at three miles to a league.

AND becauſe eminent writers have ſo confidently delivered prodigious things touching the height of this mountain, I will here, to confirm the eſtimate already made, add theſe particulars, which I took from the gentleman's own mouth, and which were afterwards confirmed to me by another, that went with him; and partly alſo by a third, who went up to the top at another time of the year, viz. that they begun their journey from *L'oretava* on the 18th of *Auguſt*, about 10 of the clock at night, and travelled till five in the afternoon on the monday following, reſting two hours by the way, and travelling about 10 miles of their way upon mules, which afterwards they were forced to leave, and betake themſelves to their feet. Reſting upon monday till midnight, they reſumed their journeying, and travelled until about nine the next morning, at which time they arrived at the top of the Sugar-loaf, or higheſt pile of the mountain; ſo that they travelled in all but 26 hours, in which, conſidering the ſteepneſs and ruggedneſs of the ways, and that they were forced to go above half way on foot, to which they were unaccuſtomed, it is likely enough, that the length of the way did not much, if at all, exceed the computation of the guides.

WE have ſince endeavoured, but without yet knowing what will be the ſucceſs, to have the height of this mountain carefully taken by ſkilful men. In the interim I ſhall not deny, but that if what *Ariſtotle* and other authors report of mount *Caucaſus* be true, there may be far higher mountains than the pic of *Teneriff*; eſpecially ſince there is one conſideration, which perhaps you will not think deſpicable, that I find not taken notice of by thoſe, that have written of the height of mountains; viz. that of two mountains, that meaſured by geometrical inſtruments may appear to be of the ſame height, there may yet be a great inequality; becauſe the meaſurer meaſures only from ſome plain piece of ground at the bottom of the hill to the top, whereas it may be, that the country, wherein one of thoſe mountains ſtands, may be exceedingly much higher than that, wherein the other is placed: which difference of heights in the ſeveral countries he, that is to meaſure only the height of one of the mountains, is not wont to take any notice of; and conſequently though in reſpect of the plains, adjacent to the feet of the mountains, their altitudes may be equal, yet in reſpect of the level, or ſuperficies of the terraqueous globe, conſidered as having no mountains at all but thoſe two, the height of the one may far exceed that of the other; and ſo the pic of *Teneriff* being looked upon from the level of the ſea, may be much leſs high than ſome other hills, but may appear much higher than ſome other hills, which yet protuberating above the level part of ſome country, which is itſelf generally exceeding high, may have its top more remote from the centre of the earth, than that of the pic, and would appear higher than it, if as well the one as the other were looked upon from the ſame ſuperficies of the ſea.

The like conſideration I ſince found to have been had, before me, by the learned *Ricciolus*.

BUT to return to the height of the atmoſphere; in order to the making an eſtimate of what we have conſidered as to the height of mountains, I ſhall add, that though by what has been already ſaid touching the height of the pic and other hills, it appears, that the atmoſphere reaches far higher than many learned men would hitherto allow; yet we are not to think, that the atmoſphere may not reach almoſt incomparably higher than the tops of mountains. Nor do I ſuffer my ſelf to be concluded by what many commentators of *Ariſtotle* and other writers are wont to teach, touching the diſtinct narrow extent they allow to that ſphere, within whoſe limits they would have the ſteams of the terreſtrial globe to produce meteors. How far the height of mountains may make the air at the tops of them inconvenient for reſpiration, ſhall be (God permitting) conſidered, when I come to acquaint your lordſhip with my looſe trials about reſpiration.

EXPERIMENT XXIV.

Shewing that the preſſure of the atmoſphere may be exerciſed enough to keep up the mercury in the Torricellian experiment, though the air preſs upon it at a very ſmall orifice.

BY a very ſlight variation of the foregoing 22d experiment we may both confirm one of the moſt important, and the leaſt likely truths of the hydroſtaticks, and remove an objection, which, for want of the knowledge of this truth, is wont to be urged againſt our hypotheſis even by learned men. For divers of theſe, when they ſee the ſame phænomena happen in the Torricellian experiment, whether it be made in the open air, or in a chamber, are forward to object, that if it were, as we ſay it is, the weight of the air, incumbent on the ſtagnant mercury, which keeps that ſuſpended in the tube from falling down, the mercury would not be ſuſtained at any thing near the ſame height in the open air, where the pillar, that is ſuppoſed to lean upon the ſtagnant mercury, may reach up to the top of the atmoſphere, as in a cloſe room, where they imagine, that no more air can preſs upon it, than what reaches directly up to the roof or cieling. And when to this it is anſwered, that though if a room were indeed exactly cloſed, the ſuſtentation of the mercury ought to be aſcribed to ſome other cauſe than the weight of the impriſoned air, which other cauſe I have elſewhere ſhewn to be its ſpring; yet in ordinary rooms there is ſtill a communication between the internal and external air, either by the chimney, or, if the room have none, by ſome crevice in the window, or by ſome chink between the wall and the door, or at leaſt by the key-hole. And when to this it is objected, that the orifice of the key-hole is much narrower than the ſuperficies of the ſtagnant mercury, and conſequently, though the atmoſphere were not reduced to preſs obliquely on the mercury, yet, entering at ſo ſmall an orifice, it could not preſs ſufficiently upon it; when, I ſay, in anſwer to this objection I have alledged that hydroſtatical theorem, that the preſſure, in ſuch caſes as ours, is to be eſtimated by the heights of the liquors and not the breadths, the aſſertion has been thought unlikely and precarious.

To confirm therefore this hydroſtatical truth, one may take the bended tube, mentioned in the 22d experiment; and inclining it till the greateſt part of the mercury paſs from the ſhorter leg into the longer, the upper end of this ſhorter leg may by the flame of a lamp be drawn out ſo ſlender, that the orifice of it ſhall not be above an eighth or tenth part (not to ſay a much leſs) as big as it was before. For this being done, and the tube erected again, if the tall cylinder of mercury be of the uſual or former height, as we have found it, it will appear congruous to our hypotheſis, that the weight of the external air may exerciſe as much preſſion upon the ſtagnant mercury through a little hole, as when all the upper ſuperficies of that mercury was directly expoſed to it.

And if one have not the conveniency to draw out the ſhorter leg as is preſcribed, one may nevertheleſs make the trial, by carefully ſtopping up the orifice with a cork and cement, leaving only, or afterwards making, a very ſmall hole for the air to paſs in and out. If I had not wanted a fit inſtrument, I would have tried to exemplify the truth of what has been delivered, by adding to the glaſſes we employed to make the fifth experiment ſuch a cover, as might be cemented on to the edge of the glaſs, having only a very ſmall hole in the midſt, at which the atmoſphere would be reduced to exerciſe its preſſure; and the like cover I would have made uſe of in the tenth experiment, about the breaking of glaſs-plates in the unexhauſted receiver, by the bare ſpring of the air.

EXPERIMENT XXV.

Shewing that an oblique preſſure of the atmoſphere may ſuffice to keep up the mercury at the wonted height in the Torricellian experiment, and that the ſpring of a little included air may do the ſame.

BY adding a couple of little circumſtances to the trials lately propoſed, we may confirm two conſiderable articles of our hypotheſis. For, 1. if, inſtead of drawing the ſhorter leg of our barometrical ſiphon, if I may ſo call it, directly upwards, or parallel to the longer leg, as in the foregoing experiment, you make the ſlender part bend off ſo, as that, if it were continued, it would make a right angle with the longer leg of the ſiphon, or elſe an acute angle, tending downwards; this being done, I ſay, if when the tube is erected the mercury reſt at its wonted ſtation, it will appear, that the preſſure of the atmoſphere may be exerciſed upon it as well obliquely, when the pipe that conveys it is either horizontal, or opens downwards. See Plate V. Fig. 3.

And, 2. if, inſtead of bending this ſlender pipe, one ſeal it up hermetically, the continuance of the mercurial cylinder at the ſame height will ſhew, that the ſpring of a very little air, ſhut up with the preſſure of the atmoſphere upon it, (though no more than what the air here below is ordinarily expoſed to by the weight of the incumbent air) is able to ſupport as tall a cylinder of mercury as the weight of the whole atmoſphere, i. e. of as much of it, as can come to exerciſe its preſſure againſt the mercury. See Plate V. Fig. 4.

N.B. If when the ſhorter leg of the baroſcope is ſealed up, you move the inſtrument up and down, the mercury will vibrate, by reaſon of the ſomewhat yielding ſpring of the impriſoned air; but becauſe of the reſiſtance of the ſpring, the motion will be diverſified after an odd and pretty manner: which may be eaſily perceived by the impreſſion it makes upon the hand, but not ſo eaſily deſcribed. And becauſe that, when the ſhorter leg is drawn out ſlender enough, after the inſtrument is furniſhed with quick-ſilver, it is eaſy to ſeal it up with the flame of a candle, without the help of any inſtrument at all, I ſhall here take notice to your lordſhip, (which I could not reaſonably do before,) that it may on ſome occaſions be convenient to ſeal up the barometer, before it be tranſported, and, in ſome caſes, to incline the tube beforehand, till the quick-ſilver have quite fill'd the longer leg: by this means the vibrations of the quick-ſilver will be leſs than otherwiſe they would be, and it will be no trouble at all, when the inſtrument is brought to the deſigned place, to break off the ſlender apex of the ſhorter leg, and ſo expoſe again the mercury to the preſſure of the atmoſphere.

As about the former experiments, ſo about theſe two this advertiſement may be given; viz. That the ſame trials, for the main, may be made without confining one's ſelf to the propoſed ways of making them.

1. For the firſt of theſe new trials may be made by cementing very carefully on to the orifice of the ſhorter leg (which need not be altered) a ſhort pipe of glaſs, whoſe upper end may be drawn out very ſlender, and bent either horizontally or downwards; which is far eaſier to be done, than to draw out the ſhorter leg, when the glaſs is furniſhed with mercury.

2. And as for the ſecond trial, that may be well enough made, by carefully ſtopping the unaltered orifice of the ſhorter leg with a good cork, and our cloſe cement, or with the latter only; and when you would afterwards uſe this inſtrument as a baroſcope, you need but heat a pin or ſlender wire red hot, and ſo burn a hole through the ſtoppel.

And this expedient, which I could not conveniently advertiſe your lordſhip of ſooner, may be of uſe, when a travelling baroſcope is to be often removed; becauſe having once ſtopped the whole orifice well, it is far more eaſy to ſtop and open a pin-hole accurately, than to cloſe and unſtop the whole orifice of the tube.

Note, I endeavoured to confirm more than one of the foregoing particulars by this one experiment. Having cauſed a portable barometer to be made with the ſhorter leg of a ſomewhat more than ordinary length, I afterwards cauſed the upper part of this leg to be drawn out very ſlender, as in this 25th experiment; and laſtly, I cauſed the ſame ſhorter leg to be either about or ſomewhat above the middle bended downwards, ſo that the ſmall orifice of the ſlender apex pointed towards the ground. This done, I was to have meaſured the height of the ſuſpended mercury, but not having a fit ruler at hand, I then deferred, and afterwards forgot to do it; but I remember, that neither I, nor ſome others verſed in ſuch experiments, to whom I ſhewed it, took any notice, that the mercury was leſs high than in ordinary barometers; whence it was concluded, that the atmoſphere could exerciſe his preſſure not only at a very ſmall orifice, which in our experiment did little, if at all, exceed a pin-hole, but when the air muſt at this little orifice preſs upwards to be able to preſs upon the ſurface of the ſtagnant mercury.

EXPERIMENT XXVI.

About the making of a baroſcope (but of little practical uſe) that ſerves but at certain times.

To ſhew ſome ingenious men by a medium, that has not hitherto (that I know of) been made uſe of, that the not ſubſiding of quick-ſilver in an inverted tube, that is a little ſhorter than thirty inches, or thereabouts, does not proceed from ſuch a *fuga vacui* as the ſchools aſcribe to nature, but from the gravity of the external air, I deviſed the following experiment.

Having made choice of a time, when it appeared by a good baroſcope, which I had frequently conſulted for that purpoſe, that the atmoſphere was conſiderably heavy, I cauſed a glaſs-pipe, hermetically ſealed at one end, and in length about two foot and a half, to be filled with quick-ſilver, ſave a very little, wherein ſome drops of water were put, that we might the better diſcern the bubbles, if any ſhould be left after the inverſion of the tube into an open glaſs with ſtagnant mercury in it. Having by this means, though not without difficulty, freed the tube from bubbles, we ſo ordered the matter, that the quick-ſilver and the little water, that was about it, filled the tube exactly, without leaving any interval, that we could diſcern at the top, and yet the mercurial cylinder was but very little higher than that of our baroſcope was at that time.

This done, the newly filled pipe was left erected in a quiet place, where the liquors retained their former height for divers days. But though an ordinary ſchool-philoſopher would confidently have attributed this ſuſtentation of ſo heavy a body to nature's fear of admitting a vacuum; yet it ſeems, that either ſhe is not always equally ſubject to that fear, or ſome other cauſe of the phænomenon muſt be aſſigned; for when (a pretty while after) I had obſerved by the baroſcope, that the atmoſphere was grown much lighter than before, repairing to my ſhort tube, I found, that according to my expectation the quick-ſilver was not inconſiderably ſubſided, and had left a cavity at the top, which afterwards grew leſſer, according as the atmoſphere grew heavier.

N.B. 1. The tube employed about this experiment may be brought to the requiſite ſhortneſs, either by wearing off a little of the glaſs at the orifice of it, or by increaſing the height of the ſtagnant mercury, into which it hath been inverted.

2. When the quick-ſilver in our ſhort tube was much ſubſided, there appeared in the wa-

ter that ſwam upon it a little bubble, about the bigneſs of a ſmall pin's-head; but, conſidering how careful we had been to free the tube from bubbles before we ſet it to reſt, it may very well be, that this ſo ſmall a bubble was not produced, till after the ſubſiding of the quickſilver, whereupon the aerial particles in the water became leſs compreſſed than before; not to mention, that the bubble (ſuch as it was) appeared very much greater than it would have done, if the preſſure of the atmoſphere had not been kept from it by the weight of the ſubjacent pillar of mercury.

EXPERIMENT XXVII.

About the aſcenſion of liquors in very ſlender pipes in an exhauſted receiver.

WHAT I related to your lordſhip, in the 35th of the publiſhed experiments, about the ſeemingly ſpontaneous aſcenſion of water in ſlender pipes, has occaſioned the making of many trials by the curious, whereby that experiment has been not a little diverſifyed. But becauſe among thoſe I have yet heard of, none have been made in our engine, it may not be amiſs to add the following trial, which may be of uſe in the examen of one or two of the chief conjectures, that have hitherto been propoſed about the cauſe of that odd phænomenon.

We tinged ſome ſpirit of wine with cocheneal, which being put into the receiver, and the air withdrawn, did exceedingly bubble for a pretty while. Then little hollow pipes of different ſizes being put into it, the red liquor aſcended higher in the ſlenderer than the others; but upon the withdrawing of the air there ſcarce appeared any ſenſible difference in the heights of the liquor, nor yet upon the letting it in again.

Afterwards two ſuch pipes of differing ſizes, being faſtened together (at a diſtance) with cement, were let down into the ſame ſpirit of wine, when the receiver was well exhauſted, notwithſtanding which the liquor aſcended in them, for ought we could plainly ſee, after the ordinary manner; only when the air was let in again, there ſeemed to be ſome little (and but very little) riſing, at leaſt, in one of the pipes. In this trial, this phænomenon was noted; that though there appeared no bubbles at all in the veſſeled ſpirit of wine, (notwithſtanding that we continued to pump,) yet there did for a pretty while ariſe bubbles in that part of the liquor, that was got into the ſlender pipes; which I gueſſed to proceed from the ſuſtentation (in part) of the ſpirit of wine, made by the inſide of the pipe, whereto it adhered.

EXPERIMENT XXVIII.

About the great and ſeemingly ſpontaneous aſcenſion of water in a pipe filled with a compact body, whoſe particles are thought incapable of imbibing it.

UPON occaſion of the (ſeemingly) ſpontaneous aſcenſion of water in ſlender pipes of glaſs, I conſidered, that it would be eaſy by another way to make it riſe to a far greater height than hitherto had been done; for ſince we had found by obſervation, that, *cæteris paribus*, the ſlenderer the little pipes were that we employed, the higher the liquor would riſe in them; and ſince the hydroſtaticks had taught us, that oftentimes, even in very crooked pipes, water would be made to aſcend by the ſame ways (of raiſing it) to the ſame perpendicular height (or thereabouts) as in ſtraight ones; I thought, that I might well ſubſtitute a powder, conſiſting of ſolid corpuſcles heaped upon one another, and included in a glaſs-cane, inſtead of the little pipes I had hitherto uſed. For I conſidered the little intervals, that would neceſſarily be left between theſe differingly ſhaped, and confuſedly placed corpuſcles, would allow paſſage to the water, as did the cavities of the little pipes, and yet would in many places be ſtraighter than the ſlendereſt pipes I had uſed. And though beaten glaſs, or fine ſand, &c. might have been employed about this experiment, yet I judged it far more convenient to make uſe of ſome metalline calx, becauſe the operation of the fire, making a more exquiſite comminution of ſolid bodies than our peſtles are wont to do, is fit to ſupply us with exceeding minute granes, that intercept proportionable cavities between them.

Upon this conſideration therefore (beſides others to be hereafter hinted) I took a ſtraight pipe of glaſs, open at both ends, and of a moderate wideneſs, (for it need not be very ſlender,) and having tied a linen-rag to one end of it, that the water might have free paſſage in, and the powder not be able to fall out, we carefully, and as exactly, as we could, filled the cavity with minium, (which is lead calcined, without addition, to redneſs;) and then having erected the tube, ſo that the bottom of it reſted upon that of a ſomewhat ſhallow, and open mouthed glaſs, containing water enough to ſwim an inch or two above the bottom of the tube; into whoſe cavity it did, as I expected, inſinuate itſelf by degrees, as appeared by a little change of colour in that part of the minium which it reached, till (the open glaſs being from time to time ſupplied with freſh liquor) it attained to the height of about 30 inches. And then, our Society expreſſing a curioſity to ſee it, and have it placed among better things, I was hindered from making any further obſervations with that particular glaſs.

This was (if I forget not) about the latter end of the year 1662.

Wherefore taking afterwards another tube, and ſome minium carefully prepared, I proſecuted the experiment, ſo as to make the water riſe in the pipe about 40 inches above the ſurface of the ſtagnant water. I gueſſed it had riſen higher, but, by reaſon that at the upper part of the minium the difference of colour was ſo ſmall, as not to be eaſily diſtinguiſhable with certainty, I forbore to allow a greater height to the aſcenſion of the water: nor could I, where I then was, much promote the experiment, for want of ſuch accommodations as I deſired; but about the experiment, as I tried it, I ſhall take notice of the following particulars.

I tried ſome other powders beſides red lead, (as beaten glaſs, pieces of fine ſpunge, putty, &c.) but did not find any of them do ſo well; which ſucceſs was yet perhaps but accidental, and therefore the trial may be repeated, eſpecially with putty, becauſe that being a metalline calx as well as minium, conſiſts of very ſmall grains, and by reaſon of its great whiteneſs, receives a greater change of colour by wetting, than minium does; in which, eſpecially if it be very fine, the diſcoloration, that water makes towards the upper part of the tube, is ſometimes not ſo eaſy to be clearly diſcerned.

2. I did indeed endeavour to remedy this inconvenience, by uſing, inſtead of meer water, tincted liquors, as ink, tincture of ſaffron, &c. but they ſeemed not to riſe near ſo high as water alone, as if the diſſolved ingredients did by degrees choak the pores of the minium.

3. To have the grains of our powder more minute, and the ſmaller intervals between them, I choſe not only to uſe the fineſt ſort of minium I could procure, but alſo to ſift it through a very fine ſearce, and to put it but by little and little into the tube, that by ramming it from time to time, it might be made to lie the cloſer; which expedients ſucceeded not ill.

4. It ſeemed by a trial or two (for I am not ſure the obſervation will always hold,) that if the tube were very ſlender, (as about the bigneſs of a ſwan's quill,) the experiment ſucceeded not well.

5. It may be worth while to obſerve, in what times the water aſcends to ſuch and ſuch heights; for at the beginning, it will aſcend much faſter than afterwards, and ſometimes it will continue riſing 24 or 30 hours, and ſometimes perhaps much longer.

6. One of the ſcopes I propoſed to my ſelf in this experiment was to diſcover a miſtake in the explication, that ſome learned modern writers have given us of the cauſe of filtration; for, whereas they teach, that the parts of filtre, that touch the water, being ſwelled by the ingreſs of it to their pores, are thereby made to lift up the water, till it touch the ſuperior parts of the filtre, that are almoſt contiguous to them; by which means, theſe being alſo wetted, and ſwelled, raiſe the water to the other neighbouring parts of the filter, till it have reached to the top of it, whence its own gravity will make it deſcend. But in our caſe, we have a filter made of ſolid metalline corpuſcles, where it will be very hard to ſhew, that any ſuch intumeſcence is produced, as the recited explication requires.

7. Water aſcends ſo few inches even in very ſlender pipes, as to ſeem much to favour their judgment, who diſallow the conjecture lately entertained by ſome ingenious men, (particularly Mr. *H.*) about the raiſing of the ſap in trees, after the like manner, that water is raiſed in ſlender pipes. But without fully delivering yet my thoughts of that ſpeculation, I may take notice, that in the laſt tryal above recited, I made water to aſcend near, if not above, 3 foot $\frac{1}{2}$; and if, by ſo ſlight an expedient, water may be made to riſe as high as is neceſſary for the nutrition of ſome thouſand of plants, (for ſuch a number there is, that exceed not 3 foot $\frac{1}{2}$ in height) one may without abſurdity aſk, Why it is not poſſible, that nature, or rather the moſt wiſe author of it, may have made ſuch contrivances in plants, as to make liquors aſcend in them, to the tops of the talleſt trees; eſpecially ſince, beſides divers things that we may already ſuſpect, (as heat, and ſomething equivalent to well placed valves) many others, that perhaps are not yet dreamt of, may probably concur to the effect.

8. As I formerly made, by bending the ſlender pipes we have been talking of, ſhort ſiphons, through which the water runs, without being at firſt aſſiſted by ſuction, ſo I thought fit to try, whether I could not in larger pipes, by the help of minium, make much longer ſiphons. But though when the orifices were turned upwards, fine minium were rammed into both the legs, and the orifices were both of them cloſed; yet when they came to be again turned downwards, the weight of the minium would ſomewhere or other (and, for the moſt part, at, or near, the flexure) make ſome ſuch chink, or diſcontinuation, as to hinder the farther progreſs of the water. Which impediment, though I judged it ſuperable enough, (eſpecially by making, at the flexure, a little pipe or ſocket, by which both legs might be cloſely filled) yet for want of accommodations and leiſure, it was left unſurmounted. Upon which account alſo, I did not ſatisfy my ſelf about the ſucceſs of ſome former tryals, as of the aſcenſion of water into pieces of wood of differing ſorts, the operation of the viciſſitudes of the ſun's beams, and the abſence of them, upon liquors aſcending in tubes filled with minium, &c.

9. Whether the preſſure of the outward air be the cauſe of the aſcenſion of liquors in our tubes, furniſhed with minium, is a problem, in order to whoſe ſolution, I could acquaint your lordſhip with a contrivance, wherewith to make ſome trials in our engine. But ſince it can ſcarce be well deſcribed without many words, unleſs you expreſs a particular curioſity to know it, I ſhall not trouble you with it: and the rather, becauſe the beſt way I know of examining this difficulty belongs to the ſecond part of this Continuation, where mention is made of an attempt about it, which did not, I confeſs, diſpleaſe me.

EXPERIMENT XXIX.

Of the ſeemingly ſpontaneous aſcenſion of ſalts along the ſides of glaſſes, with a conjecture at the cauſe of it.

TO the ſame cauſe, or the like, with that of the aſcenſion of water, in ſlender pipes, may be probably referred an odd phænomenon, which, though I remember not to have been mentioned by any chymical or other writer, I have not unfrequently obſerved, as well by chance, as in trials purpoſely made to ſatisfy my ſelf, and others, about the truth of it.

The phænomenon, in ſhort, was this. That having, in wide mouthed glaſſes, (which ſhould not be very deep) expoſed to the air a ſtrong ſolution of common ſea-ſalt, or of vitriol, which reached not, by ſome inches, to the top of the glaſs; and having ſuffered much of the aqueous part to exhale away very ſlowly, the coagulated ſalt would, at length, appear to have lined the inſide of the glaſs, and to have aſcended much higher, not only than the place where the ſurface of the remaining water then reſted at, but than the place, to which the liquor reached, when it was firſt poured in. And if the experiment were continued long enough, I ſometimes obſerved this aſcenſion of the ſalt to amount to ſome inches, and that the ſalt did not only line the inſide of the glaſs, but, getting over the brim of it, cover'd the outſide of it with a ſaline cruſt: which made them, that ſaw how little liquor remain'd in the glaſs, admire how it could poſſibly get thither.

And though I have mentioned but the ſolution of vitriol, and ſea-ſalt, becauſe they are much eaſier than others to be procured, and yet the experiment ſucceeds better in them than in ſome other far leſs parable ſalts; yet they are not the only ones, by whoſe ſolutions the recited phænomenon may be exhibited.

As for the cauſe of this odd effect, though I ſhall not propoſe any thing about it with confidence, till I have further inquired into it, and eſpecially till I have tried, whether the phænomenon may be produced in an exhauſted receiver; yet by what I have hitherto obſerved, I am inclined to conjecture, that it may be referred to ſuch a cauſe as that of the aſcenſion of liquors in pipes, after ſome ſuch manner as this.

First, I obſerved, that in water, and aqueous liquors, that part of the ſurface, which is next the ſides of the glaſs, is (whatever the reaſon of it be) ſenſibly more elevated than the reſt of the ſuperficies; and, if very little clippings of ſtraw, or other ſuch minute, and light bodies, floating upon the water, chance to approach near enough to the ſides of the glaſs, they will be apt (which one would not expect) to run up, as it were, this aſcent of water, and reſt againſt the ſides of the glaſs.

Next we may take notice, with the ſalt-boilers, and chymiſts, that ſea-ſalt is uſually wont to coagulate at the top of the water, in ſmall and oblong corpuſcles, ſo that as to theſe, it is eaſy to conceive to them, that have conſidered the firſt obſervation, how numbers of them, may faſten themſelves round about to the inſide of the glaſs. And beſides ſea-ſalt, I have found by trial divers others, if their ſolutions be ſlowly enough evaporated, that will, whilſt yet there remains a good proportion of liquor, afford ſaline concretions at the top of the water. And the faſtning of ſaline particles to the ſides of the glaſs may perhaps be promoted by the coldneſs, that may be communicated to the corpuſcles contiguous to the glaſs, by reaſon of the coldneſs, which the glaſs may be ſuſpected to have, upon the ſcore of its denſity, in compariſon of water. But to proceed: I conſider, that by the evaporation of the aqueous parts of the ſolution, the ſurface of the remaining liquor muſt neceſſarily ſubſide, and thoſe ſaline particles, that were contiguous to the inſide of the glaſs and the more elevated part of the water, having no longer enough of liquor to keep them diſſolved, will be apt to remain ſticking to the ſides of the glaſs, and upon the leaſt farther evaporation of the water, will be a little higher than the greater part of the ſuperficies of that liquor; by which means it will come to paſs, that, by reaſon of the little inequalities, that will be on the internal ſurface of the adhering corpuſcles of the ſalt, and perhaps alſo on the internal ſuperficies of the glaſs, there will be intercepted between the ſalt and the glaſs little cavities, into which the water contiguous to the bottom will aſcend or be impelled, upon ſuch an account as that, whereon it is raiſed in ſlender pipes. And when the liquor is thus got to the top of the ſalt, and comes to be expoſed to the air, the ſaline part may, by the evaporation of the aqueous, be brought to coagulate there, and conſequently to increaſe the height of the ſaline film, if I may ſo call it; which, by the like means, may be at length brought to reach to the very top of the glaſs, whence it may eaſily be brought over to the outſide of the veſſel, where the natural weight of the ſolution will facilitate its progreſs downwards; and the ſkin of ſalt, together with the contiguous ſurface of the glaſs, may, at length, conſtitute a kind of ſiphon.

To this explication it agrees well, that I have uſually obſerved the ſaline film hitherto mentioned to be with great eaſe ſeparable from the glaſs in large fleaks; which argues, that they did not ſtick cloſe to one another, except in ſome few places, but had a thin cavity intercepted between them, through which the water might aſcend.

Nor is it repugnant to this explication, that in caſe the water aſcended, it ſhould, as it ſeems, diſſolve the ſalt. For the liquor being already upon the point of concretion, is ſo glutted with ſalt, that it can diſſolve no more. Whence we may alſo render a reaſon, why, when the ſaline film chances to reach to the outſide of the glaſs, the liquor divers times does not run down to the bottom, but is coagulated by the way. And I have alſo had a ſuſpicion, (though I could not ſeaſonably take notice of it before now,) that when the concretion is once begun, the film may be raiſed and propagated, not only by the motion of the liquor between the inſide of it and the glaſs, but by the ſame liquor's inſinuating it ſelf on the outſide of the film into the ſmall chinks and creviſes, intercepted between the ſaline corpuſcles, as ink (eſpecially if ſomewhat thin) riſes into the ſlit, and along the ſides of the nib of a pen, though nothing but its very point be dipt in the ſurface of the liquor. And by this means the impregnated ſolution may, as it were, climb up to the top of the ſaline concretion, and by coagulating there add to its height.

SOME other circumſtances I have noted of our phænomenon, that agree with the propoſed explication; but perhaps it would not be worth while to ſpend more time about it. Not to examine here, whether what has been related, ſo as to make it probable, that aſcending water may carry up wherewithal to heighten and increaſe the pipes, or veſſels, through which it riſes, may contribute any thing more than was ſuggeſted in the former 28th experiment, towards the explication of the riſing and diffuſing of the ſap in trees.

EXPERIMENT XXX.

About an attempt to meaſure the gravity of cylinders of the atmoſphere, ſo as that it may be expreſſed by known and common weights.

WHILST I was making the former experiments, it was more than once my wiſh, that by knowing the juſt weight of a cylinder of quick-ſilver of a determinate diameter, and of 29 or 30 inches high, which is near the height, that the air does uſually counterballance, I might the better eſtimate the weight of a cylinder of the atmoſphere of that diameter, and conſequently make the better gueſſes, how near the effects of the ſpring of the air, as well as of its weight, produced by the help of our engine, approached to the utmoſt of what might have been expected, in caſe all the inſtruments employed had been perfect, and all concurrent circumſtances had been favourable. And upon this account, I ſeveral times regretted my want of a long inſtrument of ſteel, or hardned iron, wherewith I many years ſince made an obſervation, that was more carefully regiſtered than preſerved, of the weight of a mercurial cylinder of a determinate height, as well as diameter; which weight I did not think ſo ſafe to determine by the help of glaſs tubes, becauſe it is very difficult to have them uniformly cylindrical, and to know that they are ſo, in regard that they are formed but by blowing and drawing out; and, beſides the inequality, that may happen to the cavity upon other accounts, it is very difficult to make the ſides of the glaſs equally thick, and to examine whether they be ſo, or no.

BUT, at length, lighting upon (what I had too often wanted in the foregoing experiments) a dextrous artificer, that chanced to come for a while to the place where I then was, I endeavoured to repair my loſs, as well as he could help me to do it, by cauſing him to turn very carefully a cylindrical piece of braſs, of an inch in diameter, and 3 inches in length, and open (that it might be the better wrought) at both ends, to one of which was exactly fitted a flat bottom of the ſame metal, faſtned very cloſe to it with little ſcrews on the outſide; this being judged a better way, than if it had been turned all of a piece.

THIS inſtrument being diligently counterpoiſed in a truſty pair of ſcales, was carefully filled with mercury, which (for greater caution) we took out of a new parcel, that we had not yet employed about other experiments; and finding it to weigh 17 ounces, 1 dram, 45 grains, Troy weight, (or 137 drams 45 grains) multiplying that by 10, there will come for the weight of a mercurial cylinder, of one inch in diameter, and 30 inches in height, (and ſo high I have divers times ſeen the mercury to be in a good barometer) about 14.2 lb. (i. e. 14 lb. 2 ounces, and above three drams, Troy weight;) and almoſt 11.8 lb. Avoirdupoize weight (i. e. 11 lb. 12 ounces, and above 6 drams) which is a greater weight, than, without ſuch a trial, one would eaſily imagine, that ſo ſhort a cylinder of mercury, and much leſs that a cylinder of ſo light a body as air, being neither of them above an inch diameter, could amount to.

NOTE firſt, to examine at the ſame time the weight of the mercury, and its proportion to water, we did, before the mercury was poured into the braſs-veſſel, fill it with water, (after which, we wiped it dry before the mercury was put into it;) and this liquor weighing 10 drams, and 15 grains, the proportion between the mercury and the water appeared to be that of $13\frac{18}{41}$ to 1: which, though it ſeem ſomewhat of the leaſt, yet your Lordſhip may remember, that I formerly told you, I had ſeveral times found the received proportion of 14 to 1, between mercury and water, to be ſomewhat too great; and beſides that, in a veſſel, whoſe orifice was no leſs than an inch in diameter, it is exceeding difficult to be ſure, when it is preciſely full, either of water, or mercury; becauſe the former has a ſuperficies conſiderably concave, and the the other one, that is notably convex; and though we uſed ſome little artifices (which would be troubleſome here to mention) to eſtimate the protuberance of the one liquor, and the deficience of the other, as near the truth as could be, yet I am not ſure, but there may have been a few mercurial corpuſcles more than there ſhould have been, and that conſequently ſome ſmall abatement may have been made, of the weight newly attributed to the whole mercurial cylinder of 30 inches.

2. I had thoughts of making uſe of the barrel of a gun, of a convenient length, to find the weight of a mercurial cylinder of 2 foot and $\frac{1}{2}$; but I preferred the inſtrument already made uſe of (eſpecially not being where I could have one bored after a peculiar way) not only becauſe I could not meet with one, whoſe diameter was juſt an inch, and conſequently as convenient for calculations, and becauſe that the barrels of guns are often bored a little tapering; but becauſe a ſkilful artificer confeſſed to me, that they ſcarce ever bore ſuch barrels, but with a four-ſquare bit (as they call it,) which leaves the cavity too angular, or too imperfectly round; whereas if an hexahedrical bit be imployed, it will, as he affirmed, make the cavity almoſt as cylindrical, as can be reaſonably deſired. I ſay nothing here of making uſe, for our purpoſe, of a trunk, as they call a hollow cylinder of wood, becauſe I elſewhere ſhew, that wood (at leaſt, ſuch as the trunks to ſhoot pellets with are wont to be

made of) is not of a texture cloſe enough for ſuch an uſe.

3. Because in cylinders of mercury, 30 inches is a height, which the atmoſphere is ſeldom heavy enough to be able to counterpoiſe; and becauſe 29 inches is ſomewhat nearer the middle between the greateſt and the leaſt heights, at which I have obſerved the mercury at differing times to ſtand in good barometers. Your lordſhip may, if you pleaſe, abate a thirtieth part of the weight aſſigned above to a mercurial cylinder of 30 inches, (though I take 29 and ¼, or thereabouts, to be ſomewhat a more uſual height of the mercury, than preciſely nine and twenty.)

4. The weight of a mercurial cylinder in an æquilibrium with the atmoſphere, and of one inch in diameter, being thus ſettled, we may, by the help of the doctrine of proportions, and a few propoſitions, eſpecially the 14th of the 11th book of *Euclid*'s Elements, eaſily enough calculate the weight of a cylinder of mercury of another diameter, and conſequently the force of the preſſure of an atmoſpherical pillar of the ſame diameter. For ſince according to the forenamed 14th propoſition of the 11th, cylinders of equal heights are to one another as their baſes; and ſince by the ſecond propoſition of the ſame 11th element, circles (ſuch as are the baſes of cylinders) are to one another, as the ſquares of their diameters; and ſince laſtly, we ſuppoſe, that mercury being a homogeneous body, at leaſt as to ſenſe, the mercurial cylinders will have the ſame proportion to each other in weight, that they have in bulk; ſince, I ſay, theſe things are ſo, if, for inſtance, we deſire to know, what will be the weight of a cylinder of 30 inches high, whoſe diameter is two inches, the rule will be this,

As the ſquare of the diameter of the ſtandard cylinder, (as I call that, whoſe weight is already known) is to the ſquare of the diameter of the cylinder propoſed; ſo will the bulk of the former cylinder be to that of the latter, and the weight of that to the weight of this.

According to which rule, the ſquare of one inch (which is the diameter of the ſtandard cylinder) being but one, (whereby your lordſhip may perceive, how much the meaſure I pitched on facilitates computations,) and the ſquare of two (which is the diameter of the propoſed cylinder) being four, the bulk, or ſolid contents of this latter cylinder, and conſequently its weight, will be four times as great as thoſe of the ſtandard cylinder; and ſo, ſince the leſſer has been already ſuppoſed to weigh 11.8 lb. avoirdupoiſe, the mercurial cylinder of two inches in diameter, will weigh 47.2 lb. of the ſame weight.

EXPERIMENT XXXI.

About the attractive virtue of the loadſtone in an exhauſted receiver.

Some learned modern philoſophers, that have attempted to explicate the cauſe and manner of magnetical attraction or coition, give ſuch an account of it, as ſuppoſes, that the air between the two magnetical bodies, being driven away by their effluviums from between them, preſſes them on the parts oppoſite to thoſe, where the contact is to be made; and upon ſome ſuch ſcore (for I muſt not now ſtay to deliver their theories circumſtantially) the air is ſuppoſed to contribute very much to the attraction and ſuſtentation of the iron by the loadſtone: wherefore, partly to examine this opinion, and partly for ſome other purpoſes (not neceſſary now to be mentioned) we thought fit to make the following experiment.

We took a ſmall, but vigorous loadſtone, capped and fitted with a looſe plate of ſteel, ſo ſhaped, that when it was ſuſtained by the loadſtone, we could hang at a little crook, that came out of the midſt of it, and pointed downward, a ſcale, wherein to put what weights we ſhould think fit. Into this ſcale we put ſometimes more and ſometimes leſs weight; and then by ſhaking of the loadſtone as much as we gueſſed it would be ſhaken by the motion of the engine, we found the greateſt weight, that we preſumed it would be able to ſupport, in ſpite of the agitation it would be expoſed to, which proved to be, beſides the iron-plate and the ſcale, ſix ounces Troy weight, to which if we added half an ounce more, the whole weight appeared too eaſy to be ſhaken off. This done, we hung the loadſtone, with all the weight it ſuſtained, at a button of glaſs, which we had procured to be faſtened on to the top of the inſide of a receiver, when it was firſt blown; and though, in about 12 exſuctions, we uſually emptied ſuch receivers as much as was requiſite for moſt experiments; yet this time, to exhauſt it the more accurately, we continued pumping, till we had exceeded twice that number of exſuctions; at the end of which time, ſhaking the engine ſomewhat rudely, without thereby ſhaking off the weight that hung at the loadſtone, the iron ſeemed to be very near as firmly ſuſtained by it, as before the air began to be pumped out. I ſaid very near, rather than altogether, becauſe that the withdrawing of the air, though it be not ſuppoſed to weaken at all the power of the loadſtone preciſely conſidered, yet it muſt leſſen its power to ſuſtain the ſteel; becauſe this in ſo thin a medium muſt weigh heavier, than in the air, by the weight of as much air, as is equal in bulk to the appended body.

Some other magnetical trials (and alſo ſome electrical ones) I remember I attempted to make by the help of our engine; but not having the notes I took of them now at hand, I ſhall ſuſpend the mentioning them, till I can give your lordſhip a more punctual account of them.

EXPERIMENT XXXII.

Shewing, that when the preſſure of the external air is taken off, it is very eaſy to draw up the ſucker of a ſyringe, though the hole, at which the air of water ſhould ſucceed, be ſtopped.

Having taken notice, that ſome learned oppoſers of the modern doctrine about the weight of the atmoſphere think themſelves more than ordinarily befriended by the difficulty we find in drawing up the embolus, or ſucker of a ſyringe, when the hole, at which the air or water ſhould ſucceed, is ſtopped; and by the violence, with which, as ſoon as it is let go, it is, as they imagine, drawn back. And ſuppoſing the reaſon of this confidence of theirs to be, that men have not yet been able in theſe phænomena (as in ſome others) to prove the intereſt of the atmoſphere's gravity by direct or confeſſedly analogous experiments; I preſumed it will not be unwelcome to your lordſhip, if I here fortify the ſpeculations, that have been, or may be propoſed to explicate theſe things according to the hypotheſis of the weight of the air, by what we tried to that purpoſe, among others, when we were making uſe of a ſyringe in our engine.

The FIRST TRIAL.

WE took a ſyringe of braſs, (that metal being cloſer and ſtronger than pewter, of which ſuch inſtruments are uſually made,) being in length (in the barrel) about ſix inches, and in diameter about one inch $\frac{1}{8}$; and having, by putting a thin bladder about the ſucker, and by pouring a little oil into the cavity of the cylinder (or barrel,) brought the inſtrument to be ſtanch enough, and yet the ſucker to move to and fro without much difficulty, we thruſt this to the bottom (or baſis) of the barrel to exclude the air; and having unſcrewed, and laid aſide the ſlender pipe of the ſyringe (which in this and ſome other trials, was like to prove not only needleſs, but inconvenient) we carefully ſtopped the orifice, to which the pipe in theſe inſtruments is wont to be ſcrewed, and then drawing up the ſucker, we let it go, to judge by the violence, with which it would be driven back again, whether the ſyringe were tight enough for our purpoſe; and finding it to be ſo, we faſtened to the barrel a ponderous piece of iron to keep it down, and then faſtening to the handle of the rammer (or axle-tree of the ſucker) one end of a ſtring, whoſe other end was tied to the often-mentioned turning key, we conveyed this ſyringe, and the weight belonging unto it, into a receiver; and having pumped out the air, we then began to turn the key, thereby to ſhorten the ſtring that tied the handle of the ſyringe to it; and, as we foretold, that the preſſure of the air, lately included in the receiver, being withdrawn, we ſhould no more find the wonted reſiſtance in drawing up the ſucker from the bottom of the cylinder, ſo we found upon trial, that we could very eaſily pull it up without finding any ſenſible reſiſtance.

See Plate VI. Fig. 1.

HOWEVER, having thought fit to repeat the experiment, (which we did with the like ſucceſs,) leſt it might be objected, that this want of reſiſtance might proceed, as partly from our employing the turning key to raiſe the ſucker, ſo principally from ſome unperceived leak, at which the air may be ſuppoſed to have got into the cavity of the cylinder; I thought fit not only to examine by trial, after the receiver was removed from off the pump, whether the ſyringe were not ſtanch, (upon which I found, that I could not, without ſome ſtraining, draw up the ſucker even a little way; and that it would be violently beaten back again,) but alſo in one of theſe experiments to make this variation; that when the receiver being exhauſted, we had drawn up the ſucker almoſt to the top of the barrel, by ſuch a ſtring as was purpoſely choſen ſomewhat weak, we kept the parts of the ſyringe in that poſture, till we had open a paſſage to the outward air, upon whoſe ingreſs the ſucker was (as we intended it ſhould be) ſo forceably depreſſed, that it broke the ſtring, by which it was tied to the turning key, and was violently driven back to the lower part of the barrel, and that notwithſtanding theſe two diſadvantageous circumſtances; one, that the ſtring was not ſo weak, but that one, whom I employed to try it before it was faſtened to the ſyringe, made it ſuſtain a lump of iron, that weighed between four and five pounds; and the other, that yet this ſtring was broken long before all the air, that flowed in to fill the receiver, had got in: ſo that the preſſure of all the admitted air would doubtleſs have broken a much ſtronger ſtring, if we had employed ſuch a one to reſiſt the depreſſion of the ſucker, which will yet be more evident by a phænomenon of our ſyringe, that I ſhall preſently have occaſion to relate.

The SECOND TRIAL.

Containing a variation of the foregoing.

WE took the ſyringe employed in the foregoing experiments, and having found by trial, that it was, though not perfectly, tight, nor altogether ſo much ſo as before, yet enough ſo for our preſent purpoſe, (ſince, when the orifice of the vent in the baſis was ſtopped, if the ſucker were more forceably drawn up a little way, and then let go, it would haſtily return, or rather violently be impelled back towards the bottom of the barrel) we made it ſerve us as well as we could for the following experiment. Of this ſyringe we did very carefully with a cork and our cement cloſe the vent; and then having tied to the barrel of the ſyringe a weight, that happened to be at hand (and to amount to two pound, and as many ounces) we ſuſpended the rammer of the ſyringe by a ſtring in a large receiver; and then cauſing the pump to be applied, we made 11 or 12 exſuctions of the air, without any appearance of change in the ſyringe. But becauſe I had judged the above-mentioned weight ſufficient, and ſuppoſed, that the little air ſtill remaining in the receiver had yet too ſtrong a preſſure to be ſurmounted by it, I cauſed the pumping to be continued, and within two or three exſuctions more I perceived the cylinder to begin to be drawn down, though but very ſlowly, by the weight hanging at it (aſſiſted by its own gravity:) and likewiſe tried (after having purpoſely ſtopped a-while the working of the pump) that juſt upon a freſh ſuck the deſcent would be manifeſtly accelerated. And when we had ſuffered the barrel and weight to ſlide down as far as we thought fit, we let in the external air, which, as was to be expected, raiſed them both again much faſter than they had ſubſided.

N.B. There would not have needed any thing near ſo great a weight to depreſs the barrel of the ſyringe, but that it is difficult in ſuch an inſtrument to make the ſucker fill it accurately enough, without making it ſomewhat uneaſy to be moved to and fro. Upon which account it was neceſſary, that a weight ſhould be added, not only to ſurmount the preſſure of the air remaining in the receiver, (which was not, nor needed to be diligently exhauſted in this experiment) but to overcome that reſiſtance, which we juſt now noted the inequalities of the inſide of the cylinder, and thoſe of the ſucker to give to the motion of the one in or over the other. And yet for all this it is not eaſy, though it be not impoſſible, to make one of theſe ſyringes very tight, eſpecially when the noſe is well ſtopped, and the ſucker drawn up; there being often ſome little air, that ſtrains in between the ſucker and the barrel, and ſome that will be harboured between the ſucker, though thruſt home, and the bottom of the barrel, beſides what may lurk between the ſame ſucker and the cork that ſtops the orifice of the vent. Nor were we confident, that our ſyringe did not at length let ſome aerial particles inſinuate themſelves into the cavity, which the depreſſion of the barrel had made betwixt the baſes of that barrel and the ſucker: and in ſuch caſes we ought not to wonder, if upon the return of the air the barrel and weight be not impelled up altogether to the ſame height they reſted at, when they were firſt ſuſpended in the receiver.

2. It agreed very well with our doctrine, that as the cylinder and weight began not to fall, till a great quantity of air had been pumped out of the receiver, ſo they did not begin to move upwards preſently upon the freedom, that was allowed the air to return into the receiver. For till it had continued a pretty while flowing in, there was not enough of it entered to reſtore by its preſſure the cylinder and the annexed weight to their former ſituation.

3. What has been delivered about our experiment may be confirmed by this variation, which we made of it; that having ſubſtituted a far heavier weight inſtead of that lately mentioned, the depreſſion of the barrel of the ſyringe ſucceeded two or three times one after another much ſooner than formerly, viz. about the ſixth, or, at moſt, the ſeventh exſuction.

EXPERIMENT XXXIII.

About the opening of a ſyringe, whoſe pipe was ſtopped in the exhauſted receiver, and by the help of it making the preſſure of the air lift up a conſiderable weight.

THOUGH the trial I am about to relate had not all the ſucceſs I deſired, yet perhaps it will not be impertinent to make mention of it, becauſe there is not any ſort of experiments, that is wont ſo much to perſuade the generality of ſpectators of the great force of the preſſure of the air, as thoſe, wherein they plainly ſee heavy and ſolid bodies made to aſcend, (upon the operation of the air on them) without ſeeing any other thing lift them up.

We took the often mentioned ſyringe, and having cloſed up the hole at the bottom with good cement, we tied to the barrel a hollow piece of iron, that ſerved us for a ſcale, into which we put divers weights one after another, trying from time to time, whether, when the ſucker was forceably drawn up, and held ſteadily in its higheſt ſtation, the weight tied to the barrel (which was held down, whilſt the ſucker was drawn up, and afterwards let go) would be conſiderably raiſed. And when we perceived, that the addition of half a pound, or a pound more, would make the weight too great to be ſo raiſed, we forbore to put in that increaſe of weight; and having tied the handle of the rammer to the turning-key, we conveyed the ſyringe, together with its clog, into a receiver, out of which a convenient quantity of air being pumped, we were thereby enabled eaſily to draw up the ſucker without the cylinder; after which having let in the air, the by-ſtanders concluded, that the weight was raiſed a little, which yet I would not have allowed, if we had not been able, by inclining the engine and the receiver, to make the ſyringe and weight a little to ſwing. But to make the effect more evident, I cauſed a two pound weight to be taken out, and then the receiver being ſomewhat exhauſted, and the air re-admitted, the clog, when all the air was come in, was ſwiftly raiſed, and as it were ſnatched up from the middle to the upper part of the ſuſpended rammer.

See Plate VI. Fig. 2.

It is no eaſy matter to meaſure, with any certainty and exactneſs by a ſyringe, the weight of an atmoſpherical pillar equal to it in diameter, eſpecially if there be any imperfection in the ſyringe, either becauſe the ſucker does not go cloſe enough, in which caſe it can ſcarce be ſtanch, or becauſe by its preſſure againſt the inſide of the barrel, which often happens if it be too cloſe, it hinders the ſucker and barrel from ſliding without reſiſtance by one another, and conſequently there is an undue reſiſtance made to the endeavour of the atmoſphere, to raiſe the barrel and weight. And therefore, though our ſyringe being, upon the account of ſome ill accident, leſs in order than it was in ſome of the foregoing experiments, I muſt not conclude, that a cylinder of the atmoſphere of the ſame wideneſs with it is equipollent to no greater a weight, than that which was taken up in our trial, yet we may ſafely conclude, that ſo ſlender a pillar of the atmoſphere is able to raiſe by a ſyringe at leaſt ſuch a weight, as in our experiment it actually lifted up, which amounted to above ſixteen pounds (avoirdupoiſe weight,) for it exceeded fifteen pounds and three quarters, beſides the weight of the ſyringe's barrel itſelf.

EXPERIMENT XXXIV.

Shewing, that the cauſe of the aſcenſion of liquors in ſyringes is to be derived from the preſſure of the air.

I SHALL not here trouble your lordſhip with what I have elſewhere propoſed about the explicating of ſuction: but as by the lately recited experiments (I mean the 31, 32, and 33) it has appeared, that it is to the preſſure of the external air that we ſhould aſcribe the difficulty of drawing up the ſucker of a ſyringe, when the pipe or the vent is ſtopped; ſo I ſhall now endeavour to ſhew, that the aſcenſion of liquors, which follow the ſucker when it is drawn up, the pipe being open, depends alſo upon the preſſure of the air, incumbent on that liquor.

If I had been furniſhed with very tall receivers, and ſuch other glaſſes, as I could have wiſhed, I had tried the following experiments with water, as well as quick-ſilver; but for want of thoſe accommodations, I was reduced to make my experiment with the latter only of thoſe liquors, which yet will, I hope, ſufficiently make out what was intended.

The First Tryal.

See Plate VII. Fig. 2. which tho' made primarily for the 39 experiment, may facilitate the conceiving of this.

We took a ſmall receiver, ſhaped almoſt like a pear, cut off horizontally at both ends, (being the ſame capped glaſs, that is elſewhere mentioned in the accounts of other experiments:) we alſo took the ſyringe formerly deſcribed, and, having faſtened on to it, with good cement, inſtead of its own braſs-pipe, a ſmall glaſs-pipe, of about half a foot in length, we put this ſyringe in at the narrower end of the receiver; to whoſe orifice was afterwards carefully cemented on the braſs-cap with the turning-key, whereto was tied by a ſtring the handle of the rammer. Then having conveniently placed upon the engine a very ſhort thick glaſs, ſhaped like a ſugar-loaf, (which was made uſe of, for want of a better) with a ſufficient quantity of quick-ſilver in it; we ſo placed the receiver over it, that the lower end of the pipe of the ſyringe reached almoſt to the bottom of this glaſs, and conſequently, was immerſed a pretty way beneath the ſurface of the quick-ſilver. We had alſo poured a little water in the upper part of the ſyringe, that no air might get in between the ſucker and the cylinder, notwithſtanding that, by ſome accident or other, the ſyringe was become ſomewhat leſs tight than before. And laſt of all, we cemented the receiver to the engine, after the uſual manner.

That which now remained, being to try the experiment it ſelf, in order to which all this had been done, the air was pumped out of the receiver, (and conſequently out of the little glaſs that held the mercury) and then the ſucker being warily drawn up, we could not ſee the quick-ſilver aſcend to follow it, though a little water, which, it ſeems, the outward air had thruſt in between the ſucker and the cylinder, was either raiſed, or ſtopped in the glaſs-pipe of the ſyringe, (whereof, yet much the greateſt part remained unfilled;) of which the reaſon, according to our hypotheſis, was manifeſt, namely, that the air being pumped out of the receiver, the little that remained had not ſtrength enough to preſs up ſo ponderous a liquor as the quick-ſilver into the pipe, (though even that little unexhauſted air might have ſpring enough left to raiſe a little water.) And ſince it appeared by this, that without the preſſure of the air, the quick-ſilver would not be elevated, we thought it ſeaſonable to ſhew, that, by the preſſure of the air, it would. Whereupon, the air being let ſlowly into the receiver, the mercury was quickly impelled up, at leaſt, to the top of the glaſs-pipe, (though, by reaſon of ſome unperceived leak, it was not long ſuſtained there.)

And, for further ſatisfaction, when the experiment was to be tried over again, we ordered it to be ſo made, that might plainly be obſerved, that though, when, the receiver not being yet exhauſted, the ſucker was drawn up but one inch, the mercury would be raiſed to the upper part of the glaſs-pipe of the ſyringe, yet after the exhauſting of the receiver, though the ſucker was drawn up twice as high, there appeared no aſcenſion of the mercury in the pipe, whoſe lower part only was darkened by the little glaſs, which contained that fluid metal.

Before I diſmiſs this experiment, I muſt, to make good a promiſe I made your lordſhip, acquaint you with a phænomenon, which does not a little confirm our doctrine, according to which, it was eaſy both to foreſee, and to explain it; the phænomenon was, that if, when the air was diligently pumped out of the receiver, the ſucker were endeavoured to be pulled up, it could not be ſo, without much difficulty and reſiſtance, ſuch as was formerly found when the vent of the ſyringe was ſtopped, of which in our hypotheſis the reaſon may be clearly this; that there being no common air in the receiver, to aſſiſt by its preſſure (whether immediate, or mediate) the raiſing of the ſucker, this could not be raiſed but by a force great enough to ſurmont the weight of the external air, or atmoſpherical pillar that leaned upon it. So that as the other phænomena of our experiments manifeſt, that the raiſing of liquors by a ſyringe, which is commonly aſcribed to attraction, depends upon the preſſure of the air; ſo by this phænomenon it appears, that the difficulty of opening a ſyringe, whoſe pipe is ſtopped, need not be attributed to ſuch a *fuga vacui* as vulgar philoſophers refer it to; ſince, in our caſe, the ſame difficulty was found, though the pipe were open, and the liquor it was immerſed in might have had free acceſs to the place deſerted by the ſucker.

The Second Trial.

Being a proſecution of the former attempt.

To vary as well as confirm the foregoing experiment, we cauſed the ſyringe to be tied faſt to a competently ponderous body, that might keep the cylinder unmov'd, when the ſucker ſhould be drawn up. We alſo cemented on to the vent or ſcrew at the bottom of the ſyringe, a pipe of glaſs of about two inches in length, (which ſhould have been longer, but that then there would not have been room in the receiver, for pulling up of the ſucker) and having placed the heavy body, whereto the ſyringe was tied, upon a pedeſtal of a convenient height, that the glaſs-pipe might be all ſeen beneath it, and a very low vial almoſt filled with quick-ſilver might be ſo placed underneath the pipe, that the ſtagnant mercury reached a good way above the immerſed orifice of the ſaid pipe. Theſe things being thus provided, and the handle of the ſyringe's rammer being tied with a ſtring to the turning-key, that belonged to the braſs-cover of the receiver, this veſſel was cemented on to the engine, and by it exhauſted after the uſual manner.

When this was done, we looked upon the ſyringe's glaſs-pipe abovementioned, and being able to ſee through it, (whereby we were certain, that it was not yet full of quick-ſilver) we did, by the ſtring, draw up the ſucker to a good height, but could not perceive the pipe to be filled with any ſucceeding mercury. Wherefore warily letting in ſome air, we quickly ſaw the mercury impelled to the very top of the pipe; and we concluded, from the quantity of quick-ſilver that was raiſed, that a pretty deal was alſo driven into the cavity of the cylinder.

N. B. I had once before ſeen the mercury aſcend into the pipe, upon the letting in of the air into the emptied receiver; but it ſeeming ſomewhat difficult to me to determine, whether the ſucker had been raiſed, becauſe there was no mark to guide my eſtimate by, I thought it might be ſuſpected, that in caſe the ſucker had not been raiſed, the aſcenſion of the quick-ſilver might have proceeded from hence, that the air contained in the glaſs-pipe, breaking out through the ſtagnant mercury upon the exhauſting of the receiver, the quick-ſilver might upon the return of the air into the receiver, be preſſed up into the place deſerted by the air, that broke out of the pipe. Wherefore we cauſed a ſtring to be tied about the rammer, as near as we could to the top of the cylinder, by which means, when the receiver was the next time exhauſted, we perceived, that by drawing up the ſucker, we had raiſed it about two inches, if not more, and yet we could not diſcern any mercury to follow it, (the glaſs-pipe ſtill continuing tranſparent) until we had let ſome air return into the receiver.

This experiment, joined with thoſe we have formerly related to have been tried with our ſyringe, may teach us, that if a ſyringe were made uſe of above the atmoſphere, neither the ſtopping of the pipe would hinder the eaſy drawing up of the ſucker, nor the drawing up of the ſucker, though the pipe were not ſtopped, would raiſe by ſuction the liquor, which the pipe was immerſed in.

Postscript.

Since the laſt recited experiment was made, and written, finding ſome of our inſtruments to be in better order than they were when that trial was made, we thought fit to endeavour by that which follows, to repair an omiſſion or two, that formerly we could not well avoid.

Having then cauſed ſuch a glaſs-pipe, as has been lately mentioned, to be well cemented on to the ſyringe, (whoſe ſucker did now move more eaſily, and yet fill the barrel more exactly, than before) I ordered (being to be abſent for a while myſelf) that the pipe ſhould be filled with ſpirit of wine tincted with cocheneal, that the liquor and its motions might be the better diſcerned, and that the pipe being filled, that air might be excluded, which would elſe be harboured in the pipe, which caution was omitted in the foregoing experiment. And this the perſon, to whom I committed it, affirmed to have been carefully done, though when he inverted the pipe thus filled into the reſt of the red liquor, that was put into a vial, he could not poſſibly do it ſo well, but that a bubble of air got into the pipe, and took up ſome (though but a little) room there. By that time I was called upon, to ſee the event of the trial, and could come to look upon it, the receiver was almoſt quite exhauſted: wherefore after I had made the pumping be continued a little longer, and perceived, that the tincted ſpirit was fallen down out of the pipe, and that which lay in the vial ſeemed almoſt to boil at the top, by reaſon of the emerſion of numerous bubbles, I cauſed the ſucker to be, by the help of the turning-key, drawn up by our eſtimate, about two inches and a half, notwithſtanding which, we could not perceive the ſpirit of wine to riſe in the pipe, though the pumping were before left off. For which reaſon, I ordered the air to be let in very leiſurely, upon which we could plainly ſee, that the red ſpirit was quickly driven up to the top of the pipe; and that it was ſo likewiſe into the cavity of the barrel, appeared, when the receiver was removed, by the ſmall quantity of liquor, that remained in the vial, and the plenty of it, which came out of the ſyringe.

See Plate VI. Fig. 1.

See Plate VII. Fig. 2. which tho' made primarily for the 39 experiment, may facilitate the conceiving of this.

N. B. That if I had not wanted dexterous artificers, to work according to a contrivance I had deſigned, I had attempted to imitate, by the help of the bare ſpring of the air, ſuch experiments, as in the lately recited trials were made to ſucceed, by the help of the preſſure exerciſed by the air upon the account of its weight.

EXPERIMENT XXXV.

Shewing, that upon the preſſure of the air depends the ſticking of cupping glaſſes to the fleſhy parts they are applied to.

It is ſufficiently known, that if the air within a cupping glaſs be rarefied by the flame of tow, flax, or the like, burned for a little while in it, and the glaſs be preſently clapped upon ſome fleſhy part of a man's body, there will quickly enſue a painful and viſible ſwelling of the part covered by the cupping glaſs.

It is alſo known, that this experiment is wont to be urged by the ſchools, as a clear proof of that abhorrence of a vacuum they aſcribe to nature; for, ſay they, the reaſon of this phænomenon is plainly, that the internal air of the cupping glaſs, præternaturally rarified by heat when the inſtrument is applied, that heat after a while ceaſing, the ſucceeding cold muſt again neceſſarily condenſe the air; and ſo this contracted air being no longer able to fill the whole ſpace it repleniſhed before, there would enſue a vacuum, if the fleſh covered by the cupping glaſs, or adjoining to it, did not ſwell into the cavity of it, to fill the place deſerted by the air.

Those moderns, that aſſert the weight of the atmoſphere, do thence ingeniouſly endeavour to deduce the phænomenon. And indeed, if to their hypotheſis about the airs weight the conſideration of its ſpring be added, it will be eaſy enough to explicate the phænomenon, by ſaying, that when the cupping glaſs is firſt ſet on, though much of the air it formerly contained were a little before expelled by the heat, yet the ſame heat, increaſing the preſſure of the remaining air, is the cauſe, that the abſence of the air driven out of the glaſs does not immediately occaſion ſo ſenſible a pain: but, when that adventitious agitation of the included air ceaſes, that air having now, becauſe of the paucity of its corpuſcles, but a weak ſpring, can no longer preſs upon the part covered by the cupping glaſs near ſo ſtrongly, as the outward air does by its weight preſs upon all the neighbouring parts of the fleſh: by which means, according to what we have more than once explicated already, ſome of the yielding fleſh, or other body covered by the ſkin, muſt be forceably thruſt into the cavity of the cupping glaſs, where there is leſs preſſure, than at the outſide of it. And the fibres and membranous parts being thus violently ſtretched, there muſt needs follow a ſenſible pain as well as tumour. Which tumour yet does not fill up the cupping glaſs, not only becauſe of the reſiſtance of the ſkin to be ſo far diſtended, but alſo, if the included air have not been much rarified, becauſe of the ſpring of the impriſoned air, which grows ſo much the ſtronger, by how much the ſwelling fleſh reduces the air into leſs room, as I have ſometimes tried, by applying a cupping-glaſs to quick-ſilver, or even to water, which will riſe in it but to a certain height.

But though by this, or ſome ſuch explication, the argument urged by the ſchools in favour of the *fuga vacui* may be ſufficiently enervated; yet it ſuited better with the deſign of this treatiſe, to propoſe ſome new experiment, to illuſtrate our hypotheſis; and though it ſeemed to be far more difficult to do it in reference to cupping glaſſes, than to other ſubjects, yet I pitched upon two different ways of experimenting; whoſe ſucceſs not diſappointing me, I ſhall now give your lordſhip an account of them.

See Plate VI. Fig. 3.

We took a glaſs of about one inch and a half in diameter, but a good deal longer than an ordinarily ſhaped cupping glaſs of that breadth would have been, that there might be the more room for the flame to burn in it, and rarify the air. We alſo provided a receiver ſhaped almoſt like a pear; this receiver was open at both ends; at the ſharper whereof there was but a ſmall orifice, but at the obtuſe end there roſe up a ſhort neck, whoſe orifice was wide enough to admit with eaſe the newly mentioned cupping glaſs, without touching the ſides of it, and we were not willing it ſhould be much larger, leſt it ſhould not be ſo exactly covered by the palm of the hand that ſhould be laid upon it, and leſt alſo the hand ſhould be broken or hurt by the too great weight of the atmoſphere, when the included air ſhould be withdrawn from under it.

These things being thus prepared, and the ſmaller orifice of the receiver being faſtened with cement to the engine, I cauſed the cupping glaſs to be faſtened, with the mouth upwards, to the palm of the hand of a youth, whom your lordſhip may remember to have ſeen with me, whoſe hand ſeemed framed by nature for this experiment, being broad, ſtrong, and very plump. And having pulled the glaſs, to try whether it ſtuck well on, I cauſed him to put it into the receiver, and lay his hand ſo upon the orifice lately mentioned, that it might ſerve for a cover to it, and hinder any air from getting in between them.

That, which we pretended was, that the receiver being but ſmall (that it might be quickly exhauſted, and ſo not put the youth to a long pain) upon an exſuction or two made with the pump of the air about the cupping glaſs, the remaining air ſhould have its preſſure ſo far weakened, as not to be able to ſupport the cupping-glaſs; eſpecially ſince if the air without the cupping-glaſs, but yet in the receiver, ſhould be more rarified by the removal of that which had been pumped out, than the air included in the cupping glaſs was by the precedent heat, this laſt mentioned air having a ſtronger ſpring (or tendency to expand itſelf) than the external air of the receiver, the glaſs muſt needs fall down, or rather be thruſt off, though, in caſe there had been no air at all left in the cavity of the cupping glaſs, the air in the receiver would by its preſſure ſuſtain a far greater weight.

The event of our trial agreed very well with our conjecture. For upon the firſt ſuck the cupping glaſs fell off, the weight of the atmoſphere preſſing ſo hard upon the young man's hand, that, though he be more than ordinary ſtrong, he complained he could very hardly take it off the glaſs it was almoſt thruſt into, and, a while after, that his hand was very ſore. But this laſt inconvenience became not ſo quickly very ſenſible, but that we had time to repeat our experiment, by faſtening the cupping glaſs more ſtrongly than before; ſo that he complained, that it drew in his hand very forceably; and though that part be not wont to be fleſhy, yet the tumour occaſioned by the cupping glaſs was manifeſt enough to the eye: but as before, ſo now, at the very firſt turning of the ſtop-cock, to let out the air of the receiver, the cupping glaſs fell off.

EXPERIMENT XXXVI.

About the making, without heat, a cupping glaſs to lift up a great weight.

THE other experiment I lately told your lordſhip we had made, to illuſtrate our doctrine about the cauſe of the ſticking of applied cupping glaſſes, was tried after the following manner.

We took the braſs-hoop or ring, mentioned in the fifth and ſixth experiments, and covered it with a bladder, which was wetted to make it the more limber, and was ſo tied on to it (which was eaſy to do) that the bottom of the bladder covered the upper orifice of the hoop, and was ſtretched, though not ſtrongly, upon it, almoſt like the membrane, that makes the head of a drum; and the neck of the bladder was tied with a ſtring near the middle of the lower orifice of the hoop, and in this lower part of the bladder we made two or three ſmall holes for the air to paſs in and out at. Then having placed at the bottom of the often mentioned capped receiver a thick piece of wood, that had a hole in it, to receive the neck of the bladder, we ſo placed the covered hoop upon this piece of wood, that the upper part of the bladder lay parallel to the horizon. This done, we ſuſpended, at the turning-key belonging to the cap of our receiver, a blind head, as chemiſts call it, of glaſs, which for want of a true cupping glaſs we were fain to ſubſtitute, and which indeed was not very unlike one either for ſhape or ſize; and to the upper part of this glaſs we faſtened a large ring of metal, the better to depreſs it, and make it lean ſtrongly on the bladder.

Plate VI. Fig. 4.

THESE things being thus made ready, and the receiver cemented on to the engine, we did by the help of the turning-key let down the cupping glaſs (for ſo we ſhall hereafter call it) till it came almoſt to touch the level ſuperficies of the bladder; and when the receiver was as far exhauſted as we thought fit, but not near as far as it might have been, we let down the cupping glaſs a little lower, ſo that it leaned upon the bladder, and touched it with all the parts of its orifice: ſo that the cupping glaſs with the ſubjacent bladder was become an internal receiver, if I may ſo call it, whoſe air was conſiderably expanded, and conſequently weakened as to its ſpring. All this being done, we warily let the air into the receiver, and thereby the air, that did ſurround the cupping glaſs, which we juſt now called the internal rereiver, having now a ſtronger preſſure than the air in the cupping glaſs could reſiſt; the bladder, on which the cupping glaſs reſted, was, as we looked for, thruſt up a pretty way into the cavity of the glaſs, in which it made a conſpicuous tumor; and was made to ſtick ſo cloſe to the orifice of it, that one would have thought, that the bladder had been violently drawn in, as the ſkin is wont to be in the ordinary applications of cupping glaſſes.

AND becauſe we took notice, that though this glaſs were not capacious, for it ſcarce held a pint of water, yet the orifice of it was not very narrow, being in diameter an inch and ⅘, we thought fit in repeating the experiment to add ſomething, that ſeemed odd enough, and was fit to manifeſt, that cupping glaſſes may, without heat, by the bare preſſure of the external air, be more ſtrongly faſtened, than for ought we know they are by the help of flame. Having then reiterated the former experiment with this only variation, that we exhauſted the receiver further than before, we took out the cupping glaſs and the bladder, which together with the included braſs-hoop was hanging at it; and then having tied the glaſs to the hook of a good ſtatera, and tied a large ſcale to the neck of the bladder, we put in by degrees weights into the ſcale, till we had loaded it enough to force off the bladder from the glaſs; which happened not till the whole weight, that tended to draw down the bladder, amounted to 35 pound, if not better, of ſixteen ounces in the pound. Nor did we doubt, but that the preſſure of the atmoſphere would in our experiment have kept up a much greater weight, if we had, before we let in the outward air, diligently exhauſted the receiver; which we had purpoſely forborn to do, for fear the too diſproportionate preſſure of the external air ſhould break the bladder: which puts me in mind of adding, upon the by, that as more weight was put into the ſcale, the bladder (ſtretched more and more by the weight on one ſide, and the air on the other) appeared to ſwell higher in the cavity of the glaſs.

Plate VI. Fig. 5.

EXPERIMENT XXXVII.

Shewing, that bellows, whoſe noſe is very well ſtopped, will open of themſelves, when the preſſure of the external air is taken off.

IT is wont by the peripateticks and others to be made a great argument for the *fuga vacui*, which they attribute to nature, that if the noſe of a pair of bellows be well ſtopped, one cannot open them by raiſing the upper board from the lower. But of this another reaſon may be eaſily aſſigned, without determining whether there be a vacuum or no, namely the weight and preſſure of the air: for when the noſe of a pair of bellows, that are tight enough, is well ſtopped, no air being able to inſinuate itſelf upon the disjoining of the boards into the cavity made by that disjunction, this cannot be effected, but by ſuch a force, as is almoſt able, (I ſay almoſt, becauſe ordinary bellows cannot be ſo well ſhut, but that there will remain ſome air in them, whoſe ſpring will facilitate the opening of them) to raiſe an atmoſpherical pillar, whoſe baſis ſhall be the upper board, which is commonly ſo large, that a leſs force may ſerve to break common bellows, than to raiſe ſo great a weight: but if they were made ſtrong enough, and there were applied a ſufficient force to lift ſo great a weight, as the newly mentioned pillar of the atmoſphere, the ſides might be disjoined, how cloſe and ſtanch ſoever the inſtrument were made.

THUS far one may argue upon the bare principle of the weight of the air, but taking in the ſpring of it too, I thought one might proceed ſo much further, that I ventured to foretel divers ingenious men, that if the preſſure of the ambient air were taken off, not only it would be eaſy to open the bellows in ſpite of there being carefully ſtopped at the noſe, but that they would fly open as it were of their own accord, without the application of any external force at all. And it was partly to juſtify his prediction, as well as to make a trial, I thought more conſiderable, that we made the following experiment.

WE cauſed, then, to be made a pair of bellows, differing from ordinary ones in theſe par-

See Plate VI. Fig. 6.

ticulars. Firſt, that the boards were circular, (and ſo without handles) and of about ſix inches in diameter: 2. That there was no clack or valve: 3. That the noſe was but an inch long, or leſs, being to be lengthened if occaſion required with a pipe: 4. That the leather, which was not ſpared, that the inſtrument might be the more capacious, was not horny or very ſtiff, but limber. The reaſon of the firſt and third diverſity was, that the bellows might be capable to be conveyed into our receiver; (for which purpoſe alſo, if there had appeared need, the noſe might have been made in the uppermoſt of the two boards:) the reaſon of the ſecond variation was, that the inſtrument might be the more ſtaunch: and of the fourth, that the baſes of the bellows might, as in organ-bellows, be clapped cloſer together, and harbour leſs air in the wrinkles and cavity. So that when the bellows were opened to their full extent, by drawing up the upper baſis at a button purpoſely made in the midſt of it, the bellows looked like a cylinder of ſixteen or eighteen inches high; upon which reſemblance I take the liberty to call both the boards, as geometricians do both the circular parts of a cylinder, baſes.

But though theſe were made by an artificer, otherwiſe dexterous, yet it not being his trade to make bellows, nor any other man's in the town I then was in, he could not make them ſo tight, but that in ſpite of our oiling the leather, and choaking the ſeams with good cement, there was ſome little and unperceived hole or cranny, whereby ſome air had paſſage when the noſe was accurately ſtopped: but this was not ſo conſiderable, but that if we drew up the upper baſis from the lower, the external air would on all ſides preſs the leather inwards, and ſo make the ſhape of the inſtrument very far from being ſo cylindrical, as it would be if the noſe were left open.

See Plate VI. Fig. 7.

Wherefore concluding, that notwithſtanding this imperfection the bellows would ſerve, though not for both the experiments I deſigned, yet for one of them, we carefully ſtopped the noſe, after we had approached the baſes to one another, and conveying them into a large receiver, it quickly appeared, when the pump was ſet on work, that at every exſuction of the incumbent air, the air harboured in the folds of the leather, and the reſt of the little cavity that could not but be left between the baſes, made the upper of thoſe baſes manifeſtly riſe, though its weight (becauſe of the thickneſs and ſolidity of the wood) would ſoon after depreſs it again, either by driving out ſome of the air at ſome place, where the inſtrument was not ſufficiently tight, or by making it as it were ſtrained through the leather itſelf; and if the pump were agitated ſomewhat faſter than ordinary, the expanſion of the internal air would be greater than could be rendered quite ineffectual by ſo ſmall a leak, and the upper part of the bellows would be ſoon raiſed to a conſiderable height, as would appear more evidently, if we haſtily let in the external air, upon whoſe ingreſs the baſes would be clapped together, and the upper of them a good way depreſſed. So that the imperfection of the bellows made the experiment rather more than leſs concluding; for ſince there was no external force applied to open them, if notwithſtanding that ſome of the included air could get out of them, yet the ſpring of the internal air was ſtrong enough to open the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly ſtanch.

EXPERIMENT XXXVIII.

About an attempt to examine the motions and ſenſibility of the Carteſian Materia ſubtilis, *or the* Æther, *with a pair of bellows, made of a bladder, in the exhauſted receiver.*

I WILL not now diſcuſs the controverſy betwixt ſome of the modern atomiſts, and the Carteſians; the former of whom think, that betwixt the earth and the ſtars, and betwixt theſe themſelves there are vaſt tracts of ſpace, that are empty, ſave where the beams of light do paſs through them; and the latter of whom tell us, that the intervals betwixt the ſtars and planets, among which the earth may perhaps be reckoned, are perfectly filled, but by a matter far ſubtiler than our air, which ſome call celeſtial, and others æther. I ſhall not, I ſay, engage in this controverſy; but thus much ſeems evident, that if there be ſuch a celeſtial matter, it muſt make up far the greateſt part of the univerſe known to us. For the interſtellar part of the world, if I may ſo ſtile it, bears ſo very great a proportion to the globes, and their atmoſpheres too, if other ſtars have any, as well as the earth, that it is almoſt incomparably greater in reſpect of them, than all our atmoſphere is in reſpect of the clouds, not to make the compariſon between the ſea and the fiſhes that ſwim in it.

Wherefore I thought it might very well deſerve a heedful enquiry, whether we can by ſenſible experiments (for I hear what has been attemped by ſpeculative arguments) diſcover any thing about the exiſtence, or the qualifications of this ſo vaſt æther: and I hoped our curioſity might be ſomewhat aſſiſted by our engine, if I could manage in it ſuch a pair of bellows as I deſigned. For I propoſed to myſelf to faſten a convenient weight to the upper baſis, and clog the lower with another, great enough to keep it horizontal and immoveable; that when by the help of the turning key, frequently above-mentioned, the upper baſis ſhould be raiſed to its full height, the cavity of the bellows might be brought to its full dimenſions. This done, I intended to exhauſt the receiver, and conſequently the thus opened bellows with more than ordinary diligence, that ſo both the receiver and they might be carefully freed from air. After which I purpoſed to let go the upper baſe of the bellows, that being haſtily depreſſed by the incumbent weight, it might ſpeedily enough fall down to the lower baſis, and by ſo much, and ſo quickly leſſening the cavity, might expel thence the matter (if any where) before contained in it, and that (if it could by this way be done) at the whole of a ſlender pipe, faſtened either near the bottom of the bellows, or in the upper baſis; againſt, or over the orifice, of which pipe there was to be placed at a convenient diſtance, either a feather, or (if that ſhould prove too light) the ſail of a little windmill made of cards, or ſome other light body, and fit to be put into motion by the impulſe of any matter, that ſhould be forced out of the pipe.

By this means it ſeemed not improbable, that ſome ſuch diſcovery might be made, as would not be altogether uſeleſs in our enquiry. For if notwithſtanding, the abſence of the air, it ſhould appear by the effects, that a ſtream of other matter, capable to ſet viſible bodies a moving, ſhould iſſue out at the pipe of the compreſſed bellows; it would alſo appear, that there may be a much ſubtiller body than common air, and as yet unobſerved by the vacuiſts, or (their adverſaries) the ſchools, that may even copiouſly be found in places deſerted by the air; and that it is not ſafe to conclude from the abſence of the air in our receivers, and in the upper part of thoſe tubes, where the Torricellian experiment is made, that there is no other body left but an abſolute vacuity, or (as the atomiſts call it) a *vacuum coacervatum*. But if on the other ſide, there ſhould appear no motion at all to be produced, ſo much as in the feather, it ſeemed, that the vacuiſts might plauſibly argue, that either the cavity of the bellows was abſolutely empty, or elſe that it would be very difficult to prove by any ſenſible experiment that it was full; and, if by any other way of probation it be demonſtrable, that it was repleniſhed with æther, we, that have not yet declared for any party, may by our experiment be taught to have no confident expectations of eaſily making it ſenſible by mechanical experiments; and may alſo be informed, that it is really ſo ſubtle and yielding a matter, that does not either eaſily impel ſuch light bodies as even feathers, or ſenſibly reſiſt, as does the air itſelf, the motions of other bodies through it, and is able without reſiſtance to make its paſſage through the pores of wood and leather, and alſo of cloſer bodies, which we find not that the air doth in its natural or wonted ſtate penetrate.

To illuſtrate this laſt clauſe I ſhall add, that to make the trial more accurate, I waved the uſe of other bellows, (eſpecially not having ſuch as I deſired,) and cauſed a pair of ſmall bellows to be made with a bladder, as a body, which ſome of our former experiments have evinced to be of ſo cloſe a texture, that air will rather break it than paſs through it: and that the bladder might no where loſe its entireneſs by ſeams, we glued on the two baſes, the one to the bottom, and the other to the oppoſite part of it, ſo that the neck came out at a hole purpoſely made for it; in the upper baſis, and into the neck it was eaſy to inſert what pipe we thought fit, binding the neck very cloſe to it on the outſide. We had likewiſe thoughts to have another pair of tight bellows made with a very light clack in the lower baſis, that by haſtily drawing up the other baſis, when the receiver and bellows were very carefully exhauſted, we might ſee by the reſt, as the lifting up of the clack, whether the ſubtle matter, that was expelled by the upper baſis in its aſcent, would, according to the modern doctrine of the circle made by moving bodies, be impelled up or not.

We alſo thought of placing the little pipe of the bladder-bellows (if I may ſo call them) beneath the ſurface of water exquiſitely freed from air, that we might ſee, whether upon the depreſſion of the bellows by the incumbent weight, when the receiver was carefully exhauſted, there would be any thing expelled at the pipe, that would produce bubbles in the liquor, wherein its orifice was immerſed.

To bring now our conjectures to ſome trial, we put into a capped receiver the bladder accommodated as before is mentioned; and though we could have wiſhed it had been ſomewhat larger, becauſe it contained but between half a pint and a pint, yet in regard it was fine and limber, and otherwiſe fit for our turn, we reſolved to try how it would do; and to depreſs the upper baſis of theſe little bellows the more eaſily and uniformly, we covered the round piece of paſtboard, that made the upper baſis, with a pewter-plate, (with a hole in it for the neck of the bladder;) which nevertheleſs upon trial proved not ponderous enough; whereby we were obliged to aſſiſt it by laying on it a weight of lead. And to ſecure the above-mentioned feather, (which had a ſlender and flexible ſtem, and was left broad at one end, and faſtened by cement at the other, ſo as to ſtand with its broad end at a convenient diſtance juſt over the orifice of the pipe,) from being blown aſide to either hand, we made it to move in a perpendicular ſlit in a piece of paſtboard, that was faſtened to one part of the upper baſis, as that which the feather was glued to was to another part. Theſe things being thus provided, the pump was ſet awork; and as the ambient air was from time to time withdrawn, ſo the air in the bladder expanded itſelf ſo ſtrongly, as to lift up the metalline weight, and yet in part to ſally out at the little glaſs-pipe of our bellows, as appeared by its blowing up the feather, and keeping it ſuſpended till the ſpring of the air in the bladder was too far weakened to continue to do as it had done. In the mean time we did now and then, by the help of a ſtring faſtened to the turning key, and the upper baſis of the bellows, let down that baſis a little, to obſerve how upon its ſinking the blaſt againſt the feather would decreaſe, as the receiver was further and further exhauſted. And when we judged it to be ſufficiently freed from air, we then let down the weight, but could not perceive, that by ſhutting of the bellows, the feather was at all blown up, as it had been wont to be, though the upper baſis were more than uſually depreſſed. And yet it ſeems ſomewhat odd, that when, for curioſity, in order to a further trial, the weight was drawn up again, as the upper baſis was raiſed from the lower, the ſides of the bladder were ſenſibly (though not very much) preſſed, or drawn inwards.

See Plate VI. Fig. 7.

inwards. The bellows being thus opened, we let down the upper baſis again, but could not perceive, that any blaſt was produced; for though the feather, that lay juſt over and near the orifice of the little glaſs-pipe, had ſome motion, yet this ſeemed plainly to be but a ſhaking and almoſt vibrating motion (to the right and left hand,) which it was put into by the upper baſis, which the ſtring kept from a ſmooth and uniform deſcent; but not to proceed from any blaſt iſſuing out of the cavity of the bladder. And for further ſatisfaction, we cauſed ſome air to be let into the receiver, becauſe there was a poſſibility, that unawares to us the ſlender pipe might by ſome accident be choaked: but though upon the return of the air into the receiver, the baſes of the bellows were preſt cloſer together, yet it ſeemed, that, according to our expectation, ſome little air got through the pipe into the cavity of the bladder: for when we began to withdraw again the air we had let into the receiver, the bladder began to ſwell again, and upon our letting down the weight, to blow up and keep up the feather, as had been done before the receiver had been ſo well exhauſted. What conjecture the opening and ſhutting of our little bellows, more than once or twice, without producing any blaſt ſenſible by the raiſing of the feather, gave ſome of the by-ſtanders, may be eaſily gueſſed by the preamble of this experiment; but whilſt I was endeavouring to proſecute it for my own farther information, a miſchance, that befel the inſtrument, kept me from giving my ſelf the deſired ſatisfaction.

EXPERIMENT XXXIX.

About a further attempt to proſecute the inquiry propoſed in the foregoing Experiment.

CONSIDERING with my ſelf, that by the help of ſome contrivances not difficult, a ſyringe might be made to ſerve, as far as our preſent occaſion required, inſtead of a pair of bellows; I thought it would not be improper to try a differing, and, in ſome regards, a better way to proſecute an attempt, which ſeemed to me, to deſerve our curioſity.

See Plate VII. Fig. 2.

I cauſed then to be made, for the formerly mentioned ſyringe, inſtead of its ſtreight pipe, a crooked one; whoſe ſhorter leg was parallel to the longer. And this pipe was for greater cloſeneſs, after it was ſcrewed on carefully, faſtened with cement to the barrel; and becauſe the braſs-pipe could ſcarce be made ſmall enough, we cauſed a ſhort, and very ſlender pipe of glaſs to be put into the orifice of the ſhorter leg, and diligently faſtened to it with cloſe cement. Then we cauſed the ſucker (by the help of oyl, water, and moving it up and down) to be made to go as ſmoothly as might be, without leſſening the ſtanchneſs of the ſyringe. After this, there was faſtened to the handle of the rammer a weight, made in the form of a ring, or hoop, which by reaſon of its figure, might be ſuſpended from the newly mentioned handle of the rammer, and hang looſe on the outſide of the cylinder, and which both by its figure, and its weight, might evenly and ſwiftly enough depreſs the ſucker, when that being drawn up the weight ſhould be let go. This ſyringe thus furniſhed was faſtened to a broad and heavy pedeſtal, to keep it in its vertical poſture, and to hinder it from tottering, notwithſtanding the weight that clogged it. And beſides all theſe things, there was taken a feather, which was about two inches long, and of which there was left at the end a piece about the breadth of a man's thumb-nail, (the reſt on either ſide of the ſlender ſtalk, if I may ſo call it, being ſtript off) to cover the hole of the ſlender glaſs-pipe of the ſyringe; for which purpoſe the other extreme of it was ſo faſtened with cement to the lower part of the ſyringe, (or to its pedeſtal,) that the broad end of the feather was placed (as the other feather was in the foregoing experiment) juſt over the little orifice of the glaſs, at ſuch a convenient diſtance, that when the ſucker was a little (though but very little) drawn up and let go again, the weight would depreſs it faſt enough to blow up the broad part of the feather, as high as was permitted by the reſiſtance of the ſtalk, (and that was a good way) the ſpring of which would preſently reſtore the whole feather to its former poſition.

ALL theſe things being done, and the handle of the rammer being tied to the turning-key of a capped receiver, the ſyringe and its pedeſtal were incloſed in a capacious receiver, (for none but ſuch an one could contain them, and give ſcope for the rammer's motions) and the pump being ſet on work, we did, after ſome quantity of air was drawn out, raiſe the ſucker a little by the help of the turning-key, and then turning the ſame key the contrary way, we ſuffered the weight to depreſs the ſucker, that we might ſee at what rate the feather would be blown up; and finding, that it was impelled forceably enough, we cauſed the pumping to be ſo continued, that a pretty many pauſes were made, during each of which we raiſed and depreſſed the ſucker as before, and had the opportunity to obſerve, that as the receiver was more and more exhauſted of the air, ſo the feather was leſs and leſs briſkly driven up, till at length, when the receiver was well emptied, the uſual elevations and depreſſions of the ſucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully my ſelf, but I made one, whom I had often employed about pneumatical experiments, to watch attentively, whilſt I drew up, and let down the ſucker, but he affirmed, that he could not diſcern the leaſt beginning of aſcenſion in the feather. And indeed to both of us it ſeemed, that the little and inconſiderable motion, that was ſometimes (not always) to be diſcerned in the feather, proceeded not from any thing, that iſſued out of the pipe, but from ſome little ſhake, which it was difficult not to give the ſyringe and pedeſtal, by the raiſing and depreſſing of the ſucker.

AND that, which made our phænomenon the more conſiderable, was, that the weight, that carried down the ſucker being ſtill the ſame, and the motions of the turning-key being eaſy to be made equal at ſeveral times, there ſeemed no reaſon to ſuſpect, that contingencies did much (if at all) favour the ſucceſs; but there happened a thing, which did manifeſtly enough diſfavour it. For I remember, that before the ſyringe was put into the receiver, when we were trying how the weight would depreſs it, and it was thought, that though the weight were conveniently ſhaped, yet it was a little of the leaſt, I would not alter it, but foretold, that when the air in the cavity of the ſyringe (that now reſiſted the quickneſs of its deſcent, becauſe ſo much air could not eaſily and nimbly get out at ſo ſmall a pipe) ſhould be exhauſted with the other air of the receiver, the elevated ſucker would fall down more eaſily, which he, that was employed to manage the ſyringe whilſt I watched the feather, affirmed himſelf afterwards to obſerve very evidently. So that when the receiver was exhauſted, if there had been in the cavity of the ſyringe a matter as fit as air to make a wind of, the blaſt ought to have been greater, becauſe the celerity, that the ſucker was depreſſed with, was ſo.

After we had long enough tried in vain to raiſe the feather, I ordered ſome air to be let into the receiver; and though when the admitted air was but very little, the motions of the ſucker had ſcarce, if at all, any ſenſible operation upon the feather, yet when the quantity of air began to be ſomewhat conſiderable, the feather began to be a little moved upwards, and ſo by letting in air not all at once, but more and more from time to time, and by moving the ſucker up and down in the intervals of thoſe times of admiſſion, we had the opportunity to obſerve, that as the receiver had more air in it, the feather would be more briſkly blown up.

But not content with a ſingle trial of an experiment of this conſequence, we cauſed the receiver to be again exhauſted, and proſecuted the trial with the like ſucceſs as before, only this one circumſtance, that we added for confirmation, may be fit to be here taken notice of. Having, after the receiver was exhauſted, drawn up and let fall the ſucker divers times ineffectually; though hitherto we had not uſually raiſed it any higher at a time, than we could by one turn of the hand, both becauſe we could not ſo conveniently raiſe it higher by the hand alone, and becauſe we thought it unneceſſary, ſince that height ſufficed to make the air briſkly toſs up the feather; yet *ex abundanti* we now took an inſtrument, that was pretty long, and fit ſo to take hold on the turning-key, that we could eaſily raiſe the ſucker between two and three inches, by our eſtimate, at a time, and nimbly depreſs it again; and for all this, which would much have increaſed the blaſt, if there had been a matter fit for it in the cavity of the ſyringe, we could not ſenſibly blow up the feather, till we had let a little air into the receiver.

To be able to make an eſtimate of the quantity of air pumped out, or let in, when the feather was ſtrongly or faintly, or not at all raiſed by the fall of the ſucker; we took off the receiver, and conveyed a gage into it, but though for a while we made ſome uſe of our gage, yet a miſchance befalling it before the operation was quite ended, I ſhall forbear to add any thing concerning that trial, and proceed to ſay ſomething of another attempt, wherein, though I foreſaw and met with ſuch difficulties, as kept me from doing altogether what I deſired, yet the ſucceſs being almoſt as good as could be expected, I ſhall venture to acquaint your lordſhip with the trial, which was this.

See Plate VII. Fig. 3.

Instead of the hitherto employed pipe of braſs, there was well faſtened, with cement, to the ſyringe, a pipe of glaſs, whoſe figure differed from that of the other in this particular, that the ſhorter, or remoter, leg of our new pipe, after it had for a while been carried parallel to the other leg, was bent off ſo, that above an inch and a half of it tended downwards, that the orifice of it might be immerſed into water, contained in a ſmall open jar. The deſign of which contrivance was, that when the receiver ſhould be well exhauſted, we might, according to what I told your lordſhip was at firſt deſigned, try whether by the raiſing and depreſſing of the ſucker any ſuch matter would be driven out at the noſe of the pipe, as would produce bubbles in the incumbent water, which air (though highly rarefied, perhaps to ſome hundreds of times beyond its wonted dimenſions) is capable of doing. And I chuſe to employ rather water than quick-ſilver, becauſe though by uſing the latter, I might hope to be leſs troubled with bubbles, yet the ponderouſneſs and opacity of it ſeemed to outweigh that convenience.

I need not tell your lordſhip, that in other reſpects this experiment was made like the former; ſo that I ſhall mention only its peculiarities, which were, that as the air was pumped out of the receiver, that in the glaſs pipe made its way through the water in bubbles; and a little air having once by a ſmall leak got in, and forced ſome of the water out of the jar into the pipe, when the receiver was again well emptied, both that water, and even the little quantity of ſtagnant water, that was contained in the immerſed part of the pipe, produced ſo many bubbles of ſeveral ſizes, as quite diſturbed our obſervations. Wherefore we let alone the receiver, exhauſted as it was, for ſix or ſeven hours, to give the water time to be freed from air; and then cauſing what air might have ſtolen in to be again pumped out, till we had perceived by the gage, that the receiver was well exhauſted, we cauſed the ſucker of the ſyringe; to be raiſed and depreſſed divers times; and though even then a bubble would now and then make our obſervations troubleſome, and leſs certain, yet it ſeemed to us, that when we were not thus confounded, we ſometimes obſerved, that the elevation and fall of the ſucker, though reiterated, did not drive out at the pipe any thing, that made any diſcernable bubbles in the incumbent water; for though there would appear now and then ſome ſmall bubbles on the ſurface of the water, yet

I could not perceive, that the matter that made them iſſued out at the pipe; and ſome of them manifeſtly proceeded from aerial particles, till then lurking in the water, as I concluded from the place and time of their riſing. But this non-eruption of bubbles at the noſe of the pipe was not that, which gave me the moſt of ſatisfaction. For at length both I and another had the opportunity to obſerve the water in the immerſed part of the pipe, which was very ſlender, to be about an inch higher than the reſt of the ſtagnant water, and to continue at that height or place in the pipe, though the ſucker were divers times together raiſed and depreſſed, by gueſs, between two and three inches at a time. Which ſeemed to argue, either that there was a vacuum in the cavity of the ſyringe, or elſe, that, if it were full of æther, that body was ſo ſubtle, that the impulſe it received from the falling ſucker would not make it diſplace a very little thread (perhaps not exceeding a grain in weight) of water, that was in the ſlender pipe, though it appeared by the bubbles, that ſometimes diſcloſed themſelves in the water, after the receiver had been exhauſted, that far more water would be diſplaced and carried up by a ſmall bubble, conſiſting of ſuch rarified air, that according to my eſtimate, the aerial particles of it did not, before the pump was begun to be ſet on work, take up in the water a five-hundredth part of the quantity of a pins-head.

But whilſt we were conſidering what to do further in our trial, a little air, that ſtrained in at ſome ſmall undiſcoverable leak, drove the water into the emptied part of the pipe, and put an end for that time to our trial, which had been too toilſome to invite us then to reiterate it.

I had indeed thoughts of proſecuting the enquiry, by dropping from the top of the exhauſted receiver light bodies conveniently ſhaped, to be turned round, or otherwiſe put out of their ſimpleſt motion of deſcent, if they met with any reſiſtance in their fall; and by making ſuch bodies move horizontally and otherwiſe in the receiver, as would probably diſcover, whether they were aſſiſted by the medium. And other contrivances and ways I had in my thoughts, whereby to proſecute our enquiry; but wanting time for other experiments, I could not ſpare ſo much as was neceſſary to exhauſt large receivers ſo diligently, as ſuch nice trials would exact; and therefore I reſolved to deſiſt, till I had more leiſure than I then had, or have ſince been maſter of.

In the interim, thus much we ſeem to have already diſcovered by our paſt trials, that if when our veſſels are very diligently freed from air, they are full of æther, that æther is ſuch a body, as will not be made ſenſibly to move a light feather by ſuch an impulſe as would make the air manifeſtly move it, not only whilſt it is no thinner than common air, but when it is very highly rarefied, (which, if I miſtake not, it was in our experiment ſo much, as to be brought to take up above an hundred times more room than before).

And one thing more we gained by the trial made with water, namely, a clear confirmation of what I have delivered in the 34th experiment, about the cauſe of the ſuction that is made by ſyringes; for your Lordſhip may remember, that at the cloſe of the experiment we have all this while been reciting, I obſerved, that when the external air was ſo very well withdrawn, the pulling up of the ſucker would not make the ſtagnant water, that the pipe of the ſyringe was immerſed in, to aſcend one inch, or ſo much as the tenth part of it.

EXPERIMENT XL.

About the falling, in the exhauſted receiver, of a light body, fitted to have its motion viſibly varied by a ſmall reſiſtance of the air.

PARTLY to try, whether in the ſpace deſerted by the air, drawn out of our receivers, there would be any thing more fit to reſiſt the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpoſe to be mentioned by and by, we made the following trials.

We took a receiver, which, though leſs tall than we would have had, was the longeſt we could procure: and that we might be able, not ſo properly to let down, as to let fall a body in it, we ſo faſtened a ſmall pair of tobacco-tongs to the inſide of the receiver's braſs-cover, that by moving the turning-key, we might by a ſtring tied to one part of them open the tongs, which elſe their own ſpring would keep ſhut. This being done, the next thing was to provide a body, which would not fall down like a ſtone, or another dead weight through the air, but would in the manner of its deſcent ſhew, that its motion was ſomewhat reſiſted by the air. Wherefore that we might have a body, that would be turned about horizontally, as it were, in its fall, we thought fit to join croſs-wiſe four broad and light feathers (each about an inch long) at their quills with a little cement, into which we alſo ſtuck perpendicularly a ſmall label of paper, about an 8th of an inch in breadth, and ſomewhat more in height, by which the tongues might take hold of our light inſtrument without touching the cement, which elſe might ſtick to them.

See Plate VII. Fig. 4.

By the help of this ſmall piece of paper, the little inſtrument, of which it made a part, was ſo taken hold of by the tongs, that it hung as horizontal as ſuch a thing could well be placed: and then the receiver being cemented on to the engine, the pump was diligently plied, till it appeared by a gage, which had been conveyed in, that the receiver had been carefully exhauſted: laſtly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little inſtrument let fall, which, though in the air it had made ſome turns in its deſcent from the ſame height, which it now fell from, yet now it deſcended like a dead weight, without being perceived by any of us to make ſo much as one turn, or a part of it: notwithſtanding which I did, for greater ſecurity, cauſe the receiver to be taken off, and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhauſted, they made ſome turns in their deſcent, as they alſo did being a ſecond time let fall after the ſame manner.

BUT when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-ſtanders could perceive any thing of turning in the deſcent of the feathers; and yet for further ſecurity we let them fall twice more in the unexhauſted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhauſted receiver, they fell after the ſame manner as they had done formerly, when the air, that would by its reſiſtance have turned them round, was removed out of their way.

N.B. 1. THOUGH, as I intimated above, the glaſs, wherein this experiment was made, were nothing near ſo tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. ONE, that had had more leiſure and conveniency, might have made a more commodious inſtrument than that we made uſe of: for being accidentally viſited by that ſagacious mathematician, Dr. *Wren*, and ſpeaking to him of this matter, he was pleaſed with great dexterity as well as readineſs to make me a little inſtrument of paper, on which, when it was let fall, the reſiſtance of the air had ſo manifeſt an operation, that I ſhould have made uſe of it in our experiment, had it not been caſually loſt, when the ingenious maker was gone out of theſe parts.

3. THOUGH I have but briefly related our having ſo ordered the matter, that we could conveniently let fall a body in the receiver when very well exhauſted; yet to contrive and put in practice what was neceſſary to perform this, was not ſo very eaſy, and it would be difficult to deſcribe it circumſtantially without very many words; for which reaſon I forbear an account, that would prove too tedious to us both.

4. WHAT has been hitherto related, was done in proſecution of but one of the two deſigns I aimed at in the foregoing contrivance, by which I intended, if I could have procured a receiver tall enough, to try whether bodies (ſome very light, and ſome heavier) being let fall, when the air was very diligently pumped out, would not deſcend ſomewhat faſter than if the receiver were full of air. But though I had provided a pendulum, that vibrated quarters of ſeconds, yet the glaſs being no higher than it was, the deſcent even of our feathers took up ſo little time, that even this pendulum was of no uſe; only it ſeemed to all of us, that were preſent at making the above recited trials, that when the feathers were let fall at ſuch times as the air, that would have turned them round in their deſcent, was removed, they came to the bottom ſenſibly ſooner than at other times. But when we ſhall have opportunity to repeat the experiment in taller glaſſes, and to make ſome variation of it, I hope to be able to give your lordſhip a fuller ſatisfaction about this particular. And in the mean while I ſhall forbear to examine, whether the air might ſomewhat retard the deſcent of the feathers upon ſome other account, or meerly upon that of its being a medium not quite devoid of gravity.

ANNOTATIONS.

1. BUT here I muſt be ſo ſincere as to inform your lordſhip, that this fortieth experiment ſeemed not to prove ſo much as did the foregoing made with the ſyringe: for being ſuſpicious, that, to make the feathered body abovementioned turn in its fall, there would need a reſiſtance not altogether inconſiderable, I cauſed the experiment to be repeated, when the receiver was by our eſtimate, which was not made at random neither, little or nothing more than half exhauſted, and yet the remaining air was too far rarefied to make the falling body manifeſtly turn.

2. AND yet perchance it would have happened otherwiſe, if the receiver had been tall enough; which though I had not then leiſure and conveniency to make it, yet it will not be amiſs to let your lordſhip know by what means we did, that it might be ſomewhat fit to make the recited experiment and ſome others, bring it to the height it had, which did conſiderably exceed that of the talleſt glaſs we could then procure.

TO lengthen our receiver therefore, we thought fit to try, whether we could not cloſe enough faſten to the bottom of it with very good cement a cylindrical pipe of laton, whoſe upper orifice ſhould have near the ſame breadth with the bottom of the glaſs. And though this contrivance ſeemed liable to a couple of not mean difficulties; the one, that the laton being every where bended, and in ſome places neceſſary to be ſoddered, it would be very hard, as indeed we found it, to avoid ſome ſmall cracks and leaks; and the other, that if the metalline pipe were wide enough, ſo great and heavy a pillar of the atmoſphere would come to bear againſt it, as to preſs it inwards, if not alſo to break it; yet we hoped we ſhould be able to obviate both of theſe inconveniencies. Againſt the firſt of which our remedy was, to coat over very carefully the whole pipe with the ſame cloſe cement, wherewith we faſtened it to the glaſs receiver. And againſt the ſecond, we provided a little frame, conſiſting of divers ſmall iron bars faſtened together; which frame (though it were not too wide to go into the cylinder of laton, yet it) was wide enough to be ſo near it on the inſide, that (though the weight of the atmoſphere ſhould, as we feared, preſs the laton ſo as to make it yield inward, yet) it could make it bend no further than the iron-frame would permit; which was not far enough to ſpoil either the receiver or the experiment. And this not unpleaſant phænomenon would ſomewhat ſurpriſe unaccuſtomed ſpectators, that when after the receiver had been very well exhauſted, the external air was permitted to return, there would be heard during ſome time, from the metalline part of the receiver, divers ſounds briſk enough, which would make an odd cracking noiſe proceeding from the lattin-plate, which having been forceably, though but ſlowly, bent inwards by the predominant preſſure of the atmoſphere, was now aſſiſted by the preſſure of the returning air, to regain its former figure. And as I thought not fit to omit this circumſtance, becauſe it confirms the practicableneſs of the remedy propoſed againſt the ſecond inconvenience; ſo I thought fit to mention this way of enlarging and heightening receivers, becauſe what we have related ſeems to give grounds of hoping, that this contrivance may be made good uſe of in divers other trials, and particularly in attempts to make receivers capacious enough to contain larger animals, and perhaps even a boy, or a man. In order to ſome of which purpoſes we endeavoured to get an improvement made of our metalline cylinder by additional contrivances; but could not, where we then were, get artificers, that would perform what was directed.

EXPERIMENT XLI.

About the propagation of ſounds in the exhauſted receiver.

TO make ſome further obſervation than is mentioned in the * publiſhed experiments, about the production and conveying of ſounds in a glaſs whence the air is drawn out, we employed a contrivance, of which, becauſe we make uſe of it in divers other experiments, it will be requiſite to give your lordſhip here ſome ſhort deſcription.

See Plate VIII. Fig. 1.

WE cauſed to be made at the turner's acylinder of box, or the like cloſe and firm wood, and of a length ſuitable to that of the receiver it was to be employed in. Out of the lower baſis of this cylinder (which might be about an inch and a half in diameter) there came a ſmaller cylinder or axle-tree, not a quarter ſo thick as the other, and leſs than an inch long: this was turned very true, that it might move to and fro; or, as the tradeſmen call it, ride, very ſmoothly in a little ferrule or ring of braſs, that was by the ſame turner made for it in the midſt of the fixed trencher, (as we call a piece of ſolid wood, ſhaped like a mill-ſtone) being four or five inches, more or leſs, according to the wideneſs of the receiver, in breadth, and between one and two in thickneſs; and in a large and round groove, or gutter, purpoſely made in the lower part of this trencher, I cauſed as much lead as would fill it up, to be placed and faſtened, that it might keep the trencher from being eaſily moved out of its place or poſture, and in the upper part of this trencher it was intended, that holes ſhould be made at ſuch places as ſhould be thought fit, to place bodies at ſeveral diſtances as occaſion ſhould require. The upper baſis of the cylinder had alſo coming out of the midſt of it another axletree, but wider than than the former, that, into a cavity made in it, it might receive the lower end of the turning-key divers times already mentioned, to which it was to be faſtened by a ſlender peg of braſs, thruſt through two correſpondent holes, the one made in the key, and the other in the newly mentioned ſocket, if I may ſo call it, of the axle-tree. Beſides all which, there were divers horizontal perforations bored here and there in the pillar itſelf, to which this axis belonged, which pillar we ſhall, to avoid ambiguity, call the verticle cylinder. The general uſe of this contrivance (whoſe other parts need not to be mentioned before the experiments where they are imployed) is, that the end of the turning-key being put into the ſocket, and the lower axis of the verticle cylinder into the trencher, by the motion of the key a body faſtened at one of the holes to the cylinder may be approached to, or removed from, or made to rub or ſtrike againſt another body faſtened in a convenient poſture to the upper part of the trencher.

See the Figure laſt referred to.

To come now to our trial about ſounds, we cauſed a hand-bell (whoſe handle and clapper were taken away) to be faſtened to a ſtrong wire, that, one end of the wire being made faſt in the trencher, the other end, which was purpoſely bent downwards, took hold of the bell. In another hole, made in the circumference of the ſame trencher, was wedged in (with a wooden peg) a ſteel-ſpring, to whoſe upper part was tied a gad of iron or ſteel, leſs than an inch long, but of a pretty thickneſs. The length of this ſpring was ſuch, as to make the upper part of the hammer (if I may ſo call the piece of iron) of the ſame height with the bell, and the diſtance of the ſpring from the bell was ſuch, that when it was forced back the other way, it might at its return make the hammer ſtrike briſkly upon the outſide of the bell.

THE trencher being thus furniſhed and placed in a capped receiver, (as you know, for brevity ſake, we uſe to call one, that is fitted with one or other of the braſs covers, often mentioned already,) the air was diligently pumped out; and then, by the help of the turning-key, the vertical cylinder was made to go round, by which means as often as either of a couple of ſtiff wires, or ſmall pegs, that were faſtened at right angles into holes, made not far from the bottom of the cylinder, paſſed (under the bell, and) by the lately mentioned ſpring; they forceably did in their paſſage bend it from the bell, by which means, as ſoon as the wire was gone by, and the ſpring ceaſed to be preſſed, it would fly back with violence, enough to make the hammer give a ſmart ſtroak upon the bell. And by this means we could both continue the experiment at diſcretion, and make the percuſſions more equally ſtrong, than it would otherwiſe have been eaſy to do.

THE event of our trial was; that, when the receiver was well emptied, it ſometimes ſeemed doubtful, eſpecially to ſome of the by-ſtanders, whether any ſound were produced or no; but to me for the moſt part it ſeemed, that after much attention I heard a ſound, that I could but juſt hear; and yet, which is odd, methought

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* Page the 105, 106.

thought it had ſomewhat of the nature of ſhrilneſs in it, but ſeemed (which is not ſtrange) to come from a good way off. Whether the often turning of the cylindrical key kept the receiver from being ſo ſtaunch as elſe it would have been, upon which ſcore ſome little air might inſinuate itſelf, I ſhall not poſitively determine: but to diſcover what intereſt the preſence or the abſence of the air might have in the loudneſs or lowneſs of the ſound, I cauſed the air to be let into the receiver, not all at once, but at ſeveral times, with competent intervals between them; by which expedient it was eaſy to obſerve, that the vertical cylinder being ſtill made to go round, when a little air was let in, the ſtroak of the hammer upon the bell (that before could now and then not be heard, and for the moſt part be but very ſcarcely heard) began to be eaſily heard. And when a little more air was let in, the ſound grew more and more audible, and ſo increaſed, until the receiver was again repleniſhed with air; though even then (that we omit not that phænomenon) the ſound was obſerved to be much leſs loud, than when the receiver was not interpoſed between the bell and the ear.

And whereas in the already publiſhed phyſico-mechanical experiments, I acquainted your lordſhip with what I obſerved about the ſound of an ordinary watch in the exhauſted receiver, I ſhall now add, that that experiment was repeated not long ſince, with the addition of ſuſpending in the receiver a watch, with a good alarum, which was purpoſely ſo ſet, that it might, before it ſhould begin to ring, give us time to cement on the receiver very carefully, exhauſt it very diligently, and ſettle ourſelves in a ſilent and attentive poſture. And to make this experiment in ſome reſpect more accurate than the others we made of ſounds, we ſecured ourſelves againſt any leaking at the top, by imploying a receiver, that was made all of one piece of glaſs, (and conſequently had no cover cemented on to it,) being furniſhed only within (when it was firſt blown) with a glaſs-knob or button, to which a ſtring might be tied. And becauſe it might be ſuſpected, that if the watch were ſuſpended by its own ſilver chain, the tremulous motion of its ſounding bell might be propagated by that metalline chain to the upper part of the glaſs; to obviate this as well as we could, we hung the watch, not by its chain, but a very ſlender thread, whoſe upper end was faſtened to the newly mentioned glaſs-button.

These things being done, and the air being carefully pumped out, we ſilently expected the time, when the alarum ſhould begin to ring, which it was eaſy to know by the help of our other watches; but not hearing any noiſe ſo ſoon as we expected, it would perhaps have been doubted, whether the watch continued going, if for prevention we had not ordered the matter ſo, that we could diſcern it did not ſtand ſtill. Wherefore I deſired an ingenious gentleman to hold his ear juſt over the button, at which the watch was ſuſpended, and to hold it alſo very near to the receiver; upon which he told us, that he could perceive, and but juſt perceive ſomething of ſound, that ſeemed to come from far; though neither we that liſtened very attentively near other parts of the receiver, nor he, if his ears were no more advantaged in point of poſition than ours, were ſatisfied, that we heard the watch at all. Wherefore ordering ſome air to be let in, we did by the help of attention begin to hear the alarum; whoſe ſound was odd enough, and, by returning the ſtop cock to keep any more air from getting in, we kept the ſound thus low for a pretty while, after which a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, the by-ſtanders could plainly hear the noiſe of the yet continuing alarum at a conſiderable diſtance from the receiver.

From what has hitherto been related, we may learn what is to be thought of what is delivered by the learned *Merſennus,* in that book of his Harmonicks, where he makes this to be the firſt propoſition. *Sonus à campanis, vel altis corporibus non ſolum producitur in illo vacuo (quicquid tandem illud ſit,) quod ſit in tubis hydrargyro plenis, poſteaque depletis, ſed etiam idem acumen, quod in aere libero vel clauſo penitus obſervatur & auditur.* For the proof of which aſſertion, not long after, he ſpeaks thus: *porro variis tubis, quorum extremis lagenæ vitreæ adglutinantur, obſervari campanas in illo vacuo appenſas propriiſque malleis percuſſas idem penitus acumen retinere, quod in aere libero habent: atque ſoni magnitudinem ei ſono, qui ſit in aere quem tubus clauſus includit, nihil cedere.* But though our experiments ſufficiently manifeſt, that the preſence or abſence of the common air is of no ſmall importance as to the conveying of ſounds, and that the interpoſition of glaſs may ſenſibly weaken them; yet ſo diligent and faithful a writer as *Merſennus* deſerves to be favourably treated: and therefore I ſhall repreſent on his behalf, that what he ſays may well enough have been true, as far as could be gathered from the trials he made. For firſt, it is no eaſy matter, eſpecially for thoſe that have not peculiar and very cloſe cements, to keep the air quite out for any conſiderable time in veſſels conſiſting of divers pieces, ſuch as he appears to have made uſe of. And next, the bigneſs of the bell in reference to the capacity of the exhauſted glaſs, and the thickneſs of the glaſs, and the manner whereby the bell was faſtened to the inſide of the glaſs, and the hammer or clapper was made to ſtrike, may much vary the effect of the trial, for reaſons eaſy to be gathered out of the paſt diſcourſe, and therefore not needful to be here inſiſted on. And upon this account we choſe to make our experiment, with ſounds that ſhould not be ſtrong or loud, and to produce them after ſuch a manner, as that as little ſhaking as could be might be given by the ſounding body to the glaſs it was included in. The propoſal made by the ſame *Merſennus,* to have thoſe, that have induſtry enough, try whether a bag-pipe will be made to afford the ſame ſound as in the open air, in ſuch veſſels as he uſed for his bells, though he ſeems to think it would ſucceed, is that which your lordſhip will not, I preſume, ſollicit me to make trial of, if you remember what is related in the almoſt immediately foregoing experiments, ſhewing, that we could make nothing come out of the cavity of a pair of bellows, that had force enough to blow away a feather, when that cavity was freed from air; as the bagpipe would be by the ſame operation, that empties the glaſs that contains it, or elſe the ſound would not be made in ſuch a vacuum as the ſcope of the experiment requires.

IF I had had conveniency, I would have made ſome trials by conveying a ſmall ſtringed inſtrument (perhaps ſome ſuch as they commonly call a kit) exactly tuned, into a large receiver, and then upon briſkly ſtriking the ſtring of a bigger inſtrument, (tuned, as they ſpeak, to an uniſon to (or with) that of the ſmaller inſtrument) I ſhould have taken notice, whether the ſound would have been ſo uniformly propagated, notwithſtanding the interpoſition of the glaſs receiver, as ſenſibly to ſhake the included ſtring; in order to the diſcerning of which, a bended piece of ſtraw, or feather, or ſome ſuch light body, was to be horſed upon the ſtring to be ſhaken. I alſo intended, in caſe the ſtring were made to move, to make the like trial after the receiver was diligently exhauſted. And laſtly I deſigned to try, whether two uniſon ſtrings of the ſame inſtruments, or of a couple to be placed in the ſame receiver, would, when the air (which is the uſual medium of ſounds) was well pumped out, yet maintain ſuch a ſympathy, as it is called, that upon the motion of the one, the other would alſo be made to ſtir: which trials may be varied, by employing for the external inſtrument another inſtead of a ſtringed one.

AND becauſe contraries, as is vulgarly noted, ſerve to illuſtrate each other, I thought to ſubjoin, to the trials above related about the propagation of ſounds in a thinner medium than the air, ſome obſervations about the conveyance of them through that thicker medium, water: but having unluckily miſlaid my notes upon that ſubject, I cannot at preſent acquaint your lordſhip with what I intended, but muſt defer the doing it, till I ſhall have recovered them.

EXPERIMENT XLII.

About the breaking of a glaſs-drop in an exhauſted receiver.

YOU know, that among the cauſes, that have been propoſed of the ſtrange flying of a glaſs-drop into a multitude of pieces, when the ſlender ſtem of it comes to be broken off, one of the leaſt improbable was taken from the preſſure of the air: as if that within the porous, (and as it were honey-combed) inſide of the glaſs, being highly rarefied when the drop of melted glaſs fell into the water at its firſt formation, it was forced to continue in that preternatural ſtate of expanſion by the hardneſs and cloſeneſs of the external caſe of glaſs, that incloſed the pith-like part (if I may ſo call it;) ſo that upon the breaking off a part of this ſolid caſe at the ſtem, the external air gaining acceſs, and finding in the ſpungy part very little reſiſtance from the highly rarefied and conſequently weakened air included there, ruſhes in with ſuch violence, as to ſhiver the glaſs-drop into a multitude of pieces.

I ſhall not now trouble your lordſhip with the mention of what may be alledged to queſtion this hypotheſis, eſpecially if it be compared with that accurate account of the phænomena of ſuch glaſs-drops, which was ſometimes ſince preſented to the ſociety by that great ornament of it, Sir *Robert Moray*. But I ſhall only ſay in this place, that when I conſidered, that if the diſſilition of the glaſs would ſucceed, when the air was pumped out of it, it would be hard to aſcribe that effect to the eruption of the external air; I thought fit to try what would happen, if a glaſs-drop were broken in our exhauſted receiver. And accordingly did, though not without ſome difficulty, ſo order the matter, that the blunter part of the glaſs-drop was faſtened to a ſtable body (conveyed into the receiver,) and the crooked ſtem was tied to one end of a ſtring, whoſe other end was faſtened to the turning-key; by which means, when the air had been diligently pumped out, the ſtem was (by ſhortning the ſtring) broken off, and the glaſs-drop was ſhattered into a thouſand pieces.

This experiment was long after repeated with the like ſucceſs; and having at that time no gage to try, how far the air had been drawn out, we let the external air impell up the water out of the pump into the receiver, and thereby found, that that veſſel had not been negligently exhauſted.

EXPERIMENT XLIII.

About the production of Light in the exhauſted Receiver.

I Preſume, I need not put your lordſhip in mind, that divers attempts were made to try, whether either a flame, or kindled coals would be made to continue for ſometime burning in our receiver. But thoſe trials making it evident, that it would be either impoſſible, or very difficult to produce any durable light, without the preſence of the air, by the burning of bodies; I thought it not amiſs, conſidering the nobleneſs of light, to make trial, whether it might be otherwiſe produced in our exhauſted receiver; ſince whether or no the attempts ſhould prove ſucceſsful, the event would probably be inſtructive. For as it is the property of light, when it is produced, to be diſcoverable by it ſelf; ſo in ſuch a trial as we intended, it would teach ſomething concerning light, to find that the abſence of the air would or would not hinder it from being produced. In proſecution of this deſign, knowing that hard ſugar, being nimbly ſcraped with a knife, will afford a ſparkling light, ſo that now and then one would think that ſparks of fire fly from it; we cauſed a good lump of hard loaf-ſugar to be conveniently and firmly placed in the cavity of our capped receiver, and to the vertical cylinder forementioned we cauſed to be faſtened ſome pieces of a ſteel-ſpring, which being not very thick, might in their paſſage along the ſugar, grate, or rub forceably againſt it, and then the receiver being diligently exhauſted in the night-time, and in a dark room, the vertical cylinder (whoſe lower axis was inſerted into the often mentioned trencher) was made for a pretty while to move round by the help of the turning-key, managed by a hand ſteady and ſtrong enough. By which means the irons, that came out of the vertical cylinder, making in their paſſage vigorous impreſſions upon the ſugar, that ſtood ſomewhat in their way, there were manifeſtly produced a good number of little flaſhes, and ſometimes too, though not frequently, there ſeemed to be ſtruck off little ſparks of fire.

The contrivance here mentioned may be conceived by conſidering the figure belonging to the 41 experiment.

EXPERIMENT XLIV.

About the production of a kind of halo and colours in the the exhauſted receiver.

WE took a large inverted cucurbite for a receiver, which being ſo well wiped both within and without as to be very clear, allowed me to obſerve, and to make others do ſo too, that when the pump began to be ſet a work, if I cauſed a pretty large candle to be held on the other ſide of the glaſs, upon the turning of the ſtop-cock to let the air out of the receiver into the cylinder, the glaſs would ſeem to be full of fumes, and there would appear about the flame of the candle, ſeen through them, a kind of halo, that at firſt commonly was between blue and green, and after ſome ſucks would be of a reddiſh or orange colour, and both very vivid. The production of this meteor, if I may ſo call it, was, according to my conjecture, made on ſome ſuch ſcore as this. That the cement being ſomewhat ſoft and new, as is convenient for this experiment, abounds with turpentine; and having a little, as well to faſten on the receiver, as for the other purpoſe, applied to it a hot iron, whereby the cement was both ſoftened and heated, it ſeemed rational to expect, that upon the withdrawing of the air in the receiver, the aerial particles in the cement, freed from their former preſſure, would extricate themſelves, and with the looſer ſteams of the turpentine, and perhaps of the bees-wax, would with a kind of exploſion expand themſelves in the receiver, and by their interpoſition between the light and the eye exhibit thoſe delightful colours we had ſeen. To confirm which, I afterwards found, that by watchfully obſerving it, I could plainly enough perceive the colouring ſteams, juſt upon the turning of the ſtopcock, to fly up from the cement towards the top of the glaſs; and if we continued pumping, the receiver would grow clearer, and the colours more dilute, till we had occaſion to put on the receiver, and heat the cement afreſh: of which the reaſon might be, partly that the aerial and volatile particles of the upper part of the cement did in that tract of time ſpend themſelves more and more; and partly, becauſe the agitation they received from the heat communicated by the iron did continually decay. Not to mention, that when the receiver is more exhauſted, the want of air makes it more difficult for ſteams to be ſupported, and, as it were, ſwim up and down in it.

For farther confirmation, I cauſed ſome cement to be put into a ſmall crucible, warm enough to melt it; and conveying this into a clear receiver of a convenient ſhape and ſize, I cauſed the pump to be ſet a work; whereupon it appeared manifeſtly enough, that upon the opening of the ſtop-cock to let out the air, the ſteams would copiouſly be thrown about from the crucible into the capacity of the receiver, and would, after having a little played there, fall down again. But in theſe apparitions the vividneſs, and ſometimes the kind of the exhibited colours ſeemed much to depend upon divers circumſtances, ſuch as the degrees of heat, the bigneſs and ſhape of the receiver, the quantity of air that yet remained unpumped out, and the nature of the cement itſelf; which laſt particular I the rather mention, becauſe, though I were hindered from doing it, I had thoughts to try a ſuſpicion I had, that by varying the materials expoſed to this kind of operation, ſome pretty variety might be made in the phænomena of the experiment.

Whether or no the apparition or whiteneſs, or light, that we ſometimes happened to take notice of divers years ago, and have mentioned in the already publiſhed part of our Phyſico-mechanical Experiments, may be partly (though not entirely) referred to ſome of the cements I then employed, differing from thoſe I now uſe moſt, and to the unheeded temper of thoſe cements, as to warmth, and degrees of ſoftneſs, is a doubt, that further obſervation may poſſibly enable us to determine.

EXPERIMENT XLV.

About the production of heat by attrition in the exhauſted receiver.

THE opinion, that aſcribes the incaleſcence of ſolid bodies, ſtruck or rubbed hard againſt one another, to the attrition or vehement agitation of the intercepted air, is famous and received enough to ſeem worthy of a particular examination. But I confeſs to your lordſhip, that it was not any thing relating to this opinion, that chiefly induced me to make the experiment I am now about to give an account of; for I thought it might be uſeful to more purpoſes than one, to be able to produce by attrition a ſomewhat durable heat, even in our exhauſted receiver: and therefore, though it were to foreſee, that it would prove no eaſy taſk, yet we thought fit to attempt it ſpight of the difficulties met with at our firſt trials. In what way and with what ſucceſs we afterwards made this attempt, I now proceed to relate.

Cross the ſtable trencher, formerly mentioned, there was faſtened a pretty ſtrong ſpring of ſteel or iron, ſhaped almoſt like the lath of a croſs-bow; and to the midſt of this ſpring was ſtrongly faſtened on the outſide, a round piece of braſs hollowed almoſt like a concave burning-glaſs, or one of thoſe tools, wherein

See Plate IV. Fig. 3.

wherein they uſe to grind eye-glaſſes for teleſcopes. To this piece of braſs, which was not conſiderably thick, nor above two inches diameter, was fitted a convex piece of the ſame metal, almoſt like a gage for a tool to grind glaſſes in, which had belonging to it a ſquare handle, whereinto as into a ſocket was inſerted a ſquare piece of wood, proceeding from the baſis of a ſquare wooden pillar, which we made uſe of on this occaſion inſtead of our vertical cylinder. By the help of another piece of wood, coming from the other baſis of the ſame pillar, the turning-key was joined to this pillar, which was made of ſuch a length, that when the turning-key was forceably kept down as low as the braſs cover, it was a part of, would permit, the convex piece of metal lately deſcribed did depreſs the concave piece a pretty way, notwithſtanding a vigorous reſiſtance of the ſubjacent ſpring.

BESIDES theſe things, a little fine powder of emery was put between the convex and concave pieces of braſs, to make them more congruous, and facilitate the motion that was to be made; and there was faſtened to the upper part of the turning-key a good wimble, without which we preſumed the turning of the key would not produce a ſufficient motion; in order to the making of which, it was, after the firſt trial, judged requiſite to have a ſtrong man, that was uſed to exerciſe his hands and arms in mechanical labours, upon which account we ſent for a certain lockſmith, that was a luſty and dexterous fellow.

ALL things, that were thought neceſſary, being thus in readineſs, and a mercurial gage being conveyed into the receiver, we cauſed the air to be diligently pumped out; and then the ſmith was ordered to turn the wimble, and to continue to lean a little on it, that he might be ſure to keep the turning-key from being at all lifted up by the former mentioned ſpring.

WHILST this man with much nimbleneſs and ſtrength was moving the wimble, I watched the gage, to obſerve, whether the agitation of the ſtop-cock, and conſequently the engine, did not prejudice the experiment; and for greater caution I cauſed the pump to be almoſt all the while kept at work, though that ſeemed not ſo neceſſary.

WHEN the turner of the wimble was almoſt out of breath, we let in for haſte the air at the cover of the receiver, by lifting up the turning-key; and nimbly removing the receiver, we felt the pieces of braſs, betwixt whom the attrition had been made, and, as we expected, found both of them very ſenſibly warm.

BUT being willing to confirm the experiment by a ſecond trial, which we hoped might, after the experience taught us by the firſt, be ſomewhat better performed, we cauſed the ſmith, after he had well refreſhed himſelf with reſt and drink, to lay hold of the wimble again, when the gage made it appear, that the receiver was well exhauſted, ſo that by further pumping the quick-ſilver ſeemed not to be further depreſſed. And in this ſecond trial the nimble ſmith played his part ſo well, the pump in the mean while not being neglected, that when we did as before haſtily let in the air, and take out the bodies, that had been rubbed againſt one another, they were both of them, eſpecially the uppermoſt, ſo hot, that I could not endure to hold my hand on either of them, and they did for a conſiderable time retain a not inconſiderable degree of warmth.

THE ſame day I cauſed to be made at the turners two bodies of wood, for ſize and ſhape like thoſe of braſs we had juſt before employed; the upper of theſe was of hard oak, the other of beach, ſuch a difference between woods, to be heated by mutual attrition, being thought to be an advantageous circumſtance. But though the wimble was ſwiftly turned as before, and that by the ſame perſon, nevertheleſs the wood ſeemed not to me, (for all the by-ſtanders were not of my opinion) to have manifeſtly acquired any warmth; and yet that there had been a conſiderable attrition, appeared by the great poliſh, which part of the wood had evidently acquired, which made me ſuſpect, that though the wood ſeemed dry enough, yet it might not really be ſo, notwithſtanding the contrary was affirmed to me. But not being willing to ſit down with a ſingle trial, I cauſed the experiment to be repeated with more obſtinacy than before; the effect of which was, that the wood, eſpecially the upper piece of it, was brought to a warmth unqueſtionably ſenſible.

EXPERIMENT XLVI.

About the ſlacking of quick-lime in the exhauſted receiver.

THE ſeveral ſcopes I aimed at in making the following trial are not neceſſary to be here particularly taken notice of. But one of them may be gueſſed at by the ſubſequence of this experiment to that immediately foregoing, and the phænomena of it may be mentioned in this epiſtle, upon the account of their being exhibited by our engine.

WE took in an evaporating glaſs a convenient quantity of water, and having conveyed it into a receiver, and well drawn out the air, we let down into it by the turning-key a lump of ſtrong lime, about the bigneſs of a pippin; and obſerved not, that at the firſt immerſion, nor for ſome while after, there appeared any conſiderable number of bubbles; but within about ¼ of an hour, as I gueſſed it, the lime began (the pump having been and being ſtill plied from time to time) to ſlack with much violence, and with bubbles wonderfully great, that appeared at each new exſuction, ſo that the inſide of the receiver, though pretty large, was at length lined with lime-water, and a great part of the mixture did from time to time overflow the veſſel, that had purpoſely been but little filled; nor did any thing but our wearineſs put a period to the bubbling of the mixture, whoſe heat was ſenſible, even on the outſide of the receiver, and which continued conſiderably hot in the evaporating glaſs for ¼ of an hour, as I conjectured, after the receiver was removed.

Note. That the lime employed about this experiment, was of a very good and ſtrong kind, made of hard ſtones, and not ſuch lime, made of chalk, as is commonly uſed at *London*, which probably would not have been ſtrong enough to have afforded us the ſame phænomenon.

EXPERIMENT XLVII.

About an attempt made to meaſure the force of the ſpring of included air, and examine a conjecture about the difference of its ſtrength in unequally broad mouth'd veſſels.

THOUGH ſeveral of the foregoing trials have ſufficiently manifeſted, that the ſpring of the air in its natural or wonted ſtate hath a force very conſiderable, and indeed much greater than men ſeem to have hitherto believed, yet I could not hope by any of theſe experiments to determine by any known weight, how great that force is, ſo as to conclude, that it is equivalent to ſuch a weight, as ſo many pounds, ounces, &c. and to no more. Wherefore among the uſes I had deſigned to make of our ſyringe formerly often mentioned, it was one, to try, if by the help of that inſtrument, we could determine ſomewhat near (for no more was to be expected) how much weight a cylinder of uncompreſſed air included in it, and conſequently of the ſame diameter with the cavity of the barrel, would be able to ſuſtain, or alſo to lift up.

In order to this trial, 1. we provided a ſtable pedeſtal, or frame, wherein the ſyringe might be kept firm, and erected. Next, we alſo provided a weight of lead ſhaped like our braſs-hoop, or ring, formerly deſcribed, that (Exper. V.) by the advantage of its figure it might be made to hang down by ſtrings from the top of the handle of the rammer, and ſo preſs evenly enough on all ſides, without making the upper part of the inſtrument top-heavy. 2. We took care to leave, between the bottom of the ſyringe, which was firmly cloſed with ſtrong cement) and that part of it where the ſucker was, a convenient quantity of air, to expand itſelf, and lift up the weight, when the air external to that included air ſhould be pumped out of the receiver. And laſtly, the handle of the rammer (from which the annular weight lately ſpoken of depended) was ſo faſtened to the turning-key of the cover of the receiver, that the weight might not compreſs the air included in the ſyringe, but leave it in its natural ſtate or wonted laxity, till the air were withdrawn from the receiver.

But notwithſtanding all this, when we actually tried the experiment, that happened, which I feared. For though by this method the included air would well enough lift up a weight of ſeven or eight pound, yet when the rammer came to be clogged with ſo conſiderable a weight, as my ſcope in making the experiment required, the inſtrument proved not ſo ſtanch, but that it was eaſier for ſome particles of air to force themſelves a paſſage, and get away between the ſucker and the inſide of the barrel, than to heave up ſo great a weight. And yet I have thought fit to relate the experiment thus particularly, becauſe, if an exact ſyringe can be procured, which I fear will be very difficult, but do not think impoſſible, this ſeems to be one of the likelieſt and leaſt exceptionable ways I know, of meaſuring the force of the air's ſpring.

But deſpairing to get ſuch a ſyringe, as I deſired, in the place where I then was, I bethought my ſelf of another way, by which I hoped to be able, though not to arrive at an exact knowledge of the full force of the air's ſpring, yet, at leaſt to approach nearer it than I have been able to do by the help of the ſyringe. For this purpoſe, conſidering with my ſelf, that if a convenient quantity of air were included in a fine ſmall bladder, the ſides of it would hinder the air from getting away, and the limberneſs of them would permit the air to accommodate it ſelf, and the bladder to the figure of a cylindrical veſſel, into which it might be put:

Wherefore with much ado I procured to be made by a perſon exerciſed in turning a couple of hollow cylinders, whoſe ſides were of a ſufficient thickneſs, that they might reſiſt the preſſure of the air to be impriſoned in them, and of ſuch differing breadths, that the firſt had but one inch in diameter, and the ſecond two; their depths being alſo unequal, that the one might receive a much larger bladder than the other.

With the leſſer of theſe, which was very carefully turned, I made a diligent trial, whoſe circumſtances I cannot now acquaint your lordſhip with, the paper, wherein they were amply recorded, having been with other notes belonging to this continuation unluckily loſt: but the moſt conſiderable things in the event, were, that it was very difficult to procure a bladder ſmall and fine enough for that little cylinder; and that one, which at length we procured, would not continue ſtanch for many trials, but would after a while part with a little air in the well exhauſted receiver, when it was clogged with the utmoſt weight it could ſuſtain: but whilſt it continued ſtanch we made one fair trial with it, from whence we concluded, that a cylinder of air of but an inch in diameter, and leſs than two inches in length, was able to raiſe viſibly, though but a little, a weight of above ten pounds (I ſpeak of avoirdupoize weights, where a pound contains ſixteen ounces.) The manner of making this experiment, and the cautions uſed in judging of it, your lordſhip may learn by the recital of the ſubſequent trial; my notes about which were not ſo unfortunate as thoſe that concerned the former.

Into a hollow cylinder of wood of four inches in depth, and two in diameter, furniſhed with a broad and ſolid bottom or pedeſtal, to make it ſtand the firmer, was put a lamb's or ſheep's bladder very ſtrongly tied at the neck, on which was put a wooden plug, marked with ink where the edge of the cylinder was contiguous to it; this plug being loaden with (See Plate VIII. Fig. 2 and 4.)

weights, amounting to 35 pound, (the uppermoſt of which weights was faſtened to the turning-key, to keep it upright, and to help to raiſe it at firſt) the receiver was exhauſted, till the mark appeared very manifeſtly above the brim of the cylinder; and then, though the ſtring were by turning the key quite ſlackened, yet the mark on the plug continued very viſible: and when ſo much air was let into the receiver, as made the weight depreſs the plug quite beneath the mark, upon the repumping out of the air, the weight was without the help of any turning-key lifted up, and by degrees all the mark on the plug was raiſed about ½ above the edge of the cylinder.

WHEREFORE we ſubſtituted for a ſeven pound weight one that was eſtimated at 14, (for then we had not a ballance ſtrong enough to weigh it with,) and uſing the ſame bladder we repeated the experiment, only having a care to ſupport a little the uppermoſt weight by the turning-key, till the bladder had attained its expanſion; and then the weight being gently let go, depreſſed not the plug ſo low, but that we could yet ſee the mark on it, (which yet was all we could do,) though that part of the plug, where the mark was, were manifeſtly more depreſſed than the other.

FOR the clearing up of ſome particulars relating to this tryal, we will ſubjoin the following notes.

1. THE plug is to be ſo fitted to the cavity of the cylinder, as eaſily to ſlip up and down it, without grating againſt the ſides of it, leſt it needleſly increaſe the reſiſtance of the weight to be raiſed. And this plug ought to be of a convenient length, as about an inch and a half at leaſt, that it may be the fitter to help to reduce the bladder by compreſſion into a ſomewhat cylindrical ſhape, and yet that it may not be thruſt in too deep by the incumbent weight; and that the weight might reſt more firmly upon it, there was a broad and ſtrong ledge made at the top of it, by which it might lean on every ſide upon the brim of the hollow cylinder.

2. BEFORE the inſtrument was conveyed into the receiver, the bladder (which ought to be of a juſt ſize, and not full blown, and of a fine and limber contexture) was put into the cylinder, and by divers gradual (but not immoderate) compreſſions was reduced to conform its ſelf, as much as might be, to the cylindrical ſhape of the containing veſſel. And then the weight being put on, and taken off again, there was a mark (in the form of an horizontally placed arch) made with ink, where the edge of the brim of the hollow cylinder did almoſt touch the plug. This we thought neceſſary to do, to avoid a miſtake; for we muſt not judge, that all the weight, that might be raiſed by our bladder, may paſs for the weight ſought after by our experiment; ſince the air in the bladder is by reaſon of the incumbent weight more compreſſed than it was before, and conſequently its being able to heave up a great weight will not infer, that our common air is able in its natural ſtate (as they call it) to exert ſo great a ſtrength; that weight being only to be looked on as raiſed or ſuſtained by the uncompreſſed air, that is, raiſed or ſuſtained, when the plug is lifted up to the mark, ſince till then the ſpring of the air does but bring it back from its new ſtate of adventitious compreſſion to its natural or wonted laxity.

3. WHEN, after the operation was ended, we took the bladder out of the veſſel, it had obtained a form cylindrical enough; and though it could be but two inches in diameter, yet it was ſo little, as to be but half an inch more long than broad.

4. THE reaſon, why I choſe to have the two cylinders made of the unequal diameters above-mentioned, was to examine, as far as by this way I could, a conjecture I had, that the force of the ſpring of differing cylinders of air to lift up ſolid weights would, at the very firſt raiſing of the weights, be in duplicate proportion to the diameters of their cylinders, (thoſe diameters being proportionable to the areas of the plain ſuperficies, againſt which the air does immediately preſs,) without very much conſidering the inequality, that may be between the quantity of the ſeveral parcels of air, whoſe preſſures are compared. But it is to be remembered, that I ſaid, at the very firſt raiſing of the weights, becauſe preſently after that, the quantity of the parcels of air may be very conſiderable: for, as I have ſhewn in another treatiſe, two very unequal quantities of air being made by their expanſion to poſſeſs two equal ſpaces the leſſer quantity of air muſt be much more rarefied in proportion than the greater; and conſequently, to bring this home to our preſent argument, though both be lifted up ¼ or ½ of an inch, the ſpring of a very little air muſt be much more weakened than that of a very conſiderable quantity, and ſo it cannot continue to lift up its weight, as the above-mentioned proportion would (if it were not for this advertiſement) ſeem to require.

TAKING then our conjecture in the ſenſe now declared, the ſucceſs of our trials is agreeable to it, inviting us to conclude, that the air in the bladder, which was but two inches in diameter, was able by its preſſure to countervail the weight of 42 pound, which is about four times, the weight, that we lately obſerved the ſpring of a cylinder of air of one inch in diameter to be able to lift up. For though, according to what we have formerly ſaid of a duplicate proportion, 42 pound ſeems to be ſomewhat more than ought to have been lifted up in the cylinder of two inches bore, when that of one inch lifted up not above 10 pound; yet this diſagrees not with the hypotheſis, if we conſider, that the ſubſtance of the bladder ſtraitens the cavity of the ſmaller cylinder in a greater proportion than that of the bigger.

5. THOUGH we have thus (as far as the inſtruments we were able to procure would aſſiſt us) meaſured the preſſure of included air, yet I muſt not forbear to advertiſe your lordſhip, that conſidering what I formerly obſerved to you about the weight of an atmoſpherical pillar of an inch in diameter, I cannot but think, that if a cylinder, or other convenient inſtrument, exactly tight, can be procured, the ſpring

of an aerial cylinder will appear to be greater than we found it by the foregoing tryals; in which I conſider, that, not to mention the reſiſtance of the bladder itſelf, the membraneous ſubſtance, that lined the cylinders (though it were very thin and fine) could not but ſomewhat ſtraiten their cavities, and conſequently ſomewhat (though not much) leſſen the diameters of the included aerial cylinders.

6. To all theſe notes I muſt add this advertiſement, that it may be therefore the more difficult in ſuch trials as ours to aſcertain the force of the air's ſpring, becauſe that air itſelf, when it is concluded, being ſhut up with the preſſure of the atmoſphere upon it, it is probable, that ſince that preſſure, as we have ſhewn, is not at all times the ſame, the ſpring of the included air will accordingly be varied. And if my memory fail me not, when the lately recited experiments were made, our barometer declared the atmoſphere to be ſomewhat light.

FROM what has been hitherto delivered, this may reſult; that it is likely, that the ſpring of an aerial cylinder an inch broad may be able to ſuſtain, if not raiſe, a pretty deal more than ten pound weight; and that the paſt trials, without determining that the air can raiſe no more in them than it did, do at leaſt prove, that it can raiſe up as much weight as we have related, ſince we actually found it to do ſo.

EXPERIMENT XLVIII.

About an eaſy way of making a ſmall quantity of included air raiſe in the exhauſted receiver 50 or 60 pound, or a greater weight.

I WOULD very willingly have further proſecuted the foregoing trials, to ſee how far the lately propoſed conjecture or hypotheſis would hold; but was hindered by the want of receivers tall and capacious enough to contain the weights, that ſuch an attempt required: but remembring, that there were not any experiments made in our engine, that appeared more ſtrange to the generality of ſpectators, and ſerved more to give them a high opinion of the air's ſpring, than thoſe, wherein they ſaw ſolid bodies actually lifted up by it; and remembering, that I had lying by me a braſs veſſel, which had been beſpoken for another experiment, for which the workmen had not made it fit; I thought it not amiſs to employ it about making a trial very eaſy, and yet fit to be ſhewn to ſtrangers, to convince them, that the ſpring of the air is a much more conſiderable thing than they imagined.

See Plate VIII. Fig. 4.

WE took then a braſs veſſel made like a cylinder, and having one of his orifices exactly covered with a flat plate very firmly faſtened to it, the other orifice being wide open. The depth of this veſſel was four inches, and the diameter ſhould have been preciſely, but wanted about a quarter of an inch of, four inches. To this hollow cylinder we fitted a wooden plug, like one of thoſe deſcribed in the foregoing experiment, ſave that it was not quite ſo long, and that it was furniſhed with a rim or lip, which was purpoſely made of a conſiderable breadth, that it might afford a ſtable baſis to the weight that ſhould lean upon it. And then taking a middle-ſized and limber bladder, ſtrongly tied at the neck, but not near full blown, we preſſed it by the help of the plug into the cylinder to make it the better accommodate is ſelf to the figure of it. Then taking notice by an inky mark, how much the plug was extant above the orifice of the veſſel, we laid the weights upon the plug, whoſe rim or lip hindered it from being depreſſed too deep into the cavity of the veſſel; and having conveyed them into the receiver, we found, as we expected, that if we had loaded the plug but with a ſingle weight, as to avoid trouble and the danger of breaking the glaſs we uſually thought fit to do, though that were a common half-hundred weight, which you know amounts to 56 pound, it would very quickly be manifeſtly heaved up by the ſpring of the included air. For confirmation of more than which, I ſhall ſubjoin the enſuing trial, as I find it recorded among my looſe notes.

See Plate VIII. Fig. 3.

See Plate VIII. Fig. 5.

THE weight, that was lifted up by the bladder in the cylinder four inches broad, was 75 pound: this weight was lifted up till the wooden plug diſcloſed the mark, that was to ſhew the height, at which the air kept the ſaid plug before it was compreſſed: diſcloſed, it I ſay, viſibly at the fifth exſuction, and at the ſeventh that mark was $\frac{1}{8}$, or rather $\frac{1}{16}$ above the edge of the cylinder. In the gage, where the mercury in the open air was wont to ſtand about $\frac{1}{8}$ above the uppermoſt glaſs-mark, it was depreſſed, till it was $\frac{1}{8}$ below the ſecond mark. When the air was let in, it was a pretty while before the weight did manifeſtly begin to ſubſide; the bladder being taken out, and the place it had poſſeſſed in the cylinder being ſupplied with a ſleeve, or ſome ſuch thing, and the weight laid again upon the plug, we found, that at twenty-four exſuctions the mercury was depreſſed to the loweſt mark of the gage; and it was the thirty-fourth or thirty-fifth exſuction before the receiver appeared to be ſo exhauſted, as to put an end to the ſinking of the mercury, which was then above $\frac{1}{8}$ beneath the loweſt mark.

YOUR lordſhip will eaſily believe, that moſt of the ſpectators of ſuch trials thought it ſomewhat ſtrange to ſee a ſmall quantity of air, which was not only uncompreſſed in the bladder, but did not near fill it, and left it very ſoft and yielding to the leaſt touch, lift up ſo eaſily by its bare ſpring ſuch great weights as endeavoured to oppreſs it. But this not being any thing near a ſufficient trial, how far the conjecture or hypotheſis formerly propoſed will hold, I thought fit to make the utmoſt trials, the talleſt receivers I could procure would admit: and having cauſed leaden weights to be purpoſely caſt flat like cheeſes, and as broad as we could conveniently put into the receiver, that by the advantage of this ſhape we might be able to pile up the more of them, without much danger that any of them ſhould be ſhaken down; we laid divers of them one upon another, and then the upper part of the receiver growing too narrow to admit more of them, we added a leſs broad weight or two; and then exhauſting the receiver, till we perceived by the gage, that the air was manifeſtly withdrawn, we found, as near as we could meaſure, by the help of a mark and a pair of compaſſes, that the plug was ſo far raiſed, as that it was concluded, that the elevation would have been much greater, if the included air, being put upon ſo great a conatus, had not found it eaſier to produce ſome leak at the neck of the bladder, than to lift up ſo great a weight, which by our reckoning came to about 100 pound of 16 ounces to the pound. But this laſt experiment, for want of ſome requiſite accommodations, we were hindered from repeating and promoting; though the above mentioned hypotheſis made me preſume, that a far greater weight might this way have been raiſed, if the bladder had been ſtanch, and the receiver high enough.

I need not tell your lordſhip, that if a larger bladder be employed and included in a braſs veſſel of a ſufficiently wide orifice, a far greater weight may be lifted up by the ſpring of the internal air. But yet it will not be amiſs to give your lordſhip on this occaſion this advertiſement, which may be fit to be taken notice of on divers others; that care muſt be had not to make receivers, that ought to be well emptied, too large, and eſpecially too wide at the orifice; for otherwiſe they will be expoſed to ſo great a preſſure of the atmoſphere, that they need be of an extraordinary ſtrength to reſiſt it; and even receivers, that ſeemed thick enough proportionably to their bulk, and which held out very well till the cloſe of the operation, yet when they came to be very diligently exhauſted, they did, by reaſon of the wideneſs of their orifices, begin to crack at the bottom.

EXPERIMENT XLIX.

Viz. the XXXVI. IN one of my publiſhed experiments I long ſince told your lordſhip, that when I endeavoured, by the help of a ſealed bubble, weighed in an exhauſted receiver, to compare the gravity of air and water, I was hindered by the caſual breaking of the glaſs from compleating the experiment. Wherefore I afterwards thought fit to repeat the trial; and though when I had done ſo twice or thrice, having given away the large receiver I had made uſe of about them, and not being able ever ſince to procure a good one, that was capacious enough for the tender ſcales I thought ſo nice an experiment required, I did not proſecute that attempt ſo far as I intended; yet this very difficulty I met with, to procure the requiſites of making the trial, invites me to ſubjoin the two following notes, which I find among my looſe papers.

April the 19, 1662. WE weighed a bubble in the receiver, which we found to weigh above half a grain heavier, when much of the air was exhauſted, than when it was full. Afterwards we took out this ſealed bubble, and weighing it found it to weigh 68 grains and a half; then breaking off the ſmall tip of it under water, we found, that the heat, by which it was ſealed up, had rarefied its included air, ſo that it admitted 125 grains of water, for the admitted water and glaſs weighed 193 $\frac{1}{2}$ grains. Then filling it full with water, we found it to contain in all 739 grains of water, for it weighed 807 $\frac{1}{2}$ grains: whence it is evident, that the difference between the weight of water and air was leſs than 1228 to 1.

WE weighed in the receiver a bubble, the glaſs of which weighed 60 grains: the air that filled it, weighed *in vacuo* $\frac{27}{32}$ of a grain: the water, that filled it, weighed 720 $\frac{1}{4}$ grains: ſo that by this experiment the proportion of the weight of air to water is as one to 853 $\frac{17}{27}$.

May 26, 1662. THE trials mentioned in theſe notes, though they were too few for me to acquieſce in, yet being made in a new way, and which has ſome advantages above thoſe, that have been hitherto employed to weigh the air, may yet ſerve to keep us from the contrary extremes, that have not been avoided by ſuch eminent mathematicians as *Galileo* and *Ricciolus*; the former of which makes water to be about 400 times as heavy as the air; and the latter, whoſe conjecture is much remoter from the truth, 10,000 times heavier.

BUT it is ſo deſireable a thing, and may prove of ſuch importance, to know the proportion in weight betwixt air and water, that I ſhall not ſcruple to acquaint your lordſhip with an attempt or two, that I made to diſcover it by another way. For, though at firſt ſight this experiment may ſeem to be the ſame with one publiſhed a pretty while ago in the learned *Schottus* his *Mechanica hydrolico-pneumatica*; yet your lordſhip will eaſily perceive this difference between them; that, whereas the induſtrious author of that experiment contents himſelf to ſhew, by the diminution of the weight of a glaſs, when the air has been drawn out of it, that the air, before it was drawn out, was not devoid of gravity; the following trial does not only perform the ſame thing, and by a ſuperadded circumſtance confirm the truth to be thereby proved, but it endeavours alſo to ſhew the proportion in gravity betwixt the air and water. The trials themſelves were regiſtered among my *Adverſaria* as follow:

A ſmall receiver being exhauſted of air by the engine, and counterpoiſed whilſt it continued ſo; the ſtop-cock was turned, and the air re-admitted, which made it weigh 36 grains more than it did before: and to prevent jealouſies, we cauſed it to be applied the ſecond time to the engine, by which the air being emptied once more, the glaſs was put into the other ſcale of the former ballance, and ſo counterpoiſed; and then the external air being re-admitted, (which ruſhed in as formerly with a whiſtling noiſe,) there was found 36 grains or better requiſite to reſtore the ballance to an æquilibrium.

WE took a ſmall glaſs receiver fitted with a ſtopcock, and having exhauſted it of the air, and counterpoiſed it, and let in the outward air, we found the weight of the veſſel to be increaſed by that admiſſion 36 grains. This done, we took the receiver, after having well counterpoiſed it, out of the ſcale; and having applied it the ſecond time to the engine, we once more withdrew the air, and then turning the ſtop-cock to keep out the external air, we took care, that none of the cement, employed to join it to the engine, ſhould ſtick to it, as we had diligently freed it from adherent cement, before we laſt applied it to the engine. Then weighing it again, we found it to weigh either 35 or 36 grains (but rather the former) heavier than it did, when it was laſt counterpoiſed in the ſame ballance. This being alſo done, we immerſed the ſtop-cock into a baſon of fair water, and let in the liquor, that we might find, how much water would ſucceed in place of the air we had drawn out. When no more water was impelled in, we turned the ſtop-cock once more, to keep it from falling out, and then weighing it in the ſame ſcales, (after we had wiped the ſtop-cock, that no water might ſtick to it on the outſide,) we found the water (without computing the veſſel) to weigh 47 ounces, 3 drams, and 6 grains, which divided by 35 grains, (which I took to be the weight of the air, that was equal in bulk to this water that ſucceeded it,) the quotient was (wanting a very little) 650 grains, for the proportion of the weight between air and water of the ſame bigneſs, at the time when the experiment was made: which circumſtance I therefore take notice of, becauſe the atmoſphere appeared by the baroſcope (wherein the mercury ſtood then at 29 inches and three quarters) to be very heavy; which made me the leſs wonder to find this proportion not ſo great, as at other times I had obſerved it to be between water and air in point of weight: though I ſuſpected, that becauſe this odd experiment cannot be nimbly diſpatched, ſome little air may have got in at the ſtop-cock, beſides the air that diſcloſed itſelf in numerous bubbles in the water that was admitted, where, though it lay in ſuch ſmall particles as not to be diſcerned before, yet theſe particles by this opportunity to expand themſelves, extricated themſelves from the water, and by getting together might ſomewhat reſiſt the ingreſs of more; which is a difficulty, whereto the meaſuring the proportion between water and air in a heated æolipile is liable. But the ſtealing in of any air, before the water was let in, is mentioned but as a ſuſpicion.

YOUR lordſhip may perhaps think it ſomewhat ſtrange, that I ſhould preſent you trials, whoſe events do not ſo well agree together, as perchance you expected. But this very diſagreement was one of the motives, that induced me to acquaint you with them: for all thoſe compriſed in theſe experiments being made faithfully, and not without, at the leaſt, an ordinary diligence, as they ſeem to make it probable, that one may without any great error eſtimate the proportion of our Engliſh air to water to be as one to ſome number between 600 and 1100; ſo it is not to be expected, that the proportion, whatever it be that ſhould be pitched upon, ſhould be accurate and ſtable. For though learned men ſeem to have hitherto taken it for granted, that it may ſuffice once for all diligently to inveſtigate the proportion betwixt thoſe two bodies, yet, not only that I am apt to believe, that a determinate quantity of air (as a pint or quart) may be unequally heavy in diſtant countries, and even in differing places of the ſame country; but what I have taken notice of in the 17th of the printed experiments, and afterwards frequently obſerved of the great inequalities of the weight of the atmoſphere, inclines me to think, that in the ſelf ſame place two experiments may be made with the ſame inſtruments, and equal diligence, and yet the weights of the air may be found differing enough; which may keep your lordſhip from much wondering, that in the 36th printed experiment, made when I had the variations of the atmoſphere's gravity in my eye, I found the air to be leſs ponderous in reference to water, than in theſe latter trials. But of this, I hope, I ſhall, if God permit, make further trials with the ſame veſſels, at times when I ſhall perceive by the baroſcope, that the gravity of the atmoſphere is very great and very ſmall. And I wiſh the curious would make the like trials in other regions. I do not forget, that not only the ſchool-philoſophers, but moſt of the moderns deny, that air hath any weight in air, no more than water in water; but having * elſewhere declared and explained my ſenſe about this received opinion, I ſhall not here ſpend any of the little time I have remaining, to juſtify my diſſent; for which your lordſhip may find ſufficient grounds in the newly related experiments, eſpecially if you pleaſe to conſider, that though the opinion I diſallow have been chiefly and generally grounded upon ſome arguments ſuppoſed to evince, that water has no weight in water, I have † elſewhere ſhewn thoſe proofs not to be cogent, and taught a practical way of weighing water in water, with a pair of ordinary ſcales ‡.

EXPERIMENT L.

About the disjoining of two marbles (not otherwiſe to be pulled aſunder without a great weight) by withdrawing the preſſure of the air from them.

IN our formerly publiſhed experiments about the air ‖, I did, if I miſremember not, acquaint your lordſhip with an attempt I had made to make a couple of coherent marbles fall aſunder, by withdrawing the air from them. But though I then eſteemed, that their coheſion depended upon the preſſure of the air, yet not being at that time furniſhed with all the accommodations requiſite to make an experiment not eaſy to be performed ſucceed, I thought fit, when I had afterwards opportunity, to proſecute what I then began, and add ſome circumſtances, that I could not then make trial of, and yet whoſe ſucceſs will not, I preſume, be unwelcome, ſince it ſupplies us with no leſs than matters of fact; whence we may argue, this that experiment of coherent marbles (which not only the Ariſtotelian pleniſts have much triumphed in, but which ſome recent favourers of our hypotheſis have declared themſelves to be troubled with) is not only reconcileable to our doctrine; but capable of being made a confirmation of it; notwithſtanding what has lately been publiſhed (upon the ſuppoſition of a caſe, which at firſt bluſh may ſeem ſomewhat of a kin to our experiment,) by a very learned * writer, to whoſe objection againſt our hypotheſis, though as well confidently as very civilly propoſed, an anſwer may in due place, if your lordſhip deſire it, be returned.

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* In the Hydroſtatical Paradoxes.

† In an Appendix to thoſe Paradoxes.

‡ This method was omitted in the Engliſh edition of the newly mentioned appendix, but not in the Latin verſion.

‖ Experiment XXXI. See alſo the cauſe of this phænomenon diſcourſed of in the author's Hiſtory of Fluidity and Firmneſs.

We took two flat round marbles, each of them of two inches and about three quarters in diameter; and having put a little oil between them to keep out the air, we hung at a hook faſtened to the lowermoſt a pound weight to ſurmount the cohæſion, which the tenacity of the oil and the imperfect exhauſtion of the receiver might give them. Then having ſuſpended them in the cavity of a receiver, at a ſtick that lay horizontally a-croſs it; when the engine was filled, and ready to work, ſhe ſhook it ſo ſtrongly, that thoſe, that were wont to manage it, concluded, it would not be near ſo much ſhaken by the operation. Then beginning to pump out the air, we obſerved the marbles to continue joined, until it was ſo far drawn out, that we began to be diffident whether they would ſeparate. But at the 16th ſuck, upon the turning of the ſtop-cock, (which gave the air a paſſage out of the receiver into the pump,) the ſhaking of the engine being almoſt, if not quite, over, the marbles ſpontaneouſly fell aſunder, wanting that preſſure of the air, that formerly had kept them together: which event was the more conſiderable, not only becauſe they hung parallel to the horizon, but adhered ſo firmly together, when they were put in, that having tried to pull them aſunder, and thereby obſerved how cloſe they ſtuck together, I foretold it would coſt a good deal of pains ſo far to withdraw the air, as to make them ſeparate: which conjecture your lordſhip will the leſs wonder at, if I add, that a weight of 80 and odd pounds, faſtened to the lowermoſt marble, may be drawn up together with the uppermoſt, by vertue of the firmneſs of their coheſion.

N. B. This is not the only time, that this experiment ſucceeded with us. For ſometimes, when they were not ſo cloſely preſſed together before they were put in, the disjunction was made at the 8th ſuck, or ſooner, and we ſeemed to ourſelves to obſerve, that when we hung but half a pound weight to the lower marble, it required a greater exhauſtion of the receiver to ſeparate them, than when we hung the whole pound.

After, having proceeded thus far with the inſtruments we then had, meeting with an artificer, that was not altogether unſkilful, we directed him to make (what we wanted before in that place) ſuch a braſs-plate, to ſerve for a cover or cap to the upper orifice of receivers open at the top, as we have divers times had occaſion to mention already in giving accounts of ſome of the foregoing trials: by the help of which contrivance we proſecuted the newly related experiment much farther than we could do before, as may appear by the following account.

See Plate IV. Fig. 4.

We faſtened to the lowermoſt of the two marbles a weight of a very few ounces, (for I remember not the preciſe number,) and having cemented the capped receiver with the marbles in it, as before, to the pump, we did by a ſtring, whereof one end was tied to the bottom of this turning-key, and the other to the uppermoſt marble; and which (ſtring) paſſed through the crank or hook belonging to the braſs-cover; we did, I ſay, by the help of this ſtring, and by turning round the key, draw up the ſuperior marble, and by reaſon of their coherence the lowermoſt alſo, together with the weight that hung at it: by which means being ſure, that the two marbles ſtuck cloſe together, we began to pump out the air, that kept them coherent; and after a while, the air being pretty well withdrawn, the marbles fell aſunder. But we having ſo ordered the matter, that the lowermoſt could fall but a little way beneath the other, we were able by inclining and ſhaking the engine to place them one upon another again, and then letting in the air ſomewhat haſtily, that by its ſpring it might preſs them hard together, we found the expedient to ſucceed ſo well, that we were not only able by turning the abovementioned cylindrical key, to make the uppermoſt marble take up the other, and the annexed weight; but we were fain to make a much more laborious and diligent exhauſtion of the air, to procure the disjunction of the marbles this ſecond time, than was neceſſary to do it at the firſt.

And for further prevention of the objections or ſcruples, that I foreſaw ſome prepoſſeſſions might ſuggeſt, I thought fit to make this further trial; that when the marbles were thus aſunder, and the receiver exhauſted, we did, before we let in the air, make the marbles fall upon one another as before; but the little and highly expanded air, that remained in the receiver, having not a ſpring near ſtrong enough to preſs them together, by turning the key we very eaſily raiſed the uppermoſt marble alone, without finding it to ſtick to the other as before. Whereupon we once more joined the marbles together, and then letting in the external air, we found them afterwards to ſtick ſo cloſe, that I could not without inconvenience ſtrain any farther, than I fruitleſly did, to pull them fairly aſunder; and therefore gave them to one that was ſtronger than I, to try, whether he could do it, which he alſo in vain attempted to perform.

And now, my lord, though I had thoughts of adding divers other experiments to thoſe I have hitherto entertained you with; yet (upon a review) finding theſe to amount already to fifty, I think it not amiſs to make a pauſe at ſo convenient a number. And the rather, becauſe an odd quartanary diſtemper, that

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* Dr. H. M. in the ſecond chap. of the ſecond book, of the new edition in folio, of his *Antidote againſt Atheiſm.*

that I ſlighted ſo long, as to give it time to take root, is now grown ſo troubleſome, that I fear it may have too much influence upon my ſtyle; which apprehenſion obliges me, as well to avoid abuſing or diſtreſſing your lordſhip's patience, as to allow myſelf ſome ſeaſonable refreſhment, to reſerve the mention of the deſigned additions, until they can with leſs trouble to us both be preſented you by,

My dear lord,

Your lordſhip's moſt humble ſervant,
and affectionate uncle,

Oxford, March 24,
1667.
ROBERT BOYLE.

NOTES, &c.

ABOUT THE

ATMOSPHERES of CONSISTENT BODIES here below.

SHEWING,

That even HARD and SOLID BODIES (and ſome ſuch, as one would ſcarce ſuſpect) are capable of emitting EFFLUVIA, and ſo of having ATMOSPHERES.

ADVERTISEMENT.

HE that ſhall take the pains to peruſe the following paper, will eaſily believe me, when I tell him, that it was not deſigned to come abroad with the experiments, in whoſe company it now appears. But the ſtationer earneſtly repreſenting, that divers experiments being reſerved by me for another occaſion, the remaining ones alone would not give the book a thickneſs any thing proportionable to its breadth; I conſented, at his ſollicitation, to annex to them the following obſervations, becauſe of ſome affinity between the ſmall atmoſpheres of leſſer bodies, and the great atmoſphere that ſurrounds the terreſtrial globe; in which the other, that do at leaſt help to compoſe it, are loſt and confounded, as brooks and rivers are in the ocean. And to ſave the reader the pains of making gueſſes, to what kind of writing the enſuing diſcourſe may belong, I ſhall here intimate, that it is diſmembred from certain papers about occult qualities in general, which make part of the notes I long ſince deſigned, and alſo partly publiſhed, about the origin of qualities, of which notes thoſe, that concerned effluviums, being the moſt copious, I referred them to four general heads; whereof the firſt only is treated of in the following diſcourſe, the others being withheld, as having not affinity enough with the atmoſphere to accompany this, whereon they have no ſuch abſolute dependence, but that they may well enough ſpare it. And I make the leſs ſcruple to let it appear without them, becauſe the inducements already mentioned are not a little ſtrengthened by this ſuperadded conſideration, that the following notes may give light to ſeveral of the obſervations I have made, of ſome leſs heeded phænomena of the alterations of the air, in caſe they be allowed to enter into the Appendix to this Continuation.

Of the ATMOSPHERES of CONSISTENT BODIES.

THE ſchool philoſophers, and the vulgar, in conſidering the more abſtruſe operations and phænomena of nature, are wont to run into extremes; which, though oppoſite to one another, do almoſt equally contribute to keep men ignorant of the true cauſes of thoſe effects they admire. For the vulgar, being accuſtomed to converſe with ſenſible objects, and to conceive groſly of things, cannot eaſily imagine any other agents in nature, than thoſe that they can ſee, if not alſo touch, and handle; and as ſoon as they meet with an effect, that they cannot aſcribe to ſome palpable, or at leaſt ſenſible efficient, they are, and ſtick not to confeſs themſelves utterly at a loſs. And though the vulgar of philoſophers will not acknowledge themſelves, to be poſed by the ſame phænomena with the vulgar of men, yet in effect they are ſo. But the ſchool-philoſophers, on the contrary, do not only refuſe to acquieſce in ſenſible agents, but, to ſolve the more myſterious phænomena of nature, nay and moſt of the familiar ones too, they ſcruple not to run too far to the other ſide, and have their recourſe to agents, that are not only inviſible, but inconceivable, at leaſt to men, that cannot admit any, ſave rational and conſiſtent notions: they aſcribe all abſtruſe effects to certain ſubſtantial forms, which however they call material, becauſe of their dependence on matter, they give ſuch deſcriptions to, as belong but to ſpiritual beings: as if all the abſtruſer effects of nature, if they be not performed by viſible bodies, muſt be ſo by immaterial ſubſtances; whereas betwixt viſible bodies and ſpiritual beings there is a middle ſort of agents, inviſible corpuſcles; by which a great part of the difficulter phænomena of nature are produced, and by which may intelligibly be explicated thoſe phænomena, which it were abſurd to refer to the former, and precarious to attribute to the latter. Now for method's ſake I will refer the notes, that occur to me about effluviums, to four heads; whereof the firſt is mentioned in the title of this paper, and each of the other three ſhall be ſucceſſively treated of in as many diſtinct ones.

THAT fluid bodies, as liquors, and ſuch as are manifeſtly either moiſt, or ſoft, ſhould eaſily ſend forth emanations, will I preſume be granted without much difficulty; eſpecially conſidering the ſenſible evaporation, that is obvious to be obſerved in water, wine, urine, &c. and the looſe contexture of parts, that is ſuppoſed to be requiſite to conſtitute ſoft bodies, (as flowers, balſoms, and the like:) but that even hard and ponderous bodies, notwithſtanding the ſolidity and ſtrict coheſion of their component parts, ſhould likewiſe emit ſteams, will to many appear improbable enough to need to be ſolemnly proved.

WHETHER you admit the atomical hypotheſis, or prefer the Carteſian, I think it may be probably deduced from either, that very many of the bodies, we are treating of, may be ſuppoſed exhaleable as to their very minute parts. For, according to the doctrine of *Leucippus*, *Democritus*, and *Epicurus*, each indiviſible particle of matter hath eſſentially either a conſtant actual motion, or an unlooſeable endeavour after it; ſo that though it may be ſo complicated in ſome concretions with other minute parts, as to have its avolation hindered for a while; yet it can ſcarce otherwiſe be, but by this inceſſant endeavour of all the atoms to get looſe, ſome of them ſhould from time to time be able to extricate themſelves, and fly away. And though the Carteſians do not allow matter to have any innate motion, yet, according to them, both vegetables, animals, and minerals, conſiſt of little parts ſo contexed, that their pores give paſſage to a celeſtial matter; ſo that this matter continually ſtreaming through them, may well be preſumed to ſhake the corpuſcles, that compoſe them: by which continued concuſſion now ſome particles, and then others, will be thrown and carried off into the air, or other contiguous body, fitted to receive them. But though by theſe, and perhaps other conſiderations, I might endeavour to ſhew *à priori*, as they ſpeak, that it is probable conſiſtent bodies themſelves are exhaleable, yet I think it may be as ſatisfactory, and more uſeful, to prove it *à poſteriori*, by particular experiments, and other examples.

THAT then a dry and conſiſtent form does not neceſſarily infer, in the bodies that are endowed with it, an indiſpoſition to ſend forth ſteams, which are, as it were, little colonies of particles, is evident, not only in the leaves of damaſk roſes, whether freſh or dried; as alſo in wormwood, mint, rue, &c. but in ambergreeſe, muſk, ſtorax, cinnamon, nutmegs, and other odoriferous and ſpicy bodies. But more eminent examples to our preſent purpoſe may be afforded us by camphire, and volatile ſalts, ſuch as are chemically obtained from hartshorn, blood, &c. for theſe are ſo fugitive, that ſometimes I have had a conſiderable lump of volatile ſalt (either of fermented urine, or of harts-horn) fly away by little and little out of a glaſs, that had been carefully ſtopped with a cork, without leaving ſo much as a grain of ſalt behind it. And as for camphire, though by its being uneaſy to be powdered, it ſeem to have ſomething of toughneſs or tenacity in it; yet I remember, that having for trial's ſake counterpoiſed it in nice ſcales, even a ſmall lump of it would in a few hours ſuffer a viſible loſs of its weight, by the avolation of ſtrongly ſcented corpuſcles, and this, though the experiment were made both in a north window, and in winter.

BUT I expect you ſhould require inſtances of the effluviums of bodies of a cloſe or ſolid texture: wherefore I proceed to take notice, that amber, hard-wax, and many other electrical bodies do, when they are rubbed, emit effluviums. For though I will not now meddle with the ſeveral opinions about the cauſe and manner of electrical attraction, yet beſides that almoſt all the modern naturaliſts, that aim at explicating things intelligibly, aſcribe the attraction we are ſpeaking of to corporeal effluxes; and beſides that I ſhall ere long have occaſion to ſhew you, that there is no need to admit with Carteſius, that becauſe ſome electrical bodies are very cloſe and fixed, what they emit upon rubbing is not part of their own ſubſtance, but ſomewhat that was harboured in their pores: beſides theſe things, I ſay, I have found, that many electrical bodies may by the very noſtrils be diſcovered, when they are well rubbed, to part with ſtore of corpuſcles, as I have particularly, but not without attention, been able to obſerve in amber, roſin, brimſtone, &c.

I know not, whether it will be worth while to take notice of the great evaporation I have obſerved, even in winter, of fruits, as apples, and of bodies, that ſeem to be better covered, as eggs; which, notwithſtanding the cloſeneſs of their ſhels, did daily grow manifeſtly lighter and lighter, as I obſerved in them, and divers other bodies, that I kept long in ſcales, and noted their decrements of weight. But perhaps you will be pleaſed to hear, that having a mind to ſhew, how conſiderable an evaporation is made from wood, I cauſed a thin cup, capable of holding about a pint, or more, to be turned of a wood, that was choſen by the turner as ſolid and dry enough, though it were not of the cloſeſt ſort of woods, ſuch as are lignum vitæ, and box. And as I cauſed the ſhape of a cup to be given it, that it might have a greater ſuperficies expoſed to the air, and conſequently might be the fitter to emit ſtore of ſteams into it; ſo the ſucceſs did not only anſwer my expectation, but exceed it; for though the trial were made ſome time in winter, there was ſo quick and plentiful an evaporation made from the cup, that I found it no eaſy matter to counterpoiſe it; for whilſt grains were putting into the oppoſite ſcale, to bring the tender balance to an æquilibrium, the copious avolation of inviſible ſteams from the wood (which had ſo much of ſuperficies contiguous to the air) would make the ſcale that held it ſenſibly too light. And I remember, that for further ſatisfaction, being afterwards in a city, where there were both good materials and workmen, I ordered to be made a bowl, about the ſame bigneſs with the former, of well ſeaſoned wood, which being ſuſpended in the chamber I lay in, (which circumſtance I therefore mention, becauſe the weather and a little phyſick I had taken obliged me to keep a fire there,) it quickly began manifeſtly to looſe of its weight; and though the whole cup wanted near two drachms of near two ounces, yet in twelve hours, viz. from ten of the clock in the morning to the ſame hour at night, it loſt about 40 grains, (for it was above 39.) But of ſuch experiments, and the cautions belonging to them, I may elſewhere ſpeak farther.

It were not difficult for me to multiply inſtances of the continual emanation of ſteams from vegetable and animal ſubſtances; but I am not willing to enlarge myſelf upon this ſubject, becauſe I conſider, that there are other bodies, which ſeem ſo much more indiſpoſed to part with effluviums, that a few inſtances given in ſuch may evince what I would prove, much more than a multitude produced in other bodies. And ſince I conſider, that thoſe ſubſtances are the moſt unlikely to afford effluvia, that are either very cold, or very ponderous, or very ſolid and hard, or very fixed; if I can ſhew you, that neither of theſe qualifications can keep a body from emitting ſteams, I hope I ſhall have made it probable, that there is no ſort of bodies here below, that may not be thought capable of affording the corporeal emanations we ſpeak of.

And firſt I remember, that I have not only taken eggs, and in a very ſharp winter found them, notwithſtanding the coldneſs of the air where I kept them, to grow ſenſible lighter, in a faithful pair of ſcales, in not very many hours; but becauſe ice is thought the coldeſt viſible body we know, I thought fit to ſhew, that even this body will looſe by evaporation; for having counterpoiſed a convenient quantity of ice in a good balance, and forthwith expoſed it therein to the cold air of a froſty night, that the evaporations ſhould be from ice not from water, I found the next morning, that though the ſcale, wherein the ice were put, was dry, which argued as well as the coldneſs of the weather, that the expoſed concretion had not thawed; yet I found its weight to be conſiderably diminiſhed, and this experiment I ſucceſsfully made in more than one winter, and in more than one place. And it is now but a few days ſince, expoſing not long before midnight leſs than two ounces of ice in a good balance to a ſharply freezing air, I ſent for it before I was up in the morning; and though by the drineſs of the ſcales the ice, that was in one of them, appeared not to have thawed, yet it had loſt about ten grains of its former weight; ſo that here the evaporation was made in ſpite of a double cold, of the ice, and of the air.

I ſhould now proceed to the mention of ponderous and ſolid bodies; but before I do ſo, it may be expedient to give you notice, that, to make the proof of what I have propoſed more ſatisfactory, and more applicable to our future purpoſes, I ſhall forbear to give you any examples of the exhalations of bodies, where ſo potent an agent as the fire is made to intervene.

But though I purpoſely forbear to inſiſt on ſuch examples, yet it may not be amiſs to intimate, that in explicating ſome occult qualities, even ſuch exhalations, as are produced by the help of the fire, may be fit to be taken into conſideration, as we may hereafter have occaſion to ſhew. And therefore we may obſerve in general, that the fire is able to put the parts of bodies into ſo vehement a motion, that except gold, glaſs, and a very few more, there are not any bodies ſo fixed and ſolid, that it is not thought capable to diſſipate either totally, or in part. It is known to thoſe, that deal in the fuſion of metals, that not only lead and tin, but much harder bodies will emit copious and hurtful ſteams. And there are ſome kinds of that iron, which our ſmiths call coldſhare iron, about whoſe ſmell, whilſt it was red hot, when I made inquiry, the ingenioufeſt ſmith I had then met with told me, that he had found it ſeveral times to be ſo ſtrong, and rank, that he could ſcarce indure to work with his hammer thoſe parcels of metal, whence it proceeded. And even without being brought to fuſion, not only braſs, and copper will, being well heated, become ſtrongly ſcented, but iron will be ſo too, as is evident by the unpleaſing ſmell of many iron ſtoves. And on this occaſion I might not impertinently add here a trial we made to obſerve, whether the ſteams of iron may not be made, though not immediately viſible, yet perceptible to the eye itſelf, though the metal had not a red, much leſs a white heat. But having elſewhere related it at large, in a diſcourſe you may command a ſight of, I ſhall rather refer you to it, than looſe the time it would take up to tranſcribe it.

These things premiſed, I proceed now to the mention of ponderous bodies; and concerning them, to repreſent, that if you will admit what almoſt all the corpuſcularians aſſert, and divers of the peripateticks do not now think fit to deny, that the magnetical operations are performed by particles iſſuing forth of the body of the loadſtone, or other magnetical agent; I ſhall not need to go far for an inſtance to our preſent purpoſe, ſince I have hydroſtatically found, that ſome loadſtones (for I have found thoſe minerals very differing in gravity) are ſo ponderous, as to exceed double the weight of flints, or other ſtones of the ſame bulk.

BUT not to inſiſt on loadſtones, ſtone-cutters will inform you, as they did me, that black marble, and ſome other ſolid and heavy ſtones, will, upon the attrition they are expoſed to, when the workmen are poliſhing them, eſpecially without water, emit, and that without the help of external heat, a very ſenſible ſmell; which I found to be much more ſtrong and offenſive, when, to make it ſo, I had the curioſity to cauſe a piece of ſolid black marble to have divers fragments ſtruck off from it with a chizel and a hammer: for the ſtrokes ſucceeding one another faſt enough to make a great concuſſion of the parts of the black marble (for in white, which is not ſo ſolid, the trial will not ſucceed well) there quickly followed, as I expected, a rank unpleaſant ſmell; and you will grant me, I know, that odours are not diffuſed without corporeal emanations. I remember alſo, that having procured ſome of thoſe acuminated and almoſt conical ſtones, that paſs among the vulgar for thunder-ſtones, by rubbing them a little one againſt the other, I could eaſily, according to my expectation, excite a ſtrong ſulphureous ſtink. I have alſo tried upon a certain mineral maſs, that was ponderous almoſt as a metal, but to me it ſeemed rather an unuſual kind of marchaſite, that I could in a trice, without external heat, make it emit more ſtrongly ſcented exhalations, than I could contentedly endure: to which I ſhall add this example more, that having once made a chemical mixture of a metalline body and coagulated mercury, which you will believe could not but be ponderous, though this mixture had already endured as violent a fire as was neceſſary to bring it to fuſion, in order to caſt it into rings; yet it was ſo diſpoſed to part with corporeal effluxes, that a very ingenious perſon, that practiced phyſick, and was there when I made it, earneſtly begged a little of it of me for ſome patients troubled with diſtempers in the eyes, and other parts remote enough from the hand; which he affirmed himſelf to have very happily cured, by making the patient wear a ring of this odd mixture, or wearing a little of it as an appenſum near the diſaffected part. If you make a *vitrum Saturni* with a good quantity of minium in reference to the ſand or cryſtal, which it helps to bring to fuſion, you ſhall have a glaſs exceeding ponderous, and yet not devoid of electricity: and I remember, that having ſometimes cauſed braſs it ſelf to be turned like wood, that I might try, whether ſo great, though inviſible, a concuſſion of all the parts would not throw off ſome ſteams, that might be ſmelled, I was not reduced to forego my expectation; but yet becauſe it was not fully anſwered, and becauſe alſo there is great difference of braſs upon the ſcore of the *lapis calaminaris*, whereof together with copper it is made, I enquired of the workman, who uſed to turn great quantities of braſs, whether he did not often after find it more ſtrong; and he informed me, that he did, the ſmell being ſometimes ſo ſtrong, as to be offenſive to ſtrangers, that came to his ſhop, and were not uſed to it.

I proceed now to the effluviums of ſolid and hard bodies; of which, if moſt of our corpuſcularian philoſophers, and divers others be not much miſtaken, I may be allowed to give inſtances in all electrical bodies, which, as I have already noted, muſt according to their doctrine be acknowledged to operate by ſubſtantial emanations. Now among electrical bodies I have obſerved divers, that are of ſo cloſe a texture, that aqua fortis itſelf, nor ſpirit of ſalt will work upon them, and to be ſo hard, that ſome of them will ſtrike fire like flints. Of the former ſort I have found divers gems, which I named in my notes about electricity; and even the cornelian itſelf, which I found to attract hairs, though it be thought to be of a much ſlighter texture than precious ſtones, did yet reſiſt aqua fortis, as I tried in a large ring, brought out of the Eaſt-Indies, which I purpoſely broke, and reduced ſome part of it to powder, that I might make theſe and ſome other trials with it. Rock cryſtal alſo, though it have a very manifeſt attractive virtue, as they call it, I have yet found it ſo hard, as to ſtrike fire rather better than worſe than ordinary flints. And to ſhew, that no hardneſs of a body is inconſiſtent with its being electrical, I ſhall add, that though diamonds be confeſt to be the hardeſt bodies, that are yet known in the world, yet frequent experience has aſſured me, that even theſe, whether raw or poliſhed, are very manifeſtly, and ſometimes vigorouſly enough, electrical.

AND to let you ſee, that I need not to have recourſe to this kind of bodies, to prove, that very ſolid ones are capable of effluvia; I will, to what I have formerly noted about the odour of black marble, ſubjoin two or three examples of the like nature.

THE firſt ſhall be taken from a ſort of concretions very well known in divers parts of *Italy* by the name of cugoli, becauſe of the great uſe, that is made of it by the glaſs-men. Theſe concretions, you will eaſily believe, are very hard, as other minerals of that ſort are wont to be; and yet being invited by my conjectures about the atmoſpheres of bodies, to try them by rubbing them one againſt the other, I found, as I expected, that they afforded not only a perceptible, but a very ſtrong ſmell, which was far from that of a perfume.

AND this brings into my mind, that having met with ſome ſtones cut out of humane bladders, whoſe texture was ſo cloſe, that I could not with corroſive menſtruums make any ſenſible ſolution of one, whereon I made my trial, though, to facilitate the liquors operation, part of it were reduced to fine powder; yet by a little rubbing of one of theſe ſo cloſely contexed ſtones, it would preſently afford a rank ſmell, very like the ſtink of ſtale urine.

I remember I have cauſed iron to be turned with a lath, to examine, whether by the internal commotion, that would by that operation be produced in the corpuſcles of the metal, even that ſolid, as well as ponderous body would not become capable of being ſmelled; and though by reaſon of the nature of that parcel of iron, whereon we made our trial, or ſome accidental diſpoſition, which was at that time, being winter, in my organs of ſmelling, the odour ſeemed to me but very faint; yet upon the enquiry I made of the artificers, whether in turning greater pieces of iron they did not find the ſmell ſtronger? they told me, that they often found it very ſtrong, and ſometimes more ſo than they deſired.

And this brings into my mind what I have carefully obſerved in grinding of iron; for there are many grindſtones ſo qualified, that in caſe iron inſtruments be held upon the ſtone, whilſt it is nimbly turned under it, though the water, that is wont to be uſed on ſuch occaſions, ſtifles, if I may ſo ſpeak, the ſmell, and keeps it from being commonly taken notice of; yet if you purpoſely cauſe, as I remember I have done, the uſe of water to be forborn, your ſucceſs will not be like mine, if you do not find, that ſtore of fœtid exhalations will be produced. And though it be not always ſo eaſy to diſcern by the ſmell, from which of the two bodies they iſſue, or whether they proceed from both; yet it ſeems probable enough, that ſome of the ſteams come from the iron, and it is more than probable, that if they proceed not from that metal, they muſt from a body, that is ſo hard, as to be able to make impreſſions in a trice upon iron and ſteel themſelves.

The laſt example I ſhall name under this head, is furniſhed me by marchaſites, ſome of which would after a ſhort concuſſion without external heat be made to exhale for a pretty while together a ſtrong ſulphureous odour, and yet were ſo hard, than when ſtruck with a ſteel-hammer, (which would not eaſily break them) they afforded us ſuch a number of ſparks, as appeared ſtrange enough. And it is known, that it is from their diſpoſition to ſtrike fire, (which yet I dare not attribute to all ſorts of marchaſites,) that this kind of mineral is, by a name frequently to be met with in writers, called pyrites. And in this example we may take notice, that a body, capable of being the ſource of corporeal emanations, may be at once both very ſolid and very ponderous.

It remains now, that I manifeſt, that even the fixedneſs of bodies is not incompatible with their diſpoſition to emit effluviums.

I might allege on this occaſion, that the regulus of antimony, and alſo its glaſs, though they muſt have endured fuſion to attain their reſpective forms; yet they will without heat communicate to liquors antimonial expirations, with which thoſe liquors being impregnated become emetick and purgative. I might alſo add, that divers electrical bodies are very fixed in the fire, and particularly that cryſtal, as we have more than once tried, will endure ſeveral ignitions and extinctions in water, without being truly calcined, being indeed but cracked into a great multitude of little parts: but becauſe the above named antimonial bodies will after a while fly away in a ſtrong fire, and becauſe the effluviums of cryſtal are not ſo ſenſible as thoſe, which can immediately affect our eyes or noſtrils, I will here ſubjoin one inſtance, ſuch as I hope will make it needleſs for me to add any more, it being of a body, which muſt have ſuſtained any exceeding vehement fire, and is looked upon by moſt of the chemiſts as more undeſtroyable than gold itſelf; and that is glaſs, which is able, as you know, to endure ſo great a brunt of the fire, that you did not perhaps imagine I ſhould of all bodies name it on this occaſion. But my conjectures about the atmoſpheres of bodies leading me to think, that glaſs itſelf might afford me a confirmation of them; I quickly found, that by rubbing a very little while two ſolid pieces of it (not, as I remember, of the finer ſort) one againſt the other, they would not only yield a ſenſible odour, but ſometimes ſo ſtrong an one, as to be offenſive. By which you will eaſily perceive, why I told you above, that I did not acquieſce in the Carteſian argument againſt electrical bodies performing their operations by emanations of their own ſubſtance, drawn from hence, that glaſs does attract light bodies, as indeed it does, though but weakly; and yet is too fixed to emit effluviums, the contrary of which ſuppoſition the lately mentioned experiment, and by us often repeated, does ſufficiently evince.

From what other ſolid bodies, and that will endure the fire, I have, or have not been able to obtain ſuch odorous ſteams, it is not neceſſary to declare in this place, but may perhaps be done in another.

You may, I preſume, have taken notice, that, according to what I intimated a while ago, I have forborn in the precedent examples to mention thoſe effluvia of ſolid bodies, that need the action of the fire to be obtained. But ſince the ſun is the grand agent of nature in the planetary world, and ſince during the ſummer, and eſpecially at noon, and in ſouthern climates, his heat makes many bodies have little atmoſpheres, that we cannot ſo well diſcern that they have conſtantly; I ſee not, why I may not be allowed to aſcribe atmoſpheres to ſuch bodies, as I have obſerved to have them, when the ſun ſhines upon them; and alſo to think, that the like may be attributed at leaſt ſometimes to ſuch other bodies, as will do the things uſually performed by effluviums, when yet they are excited but by an external heat, which exceeds not that of the hot ſun.

Of theſe two ſorts of bodies I ſhall for brevity's ſake name but two or three examples, and then haſten to a concluſion.

The firſt of theſe I muſt make bold to borrow from my obſervations about electricity, among which this is one, that to ſhew, that the particular and uſual manner of exciting ſuch bodies, namely by rubbing them, is not always neceſſary; I took a large piece of good amber, and having in a ſummer morning, whilſt the air was yet freſh, tried, that it would not without being excited attract a light body I had expoſed to it; I removed it into the ſun's beams, till they had made it moderately hot, and then I found according to my expectation, that it had acquired an attractive virtue, and that not only in one particular place, as is uſually obſerved, when it is excited by rubbing, but in divers and diſtant places at once; at any of which it would draw to it the light body placed within a convenient diſtance from it; ſo that even in this climate of ours a ſolid body may quickly acquire an atmoſphere by the preſence of the ſun, and that long before the warmeſt part of the day.

The next inſtance you will perchance think ſomewhat ſtrange, it being that, when for want of an opportunity to make the like trial in the warm ſun, I took a little but thick veſſel made of glaſs, and held it near the fire, till it had got a convenient degree of heat, (which was not very great, though it exceeded that which I had given the amber) I found, as I had imagined, that the heat of fire had made even this body attractive, as that of the ſun had made the other.

What degree of heat I have obſerved to be either neceſſary, or the moſt convenient to excite electrical bodies, according to their different natures, (for the ſame degree will not indifferently ſerve for them all) this is not the propereſt place to declare, that it will be more to our preſent purpoſe to make ſome ſhort reflection on what has been hitherto delivered.

It ſeems then probably deduceable from the foregoing experiments and obſervations, that a very great number, if not the greateſt part, even of conſiſtent bodies, whether animal, vegetable, or mineral, may emit effluviums, and that even thoſe that are ſolid may, at leaſt ſometimes, have their little atmoſpheres, though the neighbouring ſolids will often keep the evaporations from being every way ambient in reference to the bodies they iſſue from.

For as the inſtances hitherto alledged (which are not all that I could have named) do plainly ſhew, that divers bodies, and ſome that have not been thought very likely, are ſuch as we ſpeak of; ſo ſeveral things induce me to believe, that there may be many more of the like nature.

For firſt, very few, if any, have (that I know of) had the curioſity to make uſe of nice ſcales, which ſuch trials require, to examine the expirations of inanimate bodies, which if they ſhall hereafter do, I make little doubt, but they will light on many things, that will confirm what we have been propoſing, by their finding, that ſome bodies, which are not yet known to yield exhalations, do afford them, and that many others do part with far more copious ones than is imagined. For one would not eaſily have thought, that ſo extremely cold a body as a ſolid piece of ice ſhould make a plentiful evaporation of itſelf in the cold air of a freezing night; or that a piece of wood, that had long lain in the houſe, and was light enough to be conveniently hung for a long time at a ballance, that would loſe its æquilibrium with, as I remember, half a quarter of a grain, ſhould in leſs than a minute of an hour ſend forth ſteams enough to make the ſcales manifeſtly turn, and that in winter.

But ſuppoſing (which is my ſecond conſideration) that trials were made with good inſtruments for weighing, though it will follow, that in caſe the expoſed body grow lighter, ſomething exhales from it; yet it will not follow, that if no diminution of weight be diſcovered by the inſtrument, nothing that is corporeal recedes from it. I will not urge, that it is affirmed, not only by the generality of our chemiſts, but by learned modern phyſicians, that when either glaſs of antimony, or crocus metallorum impregnate wine with vomitive and purgative particles, they do it without any decrement of their weight; becauſe the ſcales in apothecaries ſhops, and the little accurateneſs wont to be employed in weighing things, by thoſe that are not verſed in ſtatical affairs, made me, though not deny the tradition, which may perchance be true, yet, unwilling to build upon obſervations, which to be relied on are to be very nicely made; and therefore I ſhall rather take notice, that though the loadſtone be concluded to have conſtantly about it a great multitude of magnetical effluvia, which may be called its atmoſphere, yet it has not been obſerved to loſe any thing of its weight by the receſs of ſo many corpuſcles. But becauſe, if the Carteſian hypotheſis about magnetiſms be admitted, the argument drawn from this inſtance will not be ſo ſtrong as it ſeems, and as it otherwiſe would be; I ſhall add a more unexceptionable example, for I know you will grant me, that odours are not diffuſed to a diſtance without corporeal emanations from the odorous body: and yet, though good amber-greaſe be, even without being excited by external heat, conſtantly ſurrounded by a large atmoſphere, you will in one of the following diſcourſes find cauſe to admire, how inconſiderable the waſte of it is.

If it be ſaid, that in tract of time a decrement of weight may appear in bodies, that in a few hours or days diſcovers not any; the objection, if granted, overthrows not our doctrine, it being ſufficient to eſtabliſh what we have been ſaying, if we have evinced, that the effluvia of ſome bodies may be ſubtle enough not to make the body by their avolation appear lighter in ſtatical trials, that are not extraordinarily (and as it were obſtinately) protracted. And this very objection puts me in mind to add, that for ought we know the decrement of bodies, in ſtatical experiments long continued, may be ſomewhat greater, than even nice ſcales diſcover to us; for how are we ſure, that the weights themſelves, which are commonly made of braſs, (a metal very unfixed,) may not in tract of time ſuffer a little diminution of their weight, as well as the bodies counterpoiſed by them? and no man has I think yet tried, whether glaſs, and even gold may not in tract of time loſe of their weight, which in caſe they ſhould do, it would not be eaſily diſcovered, unleſs we had bodies, that were perfectly fixed, by compariſon to which we might be better aſſiſted, than by comparing them with braſs weights, or the like, which being themſelves leſs fixed, will loſe more than gold and glaſs?

My third and laſt conſideration is, that there may be divers other ways, beſides thoſe furniſhed us by ſtaticks, of diſcovering the effluvia of ſolid bodies, and conſequently of ſhewing, that it is not ſafe to conclude, that becauſe their operation is not conſtant or manifeſt, ſuch bodies do never emit any effluvia at all, and ſo are uncapable to work by their intervention on any other body, though never ſo well diſpoſed to receive their action. And this I the rather deſire that you would take notice of, becauſe my chief (though not only) deſign in thoſe notes is (you know) to illuſtrate the doctrine of occult qualities; and it may conduce to explicate ſeveral of them, to know, that ſome particular bodies emit effluvia, though perhaps they do it not conſtantly, and uniformly; and though perchance to, they do not appear to emit any at all, if they be examined after the ſame manner with other exhaleable bodies, but only may be made to emit them by ſome peculiar way of handling them, or appear to have emitted them by ſome determinate operation on ſome other ſingle body, or at moſt ſmall number of bodies.

Perchance you did not think, until you read what I lately told you about glaſs, that from a body, that had endured ſo violent a fire, there could, by ſo ſlight a way as rubbing a little while one piece againſt another, be obtained ſuch ſteams, as may not only affect but offend the noſtrils. Nor ſhould we eaſily believe, if experience did not aſſure us of it, that a diamond, that is juſtly reputed the hardeſt known body in the world, ſhould by a little rubbing be made to part with electrical effluvia. Nay, (that I may give ſome kind of confirmation to that part of the laſt paragraph that ſeems moſt to need it,) I ſhall add, that I once had a diamond not much bigger than a large pea, which had never been poliſhed or cut, whoſe electrical virtue was ſometimes ſo eaſily excited, that if I did but paſs my fingers over it to wipe, the virtue would diſcloſe itſelf; and if as ſoon as I had taken it out of my pocket, I applied a hair to it, though I touched not the ſtone with my fingers, that I might be ſure not to rub it, that hair would be attracted at ſome diſtance, and many times one after another, eſpecially by one of the ſides of the ſtone, (whoſe ſurface was made up of ſeveral almoſt triangular planes;) and though this excitation of the diamond ſeemed to proceed only from the warmth that it had acquired in my pocket, yet I did not find that that warmth, though it ſeemed not to be altered, had always the ſame effect on it, though the wiping it with my finger failed not (that I remember) to excite it. Something like this uncertainty I always obſerved in another diamond of mine, that was much nobler than the firſt, and very well poliſhed, and in a ſmall ruby, that I have yet by me, which would ſometimes be conſiderably electrical without being rubbed, when I but wore the ring it belonged to on my little finger; and ſometimes again it ſeemed to have loſt that virtue of operating without being excited by friction, and that ſometimes within a few minutes, without my knowing whence ſo quick a change ſhould proceed. But I muſt inſiſt no longer on ſuch particulars, of which I elſewhere ſay ſomething; and therefore I proceed to take notice, that we ſhould ſcarce have dreamed, that when a partridge or hunted deer has caſually ſet a foot upon the ground, that part, where the footſtep hath been (though inviſibly) impreſſed, ſhould continue for many hours a ſource of corporeal effluxes; if there were not ſetting dogs, and ſpaniels, and bloodhounds, whoſe noſes can take notice at that diſtance of time of ſuch emanations, though not only other ſorts of animals, but other ſorts of dogs are unable to do ſo.

I ſaw a ſtone in the hands of an academick, an acquaintance of mine, which I ſhould by the eye have judged to be an agate, not a bloodſtone; and conſequently I ſhould not have thought, that it could have communicated medicinal effluvia appropriated to exceſſive bleedings, if the wearer of it had not been ſubject to that diſeaſe, and had not often cured both himſelf and others, by wearing this ſtone about his neck; which if he left off, as ſometimes he did for trial's ſake, his exceedingly ſanguine complexion (to which I have rarely ſeen a match) would in a few days caſt him into relapſes. What I have elſewhere told you about the true virtues of ſome ſtones, (for I fear that moſt of thoſe that are wont to be aſcribed to them are falſe,) may give ſome confirmation to what I have been delivering, which I cannot now ſtay to do, being to draw to a concluſion as ſoon as I have put you in mind, that it would not probably have ever been expected, that ſo ponderous and ſolid a body as the loadſtone ſhould be invironed by an atmoſphere, if iron had been a ſcarce mineral, and had not chanced to have been placed near it.

And with this inſtance I ſhall put an end to theſe notes, becauſe it allows me to make this reflexion; that ſince ſolid bodies may have conſtant atmoſpheres about them, and yet not diſcover, that they have ſo, but by their operation upon one particular body, or thoſe few which participate of that; and ſince there are already (as we have ſeen) very differing ways, whereby bodies may appear to be exhaleable, it is not unlikely, that there may be more and more bodies (even of thoſe that are ſolid and hard) found to emit effluvia, as more and more ways of diſcovering, that they do ſo, ſhall either by chance or induſtry be brought to light.

AN

INVENTION

FOR

Eſtimating the Weight of Water in Water, with ordinary Balances and Weights.

Firſt Printed in the PHILOSOPHICAL TRANSACTIONS, No. L. p. 1001.

“THE author of this invention is the noble *Robert Boyle*; who was pleaſed to comply with our deſires of communicating it in Engliſh to the curious in *England*, as by inſerting the ſame in the Latin tranſlation of his Hydroſtatical Paradoxes he hath gratified the ingenious abroad. And it will doubtleſs be the more welcome, for as much as no body, we know of, hath ſo much as attempted to determine, how much water may weigh in water; and poſſibly, if ſuch a problem had been propoſed, it would have been judged impracticable.

“THE method or expedient he made uſe of to perform it, as near as he could, may be eaſily learned by the enſuing account of a trial or two he made for that purpoſe, which among his notes he cauſed to be regiſtered in the following words.”

A glaſs-bubble of about the bigneſs of a pullet's-egg was purpoſely blown at the flame of a lamp, with a ſomewhat long ſtem turned up at the end, that it might the more conveniently be broken off. This bubble being well heated to rarefy the air, and thereby drive out a good part of it, was nimbly ſealed at the end, and by the help of the figure of the ſtem was by a convenient weight of lead depreſſed under water, the lead and glaſs being tied by a ſtring to one ſcale of a good ballance, in whoſe other there was put ſo much weight, as ſufficed to counterpoiſe the bubble, as it hung freely in the midſt of the water. Then with a long iron forceps I carefully broke off the ſealed end of the bubble under water, ſo as no bubble of air appeared to emerge or eſcape through the water, but the liquor by the weight of the atmoſphere ſprung into the unrepleniſhed part of the glaſs-bubble, and filled the whole cavity about half full; and preſently, as I foretold, the buble ſubſided and made the ſcale, it was faſtened to, preponderate ſo much, that there needed four drachms and 38 grains to reduce the balance to an æquilibrium. Then taking out the bubble with the water in it, we did, by the help of the flame of a candle warily applied, drive out the water (which otherwiſe is not eaſily excluded at a very narrow ſtem) into a glaſs counterpoiſed before; and we found it, as we expected, to weigh about four drachms and thirty grains, beſides ſome little that remained in the egg, and ſome ſmall matter that may have been rarefied into vapours, which added to the piece of glaſs, that was broken off under water and loſt there, might very well amount to ſeven or eight grains. By which it appears, not only that water hath ſome weight in water, but that it weighs very near * or altogether as much in water, as the ſelf ſame portion of liquor would weigh in the air.

THE ſame day we repeated the experiment with another ſealed bubble, larger than the former, being as big as a great hen-egg, and having broken this under water, it grew heavier by ſeven drachms and thirty-four grains; and having taken out the bubble, and driven out the water into a counterpoiſed glaſs, we found the tranſvaſated liquor to amount to the ſame weight, abating ſix or ſeven grains, which it might well have loſt upon ſuch accounts, as have been newly mentioned.

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* This expreſſion was added, to leave liberty for a further inquiry, whether the experiment, which hereby appears convincing as to the main thing intended to be proved, may not admit the having ſomething further debated, and annexed about ſome circumſtantial thing or other.

TRACTS

ABOUT

The Coſmical Qualities of Things.
Coſmical Suſpicions.
The Temperature of the Subterraneal Regions.
The Temperature of the Submarine Regions.
The Bottom of the Sea.

To which is prefixed,

An Introduction to the Hiſtory of PARTICULAR QUALITIES.

An ADVERTISEMENT of the PUBLISHER to the READER.

THOUGH the noble author of the following tracts hath alſo written divers other ſhort diſcourſes, upon ſeveral occaſions, yet, had he not been diverted from his purpoſe, he had continued to let them lie by him, intending, in caſe he ſhould ſuffer them to come abroad, to diſpoſe of them agreeably to a deſign, that it is not neceſſary the reader ſhould be now acquainted with.

IN the mean while, ſeveral virtuoſi, to whom ſome of theſe tracts had been ſhewn, and with whom the matters handled in ſome others had been diſcourſed, did out of a concern, (as they gave out,) for the common-wealth of learning, preſſingly repreſent to the author:

FIRST, that divers of theſe looſe tracts, having little or no dependency upon one another, might without inconvenience be publiſhed apart, in what number and order the author ſhould pleaſe.

SECONDLY, that ſince his main deſign in theſe, as well as his other phyſical writings, was to provide materials for the hiſtory of nature, it would be thought enough, that they be ſubſtantial and fit for the work; in what order or aſſociation ſoever they ſhould happen to be brought into the philoſophical repoſitory.

THIRDLY, that the communicating theſe tracts to the curious would be the beſt way to ſecure them from being loſt or embezelled, as ſome others of his papers have been, not only formerly, but very lately.

FOURTHLY, that the kind reception, the curious had given to what he had hitherto preſented them, might well invite, if it did not oblige him, not to envy them the early uſe of thoſe experiments and hints, which will probably, before the time, wherein his deſign would ſuffer them to come abroad, prove ſerviceable to philoſophy, by ſetting divers inquiſitive heads on work, exciting the curioſity of ſome, and exerciſing the induſtry of others.

LASTLY, that, as of the peices, he had hitherto publiſhed (except where his own backwardneſs had expreſſedly interpoſed) the firſt edition had not long been the only; ſo probably within a moderate ſpace of time, another edition of thoſe tracts, he ſhould firſt put out, would both allow him to increaſe their number, and change their order as he ſhould judge moſt expedient, and, (in caſe he ſhould in the mean while return to his library,) recruit his diſcourſes with thoſe paſſages, that he deſigned to borrow for them thence.

BUT, though theſe conſiderations, joined to the earneſtneſs of the perſons that made them, and the juſt reſpect he had for them, rendered it uneaſy for him to reſiſt their perſwaſions; yet they never obtained an actual compliance, until they were aſſiſted by ſuch an unhappy juncture of ſickneſs and buſineſs, as, leaving him ſmall hopes of accompliſhing his firſt intentions in any reaſonable time, made him content to ſend away to the preſs ſome of thoſe tracts, that he found the leaſt unready for it, in the order wherein they chanced to come to his hands. Which being thus repreſented, the conſidering and ingenious reader will ſoon find, what cauſe there is, and how much it concerns the advancement of valuable philoſophy, that, ſince this excellent author hath (to the publiſher's knowledge, as alſo was inſinuated above) many other rare tracts of a philoſophical nature in ſtore, he be ſollicited from time to time, that he would be pleaſed, according to the meaſure of health he ſhall enjoy, to impart with all poſſible ſpeed thoſe diſcourſes, which tend to the enlargement and progreſs of uſeful knowledge, maugre all envy and malice. H. O.

The

THE HISTORY OF PARTICULAR QUALITIES.

CHAP. I.

THE paſt diſcourſe has, I hope, *Pyroph.* given you ſome tolerable account both of the nature, and of the origin of qualities in general. Wherefore it now follows, that we proceed to qualities in particular, and conſider how far the manner, whereby they are produced, and thoſe other phænomena of them, that we ſhall have occaſion to take notice of, will accord with, and thereby confirm the doctrine I have hitherto propoſed; and whether they will not, at leaſt, much better comport with that, then with the opinions either of the peripateticks, or the chymiſts.

I ſhall not ſpend time to enquire into all the ſeveral ſignifications of the word quality, which is uſed in ſuch various ſenſes, as to make it ambiguous enough: ſince by the ſubſequent diſcourſe it will ſufficiently appear, in which of the more uſual of thoſe ſignifications we employ that term. But thus much I think it not amiſs to intimate in this place, that there are ſome things, that have been looked upon as qualities, which ought rather to be looked upon as ſtates of matter, or complexions of particular qualities, as animal, inanimal, &c. health and beauty, which laſt attribute ſeems to be made up of ſhape, ſymmetry, or comely proportion, and the pleaſantneſs of the colours of the particular parts of the face. And there are ſome other attributes, namely, ſize, ſhape, motion, and reſt, that are wont to be reckoned among qualities, which may more conveniently be eſteemed the primary modes of the parts of matter; ſince from theſe ſimple attributes, or primordial affections, all the qualities are derived. But this conſideration relating to words and names, I ſhall not inſiſt upon it.

NOR do I think it worth while to enumerate and debate the ſeveral partitions, that have been made of qualities, (of which I have met with divers, and could perchance my ſelf encreaſe the number of them,) for though one, that were diſpoſed to criticiſe upon them, would not perhaps acquieſce in any of them, but look upon them as being more arbitrary, than grounded upon an attentive conſideration of the nature of the things themſelves; yet becauſe it ſeems not to me ſo eaſy to make an accurate diſtribution of qualities, till ſome things that concern them be better cleared up than yet they are, I ſhall content my ſelf for the preſent, to propoſe to you one of the more received diviſions of phyſical qualities, (for you know, I do not pretend to treat of any other) allowing my ſelf the liberty of making, where there ſeems cauſe, the members of the diſtribution ſomewhat more comprehenſive. We will then, with many of the moderns, divide phyſical qualities into manifeſt and occult; and reſerving the latter to be treated of apart, we will diſtribute the former into firſt, ſecond, and third; to the two laſt of which we will reſerve divers qualities not wont to be treated of by ſchool writers of phyſical ſyſtems, which, for diſtinction ſake, we may without much inconvenience ſtile ſome of them the chemical qualities of things, becauſe as *Ariſtotle* and the ſchool-men were not acquainted with them, ſo they have been principally introduced and taken notice of by means of chemical operations and experiments; ſuch as are fumigation, amalgamation, cupellation, volatilization, precipitation, &c. by which operations, among other means, corporeal things come to appear volatile or fixed, ſoluble or inſoluble in ſome menſtruums, amalgable or unamalgamable, capable or uncapable to precipitate ſuch bodies, or be precipitated by them, and, in a word, acquire or looſe ſeveral powers to act on other bodies, or diſpoſitions to be wrought on by them; which attributes do as well deſerve the name of qualities, as divers other attributes, to which it is allowed. And to theſe chemical qualities we may add ſome others, which becauſe of the uſe, that phyſicians either only, or above other men, make of them, may be called medical, whereby ſome bodies taken into that of a man are deoppilating, others inciding, reſolving, diſcuſſing, ſuppurating, abſterſive of noxious adherences, and thickning the blood and humours, being aſtringent, anodinous or appeaſing pain, &c. For though ſome of the faculties of medicines, as thoſe of heating, cooling, drying, attenuating, purging, &c. may be conveniently enough referred to the firſt, ſecond, or third qualities wont to be mentioned by naturaliſts; and others are wont to be reckoned among occult ones; and though theſe medical qualities are wont to be treated of by phyſicians; yet it ſeems to me, that divers of them ought not to be referred to the qualities, to which they are wont to be ſo; and the handling of them may be looked upon as a deſideratum in natural philoſophy, and may well enough deſerve a diſtinct place there; ſince the writers of that ſcience are not wont to treat of them at all, and phyſicians handle them as phyſicians, whom it concerns but to know what bodies are endowed with them, and what good or ill effects they may have upon humane bodies, not as naturaliſts, whoſe buſineſs it is to enquire into the production and cauſes of thoſe as well as of other qualities.

CHAP. II.

BEFORE we deſcend to the mention of any of theſe particular qualities, I think it very expedient to ſpend a little time in conſidering three grand ſcruples about our and the corpuſcularian doctrine touching qualities, which three difficulties, though I remember not to have found them expreſly objected by the adverſaries of the corpuſcularian philoſophy, nor (perhaps only for that reaſon) to have been purpoſely ſolved by the patrons of it; are yet ſuch, that having been ſuggeſted to me by conſidering the nature of the thing, I cannot but fear, that they alſo may occur to, and trouble you; ſince they ſeem to me of that importance, that unleſs they be removed, they may very much prejudice the reception of a good part of what I am to deliver about particular qualities.

THE firſt of the above mentioned objections is grounded upon the received opinion of vulgar and Ariſtotelian philoſophers, that diverſity of qualities muſt needs flow from ſubſtantial forms, either becauſe it is part of their nature to be the principals of properties, and peculiar operations in the bodies they inform; or elſe becauſe divers of them are ſuch, that no mixture of the elements is capable of producing them.

OF the two ſuppoſitions, whereon this difficulty is founded, we have already ſhewed the former to be unfit to be admitted, by what has been ſaid in our examen of ſubſtantial and ſubordinate forms; and therefore it will only remain, that we examine alſo this ſecond ſuppoſition, which may therefore deſerve the greater conſideration, becauſe it is much preſſed and relied by the learned *Sennertus,* (and his followers,) who improves the argument by this addition, that as no bare mixture of the elements, ſo no general *forma miſtionis* (ſuch as divers of the moderns have introduced to help out the hypotheſis) is ſufficient to give an account of divers qualities, which he ſomewhere reckons up.

BUT, in the firſt place, whereas the propoſers of this difficulty take it for granted, that there are four elements, from whoſe various mixtures all other ſublunary bodies ſpring; and are therefore only ſolicitous to prove, that ſuch and ſuch qualities cannot flow from their mixture; I need not much concern my ſelf for their whole diſcourſe, ſince I admit not that hypotheſis of the four elements, that is ſuppoſed in it; and yet I may be allowed to obſerve from hence, that by the confeſſion of thoſe modern peripateticks, that urge this argument, thoſe ancient and other Ariſtotelians were miſtaken, who aſcribed to the mixture of the elements effects, for which theſe maintain them to be incompetent.

BUT ſince replies of this nature do rather concern the objectors than the objection, I proceed to conſider the difficulty it ſelf, not only as it may be propoſed by peripateticks, but by chemiſts; who though ſome of them do not with others of their ſect allow of the four elements, do yet agree with the ſchools in this, that there is a determinate number of ingredients of compounded bodies, from whoſe mixture and proportion many qualities muſt be derived; and thoſe, that cannot, muſt be reſolved to flow from a higher principle, whether it be a ſubſtantial form, or ſomething, for which chemiſts have ſeveral names, though I doubt no ſettled and intelligible notion.

To conſider then the difficulty it ſelf, I ſhall for the removal of it preſent to you four principal conſiderations.

BUT before I begin by any of theſe to anſwer the objection, I ſhall readily acknowledge, that in ſome reſpects, and in ſome caſes, it may not be ill grounded: but I ſhall add, that in thoſe caſes I look upon it rather as a part of the corpuſcularian doctrine, than an objection againſt it; for when it happens, that there is a ſtrict connexion betwixt that modification of matter, which is requiſite to exhibit one phænomenon, and that from which another will neceſſarily follow; in ſuch caſe we may not only grant, but teach, that he, who by a change of its texture gives a portion of matter the former modification, does likewiſe qualify it by the ſame change to exhibit the congruous phænomenon; though one would not perchance ſuſpect them to have any ſuch dependance upon one another. As for inſtance, ſtrong ſpirit of diſtilled vinegar, by virtue of its being an acid ſpirit, hath the faculty to turn ſyrup of violets red: but if by making with this ſpirit as ſtrong a ſolution as you can of corral, or ſome ſuch body, you deſtroy the acidity of the ſpirit of vinegar; this liquor, as it has quite another taſte, ſo it may, and indeed will have another operation than formerly upon ſyrup of violets. For I remember, that upon a trial I purpoſely deviſed to illuſtrate this matter, I found, that the lately mentioned ſolution, and ſome others made with ſpirit of vinegar, would preſently like an alkalizate or urinous ſalt turn ſyrup of violets from its native blue, not any longer into a red, but into a lovely green. And proſecuting the experiment a little farther, I found, that ſpirit of ſalt it ſelf deflegmed by a fit concrete, though the ſolution were horribly ſtrong, had yet the ſame effect on ſyrup of violets. But becauſe the caſes, where the above-mentioned connection of qualities and modifications occur, are comparatively but few, I ſhall here conſider them no farther, but proceed to the four particulars I was lately propoſing.

AND in the firſt place, I ſay, that things may acquire by mixture very differing qualities from thoſe of any of the ingredients.

OF this I ſhall have occaſion to give a multitude of inſtances in the following notes upon particular qualities; and therefore it may now ſuffice to mention two or three, that are the more obvious in the laboratories of chemiſts; as, that ſugar of lead is extreamly ſweet, though the minium, and the ſpirit of vinegar of which it is made, be the former of them inſipid, and the latter ſour. And though neither aqua regis, nor crude copper, have any thing in them of blue; yet the ſolution of this metal in that liquor is of a deep blue; and ſometimes I have had the ſolution of crude mercury in good aqua fortis of a rich green, though it would not long continue ſo. And of ſuch inſtances you will, as I was ſaying, hereafter meet with plenty. So that they are much miſtaken, who imagine either, that no manifeſt qualities can be produced by mixture, except thoſe that reſide in the elements, or reſult immediately from the combinations of the four firſt qualities. For not to repeat what variations the mixtures of the moſt ſimple ingredients only may produce; it is manifeſt, that nature and art muſt continually make mixtures of bodies, both of already compounded bodies, as when aſhes and ſand compoſe the common coarſe glaſs, and when nature combines ſulphur with unripe vitriol, and perhaps other ſubſtances in a marchaſite; and alſo of bodies already decompounded, as native vitriol is made in the bowels of the earth of an aqueous liquor impregnated with an acid ſalt, and of a cupreous or martial mineral, ſtrictly united both to a combuſtible ſulphureous ſubſtance, and to another body of a more fixed terreſtrial nature. And thus artificers may eaſily, as trial hath aſſured me, produce new and fine colours, by ſkilfully mixing in the flame two pieces of ammels (which are already decompounded bodies) of colours more ſimple or primary than that, which reſults from their colliquation. And this way of ſo combining bodies, not ſimple or elementary, will be acknowledged capable of being made much more fertile in the production of various qualities and phænomena of nature, if you conſider, how much the variation of the proportion of the ingredients in a mixed body may alter the qualities and operations of it, and that proportion is capable of being varied almoſt *in infinitum*. Thus much may ſuffice for our firſt conſideration; eſpecially ſince divers things, by which it may be much confirmed, will be met with in the two following chapters.

IN the ſecond place I obſerve, that it is but an ill grounded hypotheſis to ſuppoſe, that new qualities cannot be introduced into a mixed body, or thoſe that it had before be deſtroyed, unleſs by adding or taking away a ſenſible portion of ſome one or more of the Ariſtotelian elements, or chemical principles. For there may be many changes, as to quality, produced in a body without viſibly adding, or taking away any ingredient, barely by altering the texture, or the motion of the minute parts it conſiſts of. For when (for inſtance) water hermetically ſealed up in a glaſs is by the cold of the winter turned into ice, and thereby both looſeth its former fluidity, and tranſparency, and acquires firmneſs, brittleneſs, and oftentimes opacity, all which qualities it looſeth again upon a thaw; in this caſe, I ſay, I demand, what element or hypoſtatical principle can be proved to get into, or out of this ſealed glaſs, and by its intruſion and receſs produce theſe alterations in the included body. And ſo in that fixed metal, ſilver, what ſenſible acceſſion or decreement can be proved to be made as to ingredients, when by barely hammering it (which doth but change the ſituation and texture of the parts) it acquires a brittleneſs, which by ignition, wherein it doth not ſenſibly looſe any thing, it may preſently be made to exchange for its former malleableneſs? And the ſame experiment gives us an inſtance alſo, that the inviſible agitation of the parts may alone ſuffice, to give a body, at leaſt for a while, new qualities; ſince a thick piece of ſilver nimbly hammered will quickly acquire a conſiderable degree of heat, whereby it will be enabled to melt ſome bodies, to dry others, and to exhibit divers phænomena, that it could not produce when cold. I might add, that ſpirit of nitre, moderately ſtrong, though when included in a well ſtoped vial in the form of a liquor it will appear diaphanous, and without any redneſs, will yet fill the upper part of the vial with red fumes, if the warm ſun-beams or any fit heat (though but externally applied, and though the glaſs continue cloſe ſtopped) do put the nitrous ſpirits into a ſomewhat briſker motion, than they had or needed whilſt in the form of a liquor. I might alſo demand, both what new element or principle is added to a needle, when the bare approach of a vigorous loadſtone endows it with thoſe admirable qualities of reſpecting the poles, and (in due circumſtances) drawing to it other needles; and what ingredient the ſteel looſes, when by a contrary motion of the loadſtone it is in a minute deprived of its magnetiſm. And to theſe I might ſubjoin divers like queſtions: but of inſtances and reflections proper to confirm this ſecond conſideration, you may meet with ſo many, partly in another treatiſe, and partly in the enſuing chapters, that it will be needleſs to multiply them here. Wherefore in the third place, I ſhall obſerve, that when we are conſidering, how numerous and various phænomena may be exhibited by mixed bodies, we are not to look upon them preciſely in themſelves; that is, as they are portions of matter, of ſuch a determinate nature, or texture; but as they are parts of a world ſo conſtituted as ours is, and conſequently as portions of matter, which are placed among many other bodies. For being hereby fitted to receive impreſſions from ſome of thoſe bodies, and to make impreſſions upon others of them, they will upon this account be rendered capable of producing, either as principal, or auxiliary cauſes, a much greater number and variety of phænomena, than they could exhibit, if each of them were placed in *vacuo*, (or if a vacuum be a thing impoſible) in a medium, that could no way either contribute to, or hinder its operations.

THIS hath been partly proved already in the diſcourſe of the origin of forms, and will be farther manifeſted ere long; and therefore it may ſuffice, that of the particulars mentioned in thoſe writings, thoſe that are pertinent to this argument be mentally referred hither.

Wherefore having thus diſpatched the third conſideration, I now proceed to the fourth and laſt, which is, that the four peripatetick elements and the three chemical principles are ſo inſufficient to give a good account of any thing near all the differing phænomena of nature, that we muſt ſeek for ſome more catholick principles; and that thoſe of the corpuſcularian philoſophy have a great advantage of the other in being far more fertile and comprehenſive than they. I muſt not here ſtay to make full repreſentation of the deficiencies of the Ariſtotelian hypotheſis, having in other tracts ſaid much to that purpoſe already; but yet our preſent argument invites me to intimate theſe two things; the firſt, that ſuch phænomena, as the conſtant and determinate ſhape and figure of the mountains, our teleſcopes diſcover (together with their ſhadows) in the moon, and the ſtrange generation and periſhing of the ſpots of the ſun, to omit the differing colour of the planets, and divers other qualities of celeſtial bodies, cannot be aſcribed to the four elements, or their mixtures, nor to thoſe of the three chemical principles, which are allowed to be confined to the ſublunary region. And the ſecond, that there are very many phaenomena in nature (divers of which I * elſewhere take notice of) ſeveral whereof neither the peripatetick nor the chemical doctrine about the elements, or the ingredients of bodies, will enable a man to give ſo much as any probable account. Such are the eclipſes of the ſun, the moon, and alſo the ſatellites of Jupiter, the proportion of the acceleration of deſcent obſervable in heavy bodies, the ebbing and flowing of the ſea, a great number of magnetical, muſical, ſtatical, dioptrical, catoptrical, and other ſorts of phænomena, which haſte makes me here leave unmentioned.

And having ſaid thus much about the firſt part of our propoſed conſideration, and thereby ſhewn, that the vulgar doctrine about the ingredients of bodies falls very ſhort of being able to ſolve ſeveral kinds of natures phænomena; we may add in favour of the ſecond part, that, it will follow in general, that it is fit to look out for ſome more pregnant and univerſal principles; and that, in particular, thoſe of the corpuſculary hypotheſis are, as to thoſe two attributes, preferable by far to the vulgar ones, will I hope appear by our anſwers to the two objections, that remain to be examined in the two following chapters, to which that I might the more haſten, I thought fit to inſiſt the leſs upon the objection hitherto examined, eſpecially becauſe partly in this and the two next chapters, and partly elſewhere, I ſuppoſe there is contained a very ſufficient reply to that objection. And I confeſs I ſhould think it ſtrange, that the conſideration of the various motions and textures of bodies ſhould not ſerve to ſolve far more phænomena, than the bare knowledge of the number (and even that of the proportions) of their quieſcent ingredients: for as local motion is that, which enables natural bodies to act upon one another, ſo the textures of bodies are the main things, that both modify the motion of agents, and diverſify their effects according to the various natures of the patients.

CHAP. III.

I ENTER now upon the conſideration of the ſecond, and indeed the grand difficulty objected againſt the (corpuſcularian) doctrine propoſed by me about the origin of qualities, viz. that it is incredible, that ſo great a variety of qualities, as we actually find to be in natural bodies, ſhould ſpring from principles ſo few in number as two, and ſo ſimple as matter and local motion; whereof the latter is but one of the ſix kinds of motion reckoned up by *Ariſtotle* and his followers, who call it lation, and the former, being all of one uniform nature, is according to us diverſified only by the effects of local motion. Towards the ſolving this difficulty, I ſhall endeavour to ſhew, firſt, that the other catholick affections of matter are manifeſtly deducible from local-motion: and next, that theſe principles being variouſly aſſociated, are ſo fruitful, that a vaſt number of qualities and other phænomena of nature may reſult from them.

The firſt of theſe will not take us up much time to make out. For ſuppoſing, what is evident, that the (1) local-motion belonging to ſome parts of the univerſal matter, does not all tend the ſame way, but has various determinations in ſeveral parts of that matter; it will follow, that by local-motion thus circumſtanced, matter muſt be divided into diſtinct parts; each of which being finite, muſt neceſſarily be of ſome (2) bigneſs or ſize, and have ſome determinate (3) ſhape or other.

And ſince all the parts of the univerſal matter are not always in motion, ſome of them being arreſted by their mutual implication, or having transferred (as far as our ſenſes inform us) all that they had to other bodies, the conſequence will be, that ſome of theſe portions of the common matter will be, in a ſtate of (4) reſt (taking the word in the popular ſence of it.) And theſe are the moſt primary and ſimple affections of matter.

But becauſe there are ſome others, that flow naturally from theſe, and are, though not altogether univerſal, yet very general and pregnant; I ſhall ſubjoin thoſe, that are the moſt fertile principles of the qualities of bodies and other phænomena of nature.

Moreover, then, not only the greater fragments of matter, but thoſe leſſer ones, which we therefore call corpuſcles or particles have certain local reſpects to other bodies, and to thoſe ſituations, which we denominate from the horizon; ſo that each of theſe minute fragments may have a particular (5) poſture, or poſition (as erect, inclining, horizontal, &c.) and as they reſpect us men, that behold them, there may belong to them a certain (6) order or conſecution, upon whoſe ſcore we ſay one is before or behind another; and many of theſe fragments being aſſociated into one maſs or body, have a certain manner of exiſting together, which we call (7) texture, or by a word more comprehenſive, modification. And becauſe there are very few bodies, whoſe conſtituent parts, can, becauſe of the irregularity or difference of their figures, and for other reaſons, touch one another every where ſo exquiſitely, as to leave no intervals between them, therefore almoſt all conſiſtent bodies, and thoſe fluid ones, that are made up of groſſer parts, will have (8) pores in them, and very many bodies having particles, which by their ſmallneſs, or their looſe adherence to the bigger, or more ſtable parts of the bodies they belong unto, are more eaſily agitated and ſeparated from the reſt by heat and other agents; therefore there will be great ſtore of bodies, that will emit thoſe ſubtle emanations, that are commonly called (9) effluviums. And as thoſe conventions of the ſimple corpuſcles, that are ſo fitted to adhere to, or be complicated with one another, conſtitute thoſe durable and uneaſily diſſoluble cluſters of particles, that may be called the primary concretions or elements of things: ſo theſe themſelves may be mingled with one another, and ſo conſtitute compounded bodies; and even thoſe reſulting bodies may by being mingled with other compounds, prove the ingredients of decompounded bodies; and ſo afford a way, whereby nature varies matter, which we may call (10) mixture, or compoſition; not that the name is ſo proper as to the primary concretions of corpuſcles; but becauſe it belongs to a multitude of aſſociations, and ſeems to differ from texture, with which it hath ſo much affinity, as perhaps to be reducible to it, in this, that always in mixtures, but not ſtill in textures, there is required a heterogeneity of the component parts. And every diſtinct portion of matter, whether it be a corpuſcle or a primary concretion, or a body of the firſt, or of any other order of mixts, is to be conſidered, not as if it were placed in vacuo, nor as if it had relation only to the neighbouring bodies, but as being placed in the univerſe, conſtituted as it is, amongſt an innumerable company of other bodies, whereof ſome are near it, and others very remote, and ſome are great and ſome ſmall, ſome particular and ſome catholick agents, and all of them governed as well by (11) the univerſal fabrick of things, as by the laws of motion eſtabliſhed by the author of nature in the world.

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* Principally in the Sceptical Chymiſt,

And now, *Pyrophylus*, that we have enumerated 11 very general affections of matter, which with itſelf make up 12 principles of variation in bodies; let me on the behalf of the corpuſcularians apply to the origine of qualities a compariſon of the old atomiſts imployed by *Lucretius*, and others, to illuſtrate the production of an infinite number of bodies, from ſuch ſimple fragments of matter as they thought their atoms to be. For ſince of the 24 letters of the alphabet aſſociated ſeveral ways, as to the number and placing of the letters, all the words of the ſeveral languages in the world may be made; ſo, ſay theſe naturaliſts, by variouſly connecting ſuch and ſuch numbers of atoms, of ſuch ſhapes, ſizes, and motions, into maſſes or concretions, an innumerable multitude of different bodies may be formed. Wherefore, if to thoſe four affections of matter, which I lately called the moſt primary and ſimple, we add the ſeven other ways, whereby, or on whoſe account, it may be altered, that are, though not altogether, yet almoſt as catholick, we ſhall have eleven principles ſo fruitful, that from their various aſſociations may reſult a much vaſter multitude of phænomena, and among them of qualities, than one, that does not conſider the matter attentively, would imagine. And to invite you to believe this, I ſhall deſire you to take notice of theſe three things.

The firſt is, that ſuppoſing theſe ten principles were but ſo many letters of the alphabet, that could be only put together in differing numbers, and in various orders; the combinations and other aſſociations, that might be made of them, may be far more numerous than you your ſelf will expect, if you are not acquainted with the way of calculating the number of differing aſſociations, that may be made between ten things propoſed. The beſt way I know of doing this is by algebra or ſymbolical arithmetick, by which it appears, that of ſo few things ſo many (α) aſſociations may be made, each of which will differ from every one of the reſt, either in the number of the things aſſociated, or in the order wherein they were placed.

But (which is the ſecond thing to be taken notice of) each of theſe ten producers of phænomena admits of a ſcarce credible variety. For not to deſcend ſo low as inſenſible corpuſcles (many thouſands of which may be requiſite to conſtitute a grain of muſtard ſeed) what an innumerable company of different bigneſſes may we conceive between the bulk of a mite, a crowd of which is requiſite to weigh one grain, and a mountain, or the body of the ſun, which aſtronomers teach us to be above an hundred and threeſcore times bigger than the whole terreſtrial globe?

And ſo though (β) figure be one of the moſt ſimple modes of matter; yet it is capable partly in regard of the ſurface, or ſurfaces of the figured corpuſcles (which may conſiſt of triangles, ſquares, pentagones, &c.) and partly in regard of the ſhape of the body itſelf, which may be either flat like a cheeſe, or lozenge; or ſphærical like a bullet; or elliptical, almoſt like an egg; or cubical like a dye; or cylindrical like a rowling-ſtone; or pointed like a pyramid, or ſugar-loaf: figure I ſay, though but a ſimple mode, is, upon theſe and other ſcores, capable of ſo great a multitude of differences, that it is concerning them, and their affections, that *Euclid*, *Apollonius*, *Archimedes*, *Theodoſius*, *Clavius*, and later writers than he, have demonſtrated ſo many propoſitions. And yet all the hitherto named figures are almoſt nothing to thoſe irregular ſhapes, ſuch as are to be met with among rubbiſh, and among hooked and branched particles, &c. that are to be met with among corpuſcles and bodies; moſt of which have no particular appellations, their multitude and their variety having kept men from enumerating them, and much more from particular naming them.

To which let me add, that theſe varieties of figure and ſhape do alſo ſerve to modify the motion, and other affections of the corpuſcle endowed with them, and of the compounded body, whereof it makes a part.

And that the (γ) ſhape and alſo ſize of bodies, whether ſmall or great, may exceedingly diverſify their nature and operation, I ſhall often have occaſion to manifeſt, and therefore I ſhall now only give you a groſs example of it; by inviting you to conſider, how many differing ſorts of tools and inſtruments, almoſt each of them fit for many different operations and uſes, ſmiths, and other not the nobleſt ſort of tradeſmen, have been able to form out of pieces of iron, only by making them of differing ſizes, and giving them differing ſhapes. For when I have named bodkins, forks, blades, hooks, ſciſſars, anvils, hammers, files, raſps, chiſſels, gravers, ſcrews, vices, ſaws, borers, wires, drills, &c. when (I ſay) I have named all theſe, I have left a far greater number undermentioned.

So likewiſe (δ) motion, which ſeems ſo ſimple a principle, eſpecially in ſimple bodies, may even in them be very much diverſified. For it may be more or leſs ſwift, and that in an almoſt infinite diverſity of degrees. It may be ſimple or compounded, uniform, or difform, and the greater celerity may preceed or follow. The body may move in a ſtreight line, or in a circular, or in ſome other curve line, as elliptical, hyperbolical, parabolical, &c. of which geometricans have deſcribed ſeveral, but of which there may be in all I know not how many more; or elſe the body's motion may be varied according to the ſituation, or nature of the body it hits againſt, as that is capable of reflecting it, or refracting it, or both, and that after ſeveral manners: the body may alſo have an undulating motion, and that with ſmaller or greater waves; or may have a rotation about its own middle parts; or may have both a progreſſive motion, and a rotation, and the one either equal to the other, or ſwifter than it, in almoſt infinite proportions. As to the determination of motion, the body may move directly upwards, or downwards, decliningly, or horizontally, eaſt, weſt, north, or ſouth, &c. according to the ſituation of the impellent body. And beſides theſe and other modifications of the motion of a ſimple corpuſcle or body, whoſe phænomena or effects will be alſo diverſified, as I partly noted already, by its bulk, and by its figure: beſides all theſe, I ſay, there will happen a new and great variety of phænomena, when divers corpuſcles, though primogenial, and much more if they be compounded, move at once, and ſo the motion is conſidered in ſeveral bodies. For there will ariſe new diverſifications from the greater or leſſer number of the moving corpuſcles; from their following one another cloſe, or more at diſtance; from the order, wherein they follow each other; from the uniformity of their motion, or the confuſedneſs of it; from the equality, or inequality of their bulk, and the ſimilitude or diſſimilitude of their figures; from the narrowneſs or wideneſs, &c. of the channel or paſſage, in which they move, and the thickneſs, thinneſs, pores, and the conditions of the medium, through which they move; from the equal or unequal celerity of their motion, and force of their impulſe: and the effects of all theſe are variable by the differing ſituation and ſtructure of the ſenſories, or other bodies on which theſe corpuſcles beat.

What we have elſewhere ſaid, to ſhew that local motion is, next the author of nature, the principal agent in the production of her phænomena, may I hope ſatiſfy you, that theſe diverſities in the motion of bodies may produce a ſtrange variety in their nature and qualities. And as I lately did, ſo I ſhall now adumbrate my meaning to you, by deſiring you to apply to our preſent purpoſe what you may familiarly obſerve in muſick. For according as the ſtrings, or other inſtruments of producing ſounds, do tremble more or leſs ſwiftly, they put the air into a vibrating motion more or leſs briſk, and produce thoſe diverſities of ſounds, which muſicians have diſtinguiſhed into notes, which they have alſo ſubdivided, and whereto they have given diſtinct names. And though the bodies, from whence theſe ſounds proceed, may be of very differing (ε) natures; as metalline, as wire, gutſtrings, bells, humane voices, wooden pipes, &c. yet provided they put the air into the like waving motion, the ſound and even the note will be the ſame: which ſhews how much that greater variety, which may be taken notice of in ſounds, is the effect of local motion. And if the ſound come from an inſtrument, as a lute, where not only one ſtring hath its proper ſound, but many have among them ſeveral degrees of tenſion, and are touched, ſometimes theſe, ſometimes thoſe, together; whereby more, or fewer, or none of their vibrations come to be coincident, they will ſo ſtrike the air, as to produce, ſometimes thoſe pleaſing ſounds we call concords, and ſometimes thoſe harſh ones we call diſcords.

It would take up too much time to inſiſt upon each of the ten remaining affections of matter, that I lately enumerated and repreſented to you as exceeding fertile; and by what I elſewhere deliver about pores alone, and the many ſorts of phænomena, in which they may have an intereſt, I could add no ſmall confirmation to what has been hitherto diſcourſed; if the inſerting of it here would not enormouſly increaſe the bulk of this paper, which I rather decline doing, becauſe what has been already ſaid of thoſe we have now, though we have but very breifly treated of, may, I hope, be ſufficient to perſwade you, that ſuch principles as theſe are capable of being made far more pregnant, than one would expect ſo few principles ſhould be. And this perſuaſion will be much facilitated, if we conſider, how great a variety may be produced, not only by the diverſifications, that each ſingle principle (upon the ſcore of the attributes that may belong to it) is capable of; but much more by the ſeveral (ζ) combinations, that may be made of them, eſpecially conſidering withal, that our external and internal ſenſes are ſo conſtituted,

ſtituted, that each, or almoſt each, of thoſe diverſifications or modifications may produce a diſtinct impreſſion on the organ, and a correſpondent perception in the diſcerning faculty; many of which perceptions, eſpecially if diſtinguiſhed by proper names, belong to the liſt of particular qualities.

CHAP. IV.

THE third and laſt difficulty, that now remains to be conſidered, may be thus propoſed: that whereas, according to the corpuſcularian hypotheſis, not only one or two qualities, but all of them proceed from the bigneſs, and ſhape, and contexture of the minute parts of matter, it is conſonant to their principles, that if two bodies agree in one quality, and ſo in the ſtructure, on which that quality depends, they ought to agree in other qualities alſo; ſince thoſe do likewiſe depend upon the ſtructure, wherein they do agree; and conſequently it will be ſcarce poſſible to conceive, that two ſuch bodies ſhould be endowed with ſo many differing qualities, as experience ſhews they may.

To illuſtrate this objection by an example, it is pretended, that the whiteneſs of froth proceeds from the multitude and hemiſpherical figure of the bubbles it is made up of. And if this or any other mechanical fabrick or contexture be the cauſe of whiteneſs, how comes it to paſs, that ſome white bodies are inodorous and inſipid, as the calx of harts-horn; others both ſtrongly ſcented, and ſtrongly taſted, as the volatile ſalt of harts-horn or of blood; ſome diſſoluble in water, as ſalt of tartar; others indiſſoluble in that liquor, as calcined harts-horn, &c. ſome fixed in the fire, as the bodies laſt named; others fugitive, as powdered ſal armoniack; ſome incombuſtible, as ſalt of tartar; others very inflammable as camphire. To which examples a greater variety of white bodies might be added, if it were neceſſary.

This I confeſs is a conſiderable difficulty may puzzle more than a novice in the corpuſcularian philoſophy: wherefore to do ſomewhat in order to the clearing of it, I ſhall recommend to you the four following conſiderations:

1. And firſt I ſhall conſider, that in the pores of viſible and ſtable bodies, there may be often lodged inviſible and heterogeneous corpuſcles, to which a particular quality, that belongs to the body as ſuch, is to be referred. Thus we ſee in a perfumed glove, that in the pores of the leather odoriferous particles are harboured, which are of quite another nature than the leather it ſelf, and wholly adventitious to it, and yet endue it with the fragrancy, for which it is prized. A like example is afforded us in raſberry wine made with claret. For the pleaſing ſmell is imparted to the wine, by the corpuſcles of the berries diſperſed *per minima* through the whole body of it.

2. The ſecond thing that I conſider is this, that oftentimes corpuſcles of very differing natures, if they be but fitted to convene, or to be put together after certain manners, which yet require no radical change to be made in their eſſential ſtructures, but only a certain juxtapoſition or peculiar kind of compoſition; ſuch bodies, I ſay, may notwithſtanding their eſſential differences exhibit the ſame quality. For inviſible changes made in the minute, and perhaps undeſcernible parts of a ſtable body may ſuffice to produce ſuch alterations in its texture, as may give it new qualities, and conſequently differing from thoſe of other bodies of the ſame kind or denomination; and therefore though there remains as much of the former ſtructure, as is neceſſary to make it retain its denomination, yet it may admit of alterations ſufficient to produce new qualities. Thus when a bar of iron has been violently hammered, though it continues iron ſtill, and is not viſibly altered in its texture; yet the inſenſible parts may have been put into ſo vehement an agitation, as may make the bar too hot to be held in one's hand. And ſo if you hammer a long and thin piece of ſilver, though the change of texture will not be viſible, it will acquire a ſpringineſs, that it had not before. And if you leave this hammered piece of ſilver a little while upon the glowing coals, and after let it cool, though your eye will perchance as little perceive, that the fire has altered its texture, as it did before, that the hammer had; yet you will find the elaſticity deſtroyed.

If on the ſurface of a body there ariſe or be protuberant a multitude of ſharp and ſtiff parts, placed thick or cloſe together, let the body be iron, ſilver, or wood, or of what matter you pleaſe, theſe extant and rigid parts will ſuffice to make all theſe bodies to exhibit the ſame quality of aſperity, or roughneſs.

And if all the extant parts of a (phyſical) ſuperficies be ſo depreſſed to a level with the reſt, that there is a co-œquation, if I may ſo ſpeak, made of all the ſuperficial parts of a body; this is ſufficient to deprive it of former roughneſs, and give it that contrary quality we call ſmoothneſs. And if this ſmoothneſs be conſiderable exquiſite, and happen to the ſurface of an opacous body of a cloſe and ſolid contexture, and fit to reflect the incident rays of light and other bodies unperturbed; this is enough to make it ſpecular, whether the body be ſteel, or ſilver, or braſs, or marble, or flint, or quickſilver, &c.

And ſo, as I noted in the laſt chapter on another occaſion, if a body be ſo framed and ſtretched, as being duly moved by another body to put the air into an undulating motion, briſk enough to be heard by us, we call that ſonorous, whether it be a metalline bell, or gut-ſtrings, or wires, &c. Nay if waving motions, whereinto the air is put by ſuch differing bodies, be alike, theſe bodies will not only in general give a ſound, but will yield that particular degree of ſound, that men call the ſame note.

For here it is to be conſidered, that beſides that peculiar and eſſential modification, which conſtitutes a body, and diſtinguiſhes it from all others, that are not of the ſame ſpecies, there may be certain other attributes, that we call extra-eſſential; which may be common to that body with many others, and upon which may depend thoſe more external affections of the matter, which may ſuffice to give it this or that relation to other bodies, divers of which relations we ſtile qualities.

Of this I ſhall give you an evident example in the production of heat. For provided there be a ſufficient and confuſed agitation made in the inſenſible parts of a body, whether it be iron or braſs, or ſilver, or wood, or, ſtone; that vehement agitation, without deſtroying the nature of the body that admits it, will fit it for ſuch an operation upon our ſenſe of feeling, and upon bodies eaſy to be melted (as butter, wax, &c.) as we call heat.

And ſo in the inſtance named in the objection about whiteneſs; it is accidental to that quality, that the corpuſcles it proceeds from ſhould be little hemiſpheres. For though it happen to be ſo in water agitated into froth; yet in water frozen to ice, and beaten very ſmall, the corpuſcles may be of all manner of ſhapes; and yet the powder be white. And it being ſufficient to the producing of whiteneſs, that the incident light be reflected copiouſly every way, and untroubled by the reflecting body, it matters not, whether that body be water, or white wine, or ſome other clear liquor turned into froth, or ice, or glaſs, or cryſtal, or clarified rozin, &c. beaten into powder; ſince without diſſolving the eſſential texture of theſe formerly diaphanous bodies, it ſuffices, that there be a comminution into grains numerous and ſmall enough by the multitude of their ſurfaces, and thoſe of the air, or other fluid, that gets between them, to hinder the paſſage of the beams of light, and reflect them every way, as well copiouſly, as unperturbed.

Perhaps it may not be impertinent to add to this, that there may be other catholick affections of corpuſcles, beſides the ſhape or ſtructure of them, by virtue whereof aggregates even of ſuch as are (as to ſenſe) homogeneous, may exhibit differing qualities: as for inſtance, they may have ſome, when they are in a briſk motion, and others, when they are but in a languid one, or at reſt: as ſalt petre, when its parts are ſufficiently agitated by the fire in a crucible, is not only fluid but tranſparent almoſt like water; whereas when it cools again, it becomes a hard and white body; and butter, that is opacous in its moſt uſual ſtate, may be diaphanous when it is melted. So I ſhall hereafter have occaſion to ſhew you, that a great quantity of beaten alabaſter, which uſually retains the form of a moveleſs heap of white powder, by being after a due manner expoſed to heat, obtains, and that without being brought to fuſion, many of the principal qualities of a fluid body. And if with good ſpirit of nitre, or aqua fortis, you fill a glaſs half full, it will (unleſs it be extraordinarily deflegmed) exhibit no redneſs, nor approaching colour in the veſſel: but if you warm it a little, or caſt into it a bit of iron or of ſilver, that it may put the liquor into a commotion, then the nitrous ſpirits deveſting the form of a liquor, and aſcending in that of fumes, will make all the upper part of the glaſs look of a deep yellow, or a red.

3. The third thing I would recommend to your conſideration is, to reflect on what I propoſed in the laſt foregoing ſection, where I told you, that in reference to the production of qualities, a body is not to be conſidered barely in itſelf, but as it is placed in, and is a portion of the univerſe. But of this ſubject I have ſaid ſo much in the newly mentioned diſcourſe, and in that which you are there referred to, that I ſhall now only put you in mind, that divers of the particulars to be met with in thoſe diſcourſes are applicable to our preſent purpoſe.

4. To all this let me add in the laſt place, that, as to that part of the grand objection that we are clearing, which urges the difficulty of explicating upon the corpuſcular principles, how, for example, the ſame body, whoſe ſtructure makes it ſhaped ſo as to be fit to exhibit whiteneſs, ſhould likewiſe have divers other qualities, that ſeemed to have no affinity with whiteneſs. This ſcruple, I ſay, we may, by what we have already diſcourſed, be aſſiſted to remove; eſpecially if we ſubjoin another conſideration to it. For if corpuſcles without loſing that texture, which is eſſential to them, may (as we have ſhewed they may) have their ſhape, or their ſurfaces, or their ſituation changed; and may alſo admit of alterations, (eſpecially as theſe corpuſcles make up an aggregate or congeries,) as to motion or reſt; as to theſe or thoſe degrees, or other circumſtances of motion; as to laxity and denſity of parts, and divers other affections; why ſhould we not think it poſſible, that a ſingle, though not indiviſible, corpuſcle, and much more an aggregate of corpuſcles, may by ſome of theſe, or the like changes, which, as I was ſaying, deſtroy not the eſſential texture, be fitted to produce divers other qualities, beſides theſe that neceſſarily flow from it? Eſpecially conſidering (which is that I have now to add) that the qualities commonly called ſenſible, and many others too, being according to our opinion but relative attributes, one of theſe now mentioned alterations, though but mechanical, may endow the body, it happens to, with new relations both to the organs of ſenſe, and alſo to ſome other bodies, and conſequently may endow it with additional qualities.

If from good venice or other turpentine you gently evaporate, or abſtract about a third part of its whole weight, you may obtain a fine tranſparent, and almoſt reddiſh colophony. If you beat this very ſmall, it will looſe its colour and tranſparency, and will afford you an opacous and very white powder. If you expoſe this to a moderate heat, it will quickly and without violence both regain its colour and tranſparency, and acquire fluidity. And if, whilſt it is thus melted, you put the end of a quill or reed a little beneath the ſurface, and blow ſkilfully into it, you may obtain bubbles adorned with very various and vivid colours. If when it has loſt its fluidity, but whilſt it is yet pretty warm, you take it into your hands, you will find, that it has in that ſtate a viſcoſity, by vertue of which you may draw it out into threads, as you may paſt; but as ſoon as it grows quite cold, it becomes exceeding brittle: and if whilſt it is is yet warm, you give it the ſhape of a triangular priſm, and make it of a convenient bulk, it will exhibit variety of colours almoſt like a triangular glaſs. Whilſt this colophony is cold, and its parts are not put into a due motion, ſtraws and other light bodies may be held unmoved cloſe to it; but if by rubbing it a little you put the parts into a convenient agitation, though perhaps without ſenſibly warming the colophony, it diſplays an electrical quality, and readily draws to it the hairs, ſtraws, &c. that it would not move before. All or moſt of theſe things you may alſo perform, if I miſtake not, with clarified roſin, though I am not ſure it will do ſo well.

To this I ſhall add one inſtance more, which may let you ſee, how the ſame body, which the chemiſts themſelves will tell you is ſimple and homogeneous, may, by vertue of its ſhape, and other mechanical affections, (for it is a factitious body, and that is made by the deſtruction of a natural one) have ſuch differing reſpects to different ſenſories, and to the pores, &c. of divers other bodies, as to diſplay ſeveral very differing qualities. The example I ſpeak of, is afforded me by the diſtillation of putrefied urine. For though ſuch urine have already loſt its firſt texture before it come to be diſtilled; yet when it has undergone two or three diſtillations to deflegm it, the ſpirits of it ſwimming in a phlegmatick vehicle have a pungent ſaltneſs upon the tongue, and a very ſtrong, and to moſt perſons an offenſive ſmell in the noſtrils; and when they are freed from the water, they are wont to appear white to the eye; and to very tender parts, as to thoſe that are excoriated, or to the conjunctiva they feel exceeding ſharp, and ſeem to burn almoſt like a cauſtick, not to ſay like fire; inſomuch that I have ſeen them preſently make bliſters upon the tongue itſelf, that was not raw or ſore before they touched it; the ſame ſaline particles inviſibly flying up to the eyes prick them, and make them water; and invading the noſe often cauſe that great commotion in the head and other parts of the body, that we call ſneezing. The ſame corpuſcles, if they are much ſmelt to by a woman in hyſterical fits, do very often ſuddenly relieve her, and ſo may be reckoned among the ſpecifick remedies of that odd and manifold diſeaſe, which is not the only one, in which they are conſiderable medicines, as we have elſewhere declared. The ſame corpuſcles taken into human bodies have the qualities, that in other medicines we call diaphoretick, and diuretick; the ſame particles being put upon filings of braſs produce a fine blue, whereas upon the blue or purple juices of many plants they preſently produce a green; being put to work upon copper, whether crude, or calcined, they do readily diſſolve it, as corroſive menſtruums are wont to do other metals; and yet the ſame corpuſcles being blended in a due proportion with the acid ſalts of ſuch menſtruums, have the virtue to deſtroy their corroſiveneſs; and if they be put into ſolutions made with ſuch menſtruums, they have a power, excepting in very few caſes, to precipitate the bodies therein diſſolved. I might here add, *Pyrophylus*, how the ſame particles applied to ſeveral other bodies, to which they have differing relations, have ſuch diſtinct operations on them, as may intitle theſe ſame ſpirits to other qualities. But to enumerate them in this place were tedious, eſpecially having already named ſo many qualities reſiding in this ſpirituous ſalt; which I therefore the rather pitched upon, becauſe being a factitious body, and made out of a putrefied one, and ſo ſimple as to be a chemical ſalt (which, you know, ſpagyriſts make one of the three principles of compounded bodies) I ſuppoſe you will make the leſs ſcruple to admit, that it works by virtue of its mechanical affections. Of which to perſuade you the more, I ſhall add, that if you compound this urinous ſalt with the ſaline particles of common ſalt (which is alſo a factitious thing, and confeſſed by chemiſts to be a ſimple principle of the concrete, that yeilds it) theſe two being mingled in a due proportion, and ſuffered leiſurely to combine, will aſſociate themſelves into corpuſcles, wherein the urinous ſalt loſes moſt of the qualities I have been aſcribing to it, and with the acid ſpirit compoſes, as I have often tried, a body little differing from ſal armoniack: which great change can be aſcribed to nothing ſo probably, as to that of the ſhape and motion (not here to add the ſize) of the urinous ſalt, which changes the one, and loſes a great part of the other by combining with the acid ſpirits. And to confirm that both theſe do happen, I have ſeveral times ſlowly exhaled the ſuperfluous, but not near the whole liquor from a mixture made in a due proportion of the ſpirit of urine and that of ſalt, and found, that anſwerably to my conjecture, there remained in the bottom a ſalt, not only far more ſluggiſh than the fugitive one of urine, but whoſe viſible ſhape was quite differing from that of the volatile cryſtals of urine, this compounded ſalt being generally figured, either like combs or like feathers.

If after all this we do either add or inculcate, that the extraeſſential changes, that may be made in the ſhape, contexture and motion, &c. of bodies, that agree in their eſſential modifications, may not only qualify them to work themſelves immediately after a differing manner upon differing ſenſories, and upon other bodies alſo, whoſe pores, &c. are differently conſtituted, but may diſpoſe them to receive other impreſſions than before, or receive wonted ones after another manner from the more catholick agents of nature; if, I ſay, we recommend this alſo to your conſideration, what has been delivered in the whole diſcourſe will I hope let you ſee, that the ſcruple propoſed at the beginning of it is not ſo perplexing a one to our philoſophy, as perhaps you then imagined it.

The three difficulties conſidered partly in this, and partly in the two foregoing ſections, I was the more inclined to take notice of in this place, (for in divers other paſſages of my writings you will meet with things, that are applicable to the paſt diſcourſe, and ſhould be referred thither) partly becauſe the ſcruples themſelves are of great moment, and for ought I know have been diſcuſſed by others; and partly becauſe theſe difficulties relating in ſome ſort to the corpuſcularian hypotheſis in the general, the clearing of them may both ſerve to confirm ſeveral of theſe things, that have above been written about the origin of forms and qualities (to which it might therefore have been joined) and will be conducive to a clear underſtanding, and explicating divers of the particulars that I am about to deliver, and perhaps ſeveral other phænomena of nature.

OF THE SYSTEMATICAL or COSMICAL QUALITIES of THINGS.

CHAP. I.

I EXPECT, *Poriphilus*, that being ſomewhat ſurprized at the title of this diſcourſe, you will preſently aſk, what I underſtand by Coſmical or Syſtematical Qualities; that name being new enough to require, that I ſhould tell you, both what is meant by it, and why I make choice of it.

To anſwer ſo reaſonable a queſtion, I ſhall inform you, that I conſider, that the qualities of particular bodies (for I ſpeak not here of magnitude, ſhape, and motion, which are the primitive modes and catholick affections of matter itſelf) do for the moſt part conſiſt in relations, upon whoſe account one body is fitted to act upon others, or diſpoſed to be acted on by them, and receive impreſſions from them; as quick-ſilver has a quality or power (for I here take qualities in the larger ſenſe) to diſſolve gold and ſilver, and a capacity or diſpoſition to be diſſolved by aqua fortis, and (though leſs readily) by aqua regis. And this being premiſed, I obſerve farther, that, though in eſtimating the qualities of natural bodies, we are wont to conſider but the power any particular one has of acting upon, or the capacity it has of ſuffering from ſuch and ſuch particular bodies, wherewith it is taken notice of to have manifeſt commerce in point of making or of receiving impreſſions; yet there may be ſome attributes, which may belong to a particular body, and divers alterations, to which it may be liable, not barely upon the ſcore of theſe qualities, that are preſumed to be evidently inherent in it, nor of the reſpects it has to thoſe other particular bodies, to which it ſeems to be manifeſtly related, but upon the account of a ſyſtem ſo conſtituted as our world is, whoſe fabrick is ſuch, that there may be divers unheeded agents, which, by unperceived means, may have great operations upon the body we conſider, and work ſuch changes it, and enable it to work ſuch changes on other bodies, as are rather to be aſcribed to ſome unheeded agents, than to thoſe other bodies, with which the body propoſed is taken notice of to have to do. So that although if divers bodies, that I could name, were placed together in *vacuo*, or removed together into ſome of thoſe imaginary ſpaces, which divers of the ſchoolmen fancy to be beyond the bounds of our univerſe, they would retain many of the qualities they are now endowed with; yet they would not have them all: but by being reſtored to their former places in this world, would regain a new ſet of faculties (or powers) and diſpoſitions, which, becauſe they depend upon ſome unheeded relations and impreſſions, which theſe bodies owe to the determinate fabrick of the grand ſyſtem or world they are parts of, I have, until I can find a more proper appellation, thought fit to name their coſmical or their ſyſtematical qualities.

I have in the Origin of Forms touched upon this ſubject already, but otherwiſe than I am now about to do. For whereas that which I do principally, (and yet but tranſiently,) take notice of, is, that one body being ſurrounded with other bodies, is manifeſtly wrought on by many of thoſe among whom it is placed; that which I chiefly in this diſcourſe conſider, is the impreſſions, that a body may receive, or the power it may acquire, from thoſe vulgarly unknown, or at leaſt unheeded agents, by which it is thus affected, not only upon the account of its own peculiar texture or diſpoſition, but by virtue of the general fabrick of the world.

CHAP. II.

NOW though there be ſeveral of the grand mundane bodies, and divers laws and cuſtoms of nature, which may contribute (more or leſs) to the phænomena of the qualities we are treating of; yet becauſe a diſtinct and particular inquiry into each of them would challenge a much longer diſcourſe than this ſhort eſſay is to be, and a much abler pen than his that writes it; I did not only think it fit to reſerve what occurs to me about the laws and cuſtoms of nature, as they concern this ſubject, to another diſcourſe, or an appendix to this; but to declare to you alſo, that whereas the three main bodies, whoſe more unobſerved operations

rations and changes have the moſt conſiderable influence on the qualities we are to treat of, are, the ſubterraneal parts of the globe we inhabit; the ſtars, whether fixed or wandring, with the æther that is about them; and the atmoſphere or air we live in; I foreſee, that it will be requiſite for me to aſſign the experiments and obſervations I have collected about theſe three ſubjects to other tracts. So that in this eſſay my chief work will be, to take notice to you of ſome conſiderations, that may be introductory in a more general way to the clearer knowledge of the ſubject to be diſcourſed of. To which I may, as time and my occaſions may permit, ſubjoin ſome particulars, which, though perhaps they do not all of them ſo directly or properly belong to the ſolemnly propoſed heads of this diſcourſe, yet are not impertinent to the deſign of it; and on that ſcore may be allowed their places in it.

AND leaſt you ſhould think, that under the name of coſmical qualities, I ſhould introduce chimæras into natural philoſophy, I muſt betimes advertiſe you, that you will meet with divers particles in the following diſcourſe, fit to ſhew, that theſe qualities are not meerly fictitious qualities; but ſuch, whoſe exiſtence I can manifeſt, not only by conſiderations not abſurd, but alſo by real experiments and phyſical phænomena. And to prevent miſtakes, I ſhall add, that under the name of catholick and unminded cauſes or agents, I comprehend not only divers inviſible portions of matter, but alſo the eſtabliſhed laws of the univerſe, or that which is commonly called the ordinary courſe of nature. And when I ſpeak of unobſerved agents or cauſes, I do not always mean, that they are not known or taken notice of to be *in rerum natura*, but that they are not vulgarly conſidered or looked upon, as the cauſes of ſome particular phænomena, wherein I aſcribe to them an intereſt or efficiency.

BUT before I proceed any farther, it will not be amiſs to intimate in this place, that the things, on which I founded the purpoſed notion of the coſmical attributes of bodies, were principally theſe three;

1. THAT there are many bodies, that in divers caſes act not, unleſs they be acted on; and ſome of them act, either ſolely or chiefly, as they are acted on by the catholick and unheeded agents, we have been ſpeaking of.

2. THAT there are certain ſubtle bodies in the world, that are ready to inſinuate themſelves into the pores of any body diſpoſed to admit their action, or by ſome other way affect it, eſpecially if they have the concurrence of other unobſerved cauſes and the eſtabliſhed laws of the univerſe.

3. THAT a body by a mechanical change of texture may acquire or looſe a fitneſs to be wrought upon by ſuch unheeded agents, and alſo to diverſify their operations on it upon the ſcore of its varying texture.

THESE three propoſitions I ſhall endeavour to confirm diſtinctly by the enſuing experiments and phænomena: but becauſe divers of theſe proofs may each of them ſerve to confirm more than one of theſe propoſitions, and becauſe the making out of the two laſt, which are the moſt important (and the leaſt probable) is the main deſign of this diſcourſe, I ſhall ſay the leſs to the firſt, leaving it for the moſt part to you, to refer to either of the three propoſitions what you ſhall meet with belonging to it in what is ſaid upon either of the other two.

CHAP. III.

TO begin then with the firſt propoſition, namely, "That there are many bodies, that in divers caſes act not, unleſs they be acted on; and ſome of them act either ſolely or chiefly, as they are acted on by the catholick and unheeded agents, we have been ſpeaking of:" the former part of it will, I preſume, be eaſily granted, it being evident by ſuch groſs inſtances as theſe, that a wedge will not cleave a block, unleſs it be impelled againſt it by a hammer (or ſome æquivalent inſtrument) nor a knife attract a needle, unleſs it be excited by a magnet. But as to the ſecond, it will not in likelihood be ſo readily aſſented to, and therefore having *in tranſitu* illuſtrated it by obſerving to you, that concave looking-glaſſes, and convex burning-glaſſes, kindle not other bodies, unleſs they be enabled to do ſo by the reflected or trajected beams of the ſun, I ſhall proceed to prove it by a couple of inſtances.

THE one is, an iron bar, that hath long ſtood in a window, or ſome other fit place in a perpendicular poſture; for though this bar was not, when it was firſt erected, endowed with a magnetiſm any thing ſuperior to that of other iron bars of the like ſhape and bigneſs, yet after it hath very long ſtood in that poſition, it will by the operations of inviſible agents acquire a farther degree of magnetiſm, than belonged to it, as a bar of iron, and is enabled to produce ſome magnetical phænomena (elſewhere mentioned) that it could not before.

THE ſecond inſtance is afforded us by what happens to a very flat and exquiſitely poliſhed piece of marble; for though of itſelf it hath no power to help to lift up any other dry body that it is laid upon, yet if it come to be ſkilfully laid upon another piece of marble as flat and ſmooth as it, and of a bulk not too unweildy; this upper ſtone, by virtue of the fabrick of the world, which gives the ambient air fluidity and weight, is enabled without any other cement or faſtening inſtrument than immediate contact, to raiſe with itſelf (in caſe a man lift it up) the lower marble, though perhaps an hundred times heavier than itſelf. * [Whereas, if this laying one of theſe ſtones upon the other had been done *in vacuo*, I doubt not but no ſuch power had thereby accrued to the uppermoſt of them.]

CHAP.

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* See this experimentally proved in the Continuation of the Author's New Experiments touching the air, experiment the fiſtieth,

CHAP. IV.

PROCEED we now to our ſecond propoſition, which ſpeaks to this purpoſe; "That there are certain ſubtle bodies in the "world, that are ready either to inſinuate them-"ſelves into the pores of any body, diſpoſed "to admit their actions; or by ſome other "way to affect it; eſpecially if they have the "concurrence of other unobſerved cauſes, and "the eſtabliſhed laws of the univerſe." I need not take notice on this occaſion, that divers of the ancient philoſophers thought, that there was a ſubtler body than the common air, and called æther; and that the Carteſians tells us, that there is ſuch a ſubſtance diffuſed throughout the univerſe, which they call, according to the differing ſizes of its parts, ſometimes *primum elementum*, and ſometimes *materia cœleſtis*, to which they attribute the uſe of pervading all other bodies, and adequately filling thoſe pores of theirs, that are correſpondent in bigneſs and figure to the differing portions of this inſinuating matter. That there may be ſuch a ſubſtance in the univerſe, the aſſerters of it will probably bring for proofs ſeveral of the phænomena I am about to relate. But whether there be, or be not in the world any matter, that exactly anſwers to the deſcriptions they make of their firſt and ſecond elements, I ſhall not here diſcuſs; though divers experiments ſeem to argue, that there is in the world an æthereal ſubſtance very ſubtle and not a little diffuſed. But though theſe things ſeem, as I was ſaying, probable enough; yet the inviſible agents, I ſhall here chiefly, though not only, take notice of, will be the air (as it hath a weight and ſpring) and the magnetical effluvia of the terreſtrial globe.

IF you take a bar of iron, or rather of ſteel, and another like it of ſilver, and having heated each of them red hot, and put them to cool directly north and ſouth; though they be both acted upon by the ſame agent, the fire, and the ſteel, as to ſenſe, ſeems ſuch as it was before, yet the texture of theſe two metals being different, the ſilver acquires no new quality by what hath been done to it, whereas the ignition of the ſteel having opened its pores, and made its parts more pliable (as may be argued from the ſwelling of iron heated red hot, and its ſoftneſs under the hammer) it is eaſily, whilſt in this ſtate it lies north and ſouth, pervaded by the magnetical effluvia of the earth, which glide perpetually through the air from one pole to another, and by the paſſage of theſe ſteams it becomes endowed with a magnetical property, which ſome call polarity, whereby being freely ſuſpended and exactly poiſed, it will, as it were, ſpontaneouſly direct itſelf towards the north and ſouth, and exerciſe ſome operations peculiar to magnetical bodies. And that it may ſeem the leſs ſtrange, that I ſhould aſcribe to ſo groſs and dull a body, as the earth, the power of inviſible communicating to iron a magnetical virtue, which is thought to be of ſo ſpiritual a nature; I ſhall put you in mind of an experiment, that I acquainted you with divers years ago, about the earth's power to impart, in ſome caſes, without the help of a loadſtone, a directive faculty to the loadſtone itſelf. For, having by ignition deprived an oblong magnet of its former attractive power, by taking it red hot out of the fire, and ſuffering it to cool north and ſouth, I could at pleaſure, by placing either end northward or ſouthward, whilſt the ſtone was refrigerating, make what end I had a mind to, point to the north pole; and when it had done ſo, I could, by a new ignition and refrigerating of it in a contrary poſition, make the ſame end of the ſtone become its ſouthern pole.

IF you take a capacious glaſs vial with a ſlender neck, ending in a ſharp angle, with only a pin-hole left open at the apex, (inſtead of which veſſel, *Hero*'s egg, as ſome call it, though far ſmaller, and without ſuch a neck may ſerve turn) and by ſuction [illegible] otherwiſe free it from as much of the included air as you can; and if then having ſtopped this hole with your finger, you immerſe it ſomewhat deep under water, and, laſtly, withdraw your finger; the water will, contrary to its own nature (as is vulgarly conceived) ſpring up with violence, and to a good height into the cavity of the vial; which motion of a heavy liquor upwards cannot be aſcribed to the motion of the finger; for that did but unſtop the orifice, and not impel up the water; nor need be attributed to nature's abhorrence of a vacuum, which (whether there be ſuch a thing or not) it is altogether unneceſſary to have recourſe to in this caſe; the preſſure of the ambient air, proceeding from its weight upon the ſurface of the water, being ſufficient to force up that liquor into the vial, in which the remaining air, by being rarefied, upon the ſcore of the abſence of that which was taken out, hath its ſpring too much weakened to be able to reſiſt the preſſure of the outward air, as it formerly could do; whereas, if this experiment were tried in vacuo, the water would not be raiſed, there being no outward agent to impel it up.

CHAP. V.

I HAD ſometimes the curioſity to conſider beans and peaſe pulled up out of the ground by the ſtalks, in order to an inquire into their germination; and after having taken notice of their tumidneſs upon their having imbibed the moiſture of the ſoil, and of their way through the ambient earth not only upwards with their ſtems, but downwards with their tender roots; I thought fit to try, with what ſtrength and force the cauſes of their intumeſcence endeavoured to dilate them. Whereupon I filled with a quantity of ſuch dry beans, as are in *England* wont to be given to horſes, ſeveral vials and bottles, ſome of glaſs, and ſome of earth, whereof two or three were of a conſiderable ſtrength: which done, the intervals between the beans were filled with water, and the veſſels were exactly ſtopped with corks ſtrongly tied down with ſtrings, that nothing might get out; for I ſuppoſed, that the water ſoaking into the pores of the beans, would alter the figure of the pores, and produce in them an endeavour to ſwell; which being checked by the ſides and ſtopples of the veſſels would diſcover, whether that endeavour were ſo forceable as I ſuſpected. The ſucceſs was, that moſt of theſe veſſels (for in one or two of them we found the ſtrings broke, that withſtood the raiſing of the ſtopples) whether of glaſs or earth, were burſt in ſunder.

BUT being deſirous to make a nearer eſtimate, how great this expanſive force of the ſwelling beans was, we put a convenient quantity of them into an hollow, but ſtrong cylinder of braſs, which I had cauſed to be purpoſedly made for ſuch kind of trials, whoſe cylindrical cavity was juſt ſix inches in length, and two in diameter: then having put in water enough to reach the top of the beans, we put into the upper part of the cylinder, which was purpoſedly left unfilled, a wooden plug, made fit for the orifice, by being turned into a cylindrical form, and a little narrower than the orifice, that it might move freely up and down, though the water ſhould make it ſomewhat ſwell. Upon the top of this plug, on which leaned a broad and thick piece of wood, ſhaped like a round trencher, and made of the ſame piece with the plug, was placed a common half hundred weight of lead, which yet could not depreſs the plug too low, being hindered by the breadth of the trencher, made as well to prevent the too great depreſſion of the plug, as to afford a convenient baſis to the weight. Laſtly, having kept the cylinder in a quiet place for a fit ſpace of time (which is in ſuch trials ſometimes two or three days, ſometimes more or leſs according to the temperature of the air, and quantity of the included matter,) we obſerved, as I expected, that the ſwelling beans had very manifeſtly heaved up the plug, and the incumbent weight beyond the former ſtation. And I ſuſpected, that if we had had ſmall weights (of a pound or two a piece) conveniently ſhaped, a heavier weight might have been raiſed by the ſame force.

IT is not neceſſary in this place, that I mention ſeveral particulars relating to the experiment, as how it ſucceeds in corn ground and unground, how in dried fruits, as raiſins and currants, how in dried peaſe, (which we found to dilate themſelves very ſtrongly) and what liquors will or will not cauſe an intumeſcence; nor ſhall I here ſpeak of divers circumſtances, that may be taken notice of in ſuch trials: only I muſt not omit this particular, that I had a mind to make ſome trial, whether the force of ſwelling beans, to preſs or thruſt up the incumbent weight, would not in cylinders of different ſizes be increaſed in ſomewhat near a duplicate proportion to that of the diameters, or the areas of the orifices of the differing cylinders (becauſe it is according to the greatneſs of thoſe areas, that the force can be applied upwards;) but having not weights enough ſo ſhaped as I needed, I could not make ſuch an experiment as I deſired; but thus much however I diſcovered in order to my purpoſe, that the preſſure upwards of the drenched beans was very much greater in wider cylindrical veſſels than in narrower ones: for having put a convenient quantity of dried beans into a metalline cylinder, that wanted a pretty deal of being ſo deep as ſix inches, and was not quite four inches broad; when the included beans began to ſwell, they manifeſtly lifted up ſuch a plug as was lately deſcribed (but broader) with weights upon it, amounting to an hundred pounds or better.

WHETHER this may paſs for a new phyſical *vis movens*, I freely leave to you to determine; as alſo to conſider, whether by mechanical contrivances, ſo great a force, as may be this way produced, and which ſlowly and ſilently proceeds, till it hath attained its utmoſt energy, and may be conveyed into bodies without working any effect before the due time, may not in ſome caſes be made applicable to uſeful purpoſes.

I ſhall not now examine, whether or how far the foregoing experiment may confirm the Carteſian hypotheſis about their *materia ſubtilis*; nor whether upon the notions, which our experiments may ſuggeſt, we may be enabled to explicate the force, wherewith fermenting liquors do often break the veſſels, wherein they are too exactly ſhut up; about which phænomena, and of ſome others of kin to it, I elſewhere propoſe ſome conjectures.

I think it fitter in this place to take notice to you of ſomething, that more directly belongs to our preſent ſubject; namely, that the air, within which name I here compriſe the æther, that may be harboured in its pores, may in ſome caſes, by its conſtant preſence, and in others, by its being always at hand, and its readineſs to inſinuate itſelf wherever it can get admittance, concur to the production of divers phænomena, wherein its co-operation has not been ſuſpected even by philoſophers: for, not to mention what I have by experiments purpoſely deviſed, that the air's being preſent to preſs upon the ſuperficies of liquors is ſo requiſite in ſuction, that they will not thereby be made to aſcend without it; and beſides that to the putrefying of ſome bodies within the time (or even within ten times the time) that nature is wont to putrefy them in, they will not be brought to putrefaction, if the air be all the while carefully ſecluded: beſides theſe things, I ſay, I found, [that the light, which appears in ſome rotten woods, and in ſome putrefied fiſhes, did ſo much depend upon the preſence of the air, that if that were quite withdrawn from them, the light would diſappear, and when they were reſtored to the contact of the air, they would ſhine forth again as formerly. But of this elſewhere.]

CHAP. VI.

I KNOW not, whether it will be fit to add, that beſides what the air (with the ſubtler matter that may be mingled with it) may do as a ſubſtance; it may perform divers things upon other accounts, as its finer parts may be, though inſenſibly, moved in phyſical ſtrait lines; or as it is the ſubject of ſwarms of corpuſcles put into peculiar, though inviſible, motions. For inſtance, if I take a ſheet of paper, and rub it over with oil, or even a fit kind of greaſe; that, which the liquor apparently does, is only to pierce or ſoak into the pores of the paper, which before did by their crookedneſs, or upon ſome other mechanical account render the paper opacous. But this inſinuation of the unctuous body into the pores having altered them as to figure, or to ſize, or to both, and having by that alteration given the paper a texture diſpoſed to allow due paſſage to the corpuſcles of light, or to tranſmit their peculiar kind of impulſe (whence ſeveral naturaliſts derive light) the motions, as I was ſaying, or inviſible corpuſcles in the air, depending upon the conſtitution of the world, do preſently act upon the paper, and produce beyond it both a ſenſation of light, and the repreſentations of a multitude of objects, whence the light reflects, and which could not be ſeen through it before.

I need not perhaps tell you, that if a pretty large box be ſo contrived, that there may be towards the one end of it a fine ſheet of paper, ſtretched like the leather of a drum-head, at a convenient diſtance from the remoter end, where there is to be left an hole covered with a lenticular glaſs fitted for the purpoſe, you may at a little hole, left at the upper part of the box, ſee upon the paper ſuch a lively repreſentation, not only of the motions, but ſhapes and colours of outward objects, as did not a little delight me, when I firſt cauſed this portable darkned room, if I may ſo call it, to be made. Which inſtrument I ſhall not here more particularly deſcribe, partly becauſe I ſhewed it you ſeveral years ago, ſince when diverſe ingenious men have tried to imitate mine (which you know was to be drawn out or ſhortned like a teleſcope, as occaſion required) or improve the practice; and partly, becauſe that, which I pretended in mentioning of it here is, to ſhew, that ſince that almoſt upon every turning of the inſtrument this way or that way, whether it be in the town or open fields, one may diſcover new objects, and ſometimes new landſcapes upon the paper, there muſt be all day long in all parts of the air, where this phænomenon can be exhibited, either certain effluvia emitted every way from the objects, or certain motions of inſenſible corpuſcles, which rebounding firſt from the external object, and then from the paper, produce in the eye the images of theſe objects: ſo that the air is every where full of viſible ſpecies, which cannot be intelligibly explicated without the local motions of ſome minute corpuſcles, which, whilſt the air is enlightened, are always paſſing thorough it.

You may remember, *Pyrophilus*, that in the clauſe of the ſecond propoſition, hitherto diſcourſed of, I take in the eſtabliſhed laws of the univerſe, as a part of the preſent conſtitution of this our world; ſome of thoſe laws contributing much to the operation of thoſe unheeded cauſes, we are treating of. Of theſe I may another time give you ſome inſtances; but for the preſent it may ſuffice to take notice of this one, that if you take a bar of iron, and holding it perpendicularly, apply the loweſt part of it to the northern point of a well-poiſed magnetical needle, the bar will preſently drive it away: but that magnetiſm, by which the bar does it, as it is preſently acquired by the poſture which it had, ſo it is as ſuddenly changed, if you invert that poſture; as appears by this, that though you hold the bar perpendicular, if it be held under the needle, ſo that the ſame part of the bar, which before was placed directly over the north point of the needle, be held directly under the ſame point, the bar will not, as before, drive it away, but, as they commonly ſpeak, attract it. But if this bar have been for a long time kept in an erected poſture, as if it be taken from ſome old window, or if, having been heated and refrigerated, it have very long lain north and ſouth, it will appear endowed with a ſtronger and more durable verticity, as we elſewhere more fully declare; which ſeems to proceed from this, that by lying north and ſouth, it lay in the way, which, according to the eſtabliſhed laws of nature, the magnetical effluvia of the earth muſt paſs along in ſteams from pole to pole; whereby they have the opportunity by little and little to work upon the pores of the iron, that lies in their way, and fit them to give paſſage to the effluvia of magnetical bodies; in which fitneſs ſeems principally to conſiſt the magnetiſm of iron: whereas, if this metal had all this while lain eaſt and weſt, inſtead of north and ſouth, it would have acquired little or no magnetical virtue. And the reaſon, why an erected poſture gives a rod or bar of iron a power to drive away the north point of the needle, has been probably conceived to be this, that the lower end being nearer the earth does more plentifully participate of the magnetick ſteams, which fly in a cloſer order there, than further off, and by powerfully affecting that part of the iron, turn it, for a time, into the iron's north pole, which according to the laws magnetical ought to drive away the north pole of the needle, and attract the ſouth; whereas, if the bar being inverted, that end, which was uppermoſt, becoming the lower, muſt for the ſame reaſon have a contrary operation, unleſs by having long ſtood, its verticity be too well ſettled to be ſuddenly deſtroyed or altered by the effluvia of ſo languid a magnet as the earth. But whether or no this explication be the right one (for I would not contend for its being ſo) it appears, by the requiſiteneſs both of a determinate poſition of the iron, and of its long continuance in that poſition, to make that metal acquire a durable verticity, that thoſe unheeded magnetical ſteams, which communicate ſuch a magnetiſm to the iron, move and act according to laws eſtabliſhed in nature: which is as much as my deſign in this diſcourſe makes neceſſary to be made out.

CHAP. VII.

IT remains now, that we diſcourſe of the laſt of our three grand propoſitions, namely, "That a body by a mechanical change of "texture may acquire or loſe a fitneſs to be "wrought upon by ſuch unheeded agents,

“ and alſo to diverſify their operations on it
“ upon the ſcore of its varying texture.”

THIS propoſition is of ſo much affinity with the foregoing, that there are divers caſes, wherein the ſame experiments and other arguments may ſerve for the confirmation of both.

BUT to illuſtrate a little what I mean, by groſs and ſenſible examples, it is a cuſtom we often obſerve at ſea, when we ſail with too ſlack a wind, to take up water with certain inſtruments, and throw it againſt the ſails. At the firſt propoſal, this may ſeem a very improper way to promote the ſwiftneſs of the ſhip, ſince there is the weight of ſo much water added to that of the veſſel itſelf; but yet I have ſeen the ſeamen make uſe of it as one of their beſt expedients, when we were cloſely chaſed by pirates, nor did I look upon it as irrational; for whereas, when the ſails are dry, a good part of the wind, that blows upon them, eaſily gets thorough thoſe meſhes or great pores, that are left between the threads of which a ſail conſiſts; when it comes to be wetted, the imbibed water makes the threads ſwell every way, and conſequently very much ſtreightens the pores or intervals, that were formerly left between them; by which means the wind cannot permeate them as freely as formerly, but by finding a greater reſiſtance in the ſail comes to beat more forcibly upon it, and conſequently drives it, and with it the ſhip, more ſtrongly on, than elſe it would have done: not to mention the ſtiffneſs of the ſail acquired by the imbibed ſea-water, becauſe I would not ſtay to take notice of other particulars, to which the ſucceſs of this practice may perhaps be in part aſcribed.

To add another inſtance to the ſame purpoſe with the former; ſuppoſe an high wind to blow againſt a chamber, wherein the windows and doors are all ſhut, the effect will be only, to ſhake a little the room in general: but if one open the caſement, though he, that does it, do properly and immediately but diſplace ſome little piece of the iron, or other thing, that ſhuts the window, yet this being done in a place, where there is a ſtrong current of air, which we call a wind, there will preſently follow a blowing up of curtains or hangings, and blowing about of duſt, ſtraws, feathers, or other light bodies, that are not firmly enough faſtened, nor very ponderous, and yet are too heavy to be blown about.

BUT to proceed to inſtances, that are not ſo groſs, I might take notice, that though good common tartar does uſually of it ſelf keep dry in the air, nay, and will not eaſily be diſſolved in cold water; yet if it be calcined, though but very moderately, the ſalt in the remaining coal, the texture being now altered, will readily enough in the moiſt air (as that of a cellar) run into that liquor, that chemiſts have been pleaſed to call oil of tartar per diliquium. But in regard that to make the change the greater, part of the tartar muſt be driven away by the fire, I ſhall rather make uſe of an example eaſily drawn from an experiment, I elſewhere mentioned to another purpoſe; for having taken a loadſtone, and according to the way there delivered, heated it and cooled it, though it had loſt ſo little by the fire, that the eye took no notice of its being changed either as to ſhape or bulk, yet the operation of the fire, by changing the inviſible texture, did ſo diverſly alter the diſpoſition of it in reference to the magnetical effluvia of the earth, that I could preſently and at pleaſure change and realter the poles of the ſtone, making the ſame end point ſometimes to the north and ſometimes to the ſouth. The like change of verticity I have, as I elſewhere declare, made mere iron capable of, without the help of fire or any other magnet than the earth; and I have alſo found by trial, that a certain heavy ſtone, that is uſually thought to be not ſo much as of a metalline nature, may by a ſlight and quick preparation, that alters not the ſhape nor bigneſs, be enabled to attract and repel the poles of a magnetic needle.

CHAP. VIII.

TO the inſtances already given in ſolid bodies it will not be amiſs to annex two or three in liquid bodies, becauſe it may be thought ſtrange by ſome, that conſiderable changes of texture ſhould, without fire or any new ingredients, be produced in bodies, which, by reaſon of their fluidity, ſeem preſently to recover their texture if it be diſordered. If honey and water be each of them apart put into a convenient veſſel, they will both of them retain their nature; and though you mix them together in an undue proportion, ſo that by reaſon of overmuch honey the conſiſtence be too thick, or that by being diluted by too great a proportion of water, the ſolution of honey be too thin, they may continue honey and water: but if thoſe two liquors be duely proportioned (as if you put to one part of honey four or five of water) then their new texture ſo diſpoſes them to be acted on by the ſubtle permeating matter, or whatever other common agent nature imploys to produce fermentations, that the ingredients do no longer continue what they were, but begin to work like new muſt, or beerwort: and I have tried, that ſo ſmall and ſhort a local motion (as carrying ſuch mixtures a while in a coach) has ſo excited the liquor, as to make it violently force its way out of the veſſel, or throw off the ſtopple, that I have wondered at it. And I remember, that an eminent merchant of wines, who ſpent divers years in the *Canaries*, being aſked by me about ſome things of this nature, aſſured me, that in thoſe fortunate iſlands (as the ancients ſtyle them) he had ſeveral times obſerved, that if a pipe of the beſt ſort of Canary were, when it was about a month old, rudely rolled, though but the length of an hall or moderate gallery, ſo tranſient and ſlight a diſcompoſure of the texture would quickly make ſo great a change in it, that oftentimes a good quantity of wine would be violently thrown out at the bung; or if the pipe were too cloſe ſtopped, that great veſſel it ſelf would oftentimes have the bottom beaten out; by which means he had known ſeveral pipes of that rich liquor loſt.

WE have divers examples of the cracking of common glaſs, when it is too ſoon, after it hath been removed from the fire, expoſed to the cold air, and the ſubtle bodies that are in it; which would not have cracked it, if it had been cooled more ſlowly, ſo that its parts would have had leiſure to ſettle into a texture convenient for the paſſage of thoſe ſubtle bodies, which in that caſe would harmleſly have permeated it. But I have ſometimes ſhewn the curious a more quick and manifeſt inſtance of the importance of the preſent texture of a body in reference to the catholick and inviſible cauſes, that may work upon it. For having taken a plate of ſo ponderous and ſolid a body as copper, and heated it red hot, and then ſuffered it to cool a while upon ſome more moderately hot place in the fire, though it did not appear at all ignited, when I removed it to a plate, or even to a ſheet of paper; yet upon its being expoſed to the atmoſphere, the ſuperficial part would not only crack as in over haſtily cooled glaſs, but would, and that preſently, fly off in flakes in good number, and not without noiſe; ſo that in a ſhort time I have had the neighbouring part of the paper, on which the braſs plate reſted, almoſt quite covered with little ſcales, as it were, of that metal.

AND to give you, in favour of what I have been hitherto diſcourſing, an inſtance of a very ſubtle nature, I will not, though I juſtly might, take notice, that in rotten fiſh and rotten wood, the change of texture is oftentimes inviſible, that will ſuffice to make the contact of the air, and the ſubtle corpuſcles, whereto it gives harbour or paſſage, confer or loſe a power of ſhining: but I will rather chuſe to inſtance in the Bolonian ſtone, which by calcination acquires this admired property, that if it be but expoſed to the ſun beams (to which I have found other ſtrong lights ſuccedaneous) it will not only in a few minutes acquire a luminouſneſs, but for ſome time after retain it in the dark.

COSMICAL SUSPICIONS

(SUBJOINED AS AN
APPENDIX
To the DISCOURSE of
The COSMICAL QUALITIES of THINGS.)

IN the former eſſay, *Pyrophilus*, I propoſed to you ſome things about the ſubject there treated of, that ſeemed to have in them ſuch a degree of probability, as is wont to be thought ſufficient to phyſical diſcourſes, or at leaſt is uſually to be met with in them. But in regard the world, whether we take it in the larger ſenſe for the whole univerſe, or, in the more narrow but not leſs common acception, for the globe we men inhabit, is a ſubject ſo vaſt, that not only all demonſtrable truths, that may be diſcovered concerning it, may be looked upon as important, but even conjectures and ſuſpitions themſelves, that relate to it in general, if they be not very groundleſs or extravagant, may deſerve not to be altogether paſſed by in ſilence. I will adventure to entertain you a while with ſome thoughts of this nature, eſpecially becauſe they will give me opportunity to alledge in their favour ſome hiſtorical obſervations, which, whatever the doubts or conjectures be thought of, may appear to be more new then deſpicable.

IT may now therefore be not unſeaſonable to confeſs to you, that I have had ſome faint ſuſpicion, that beſides thoſe more numerous and uniform ſorts of minute particles, that are by ſome of the new philoſophers thought to compoſe the æther I lately diſcourſed of; there may poſſibly be ſome other kind of corpuſcles fitted to have conſiderable operations, when they find congruous bodies to be wrought on by them. But though it is poſſible, and perhaps probable, that the effects we are conſidering, may be plauſibly explicated by the æther, as it is already underſtood; yet I ſomewhat ſuſpect, that thoſe effects may not be due ſolely to the cauſes they are aſcribed to, but that there may be, as I was beginning to ſay, peculiar ſorts of corpuſcles, that have yet no diſtinct name, which may diſcover peculiar faculties, and ways of working, when they meet with bodies of ſuch a texture, as diſpoſes them to admit, or to concur with the efficacy of theſe unknown agents.

THIS ſuſpicion of mine will ſeem the leſs improbable, if you conſider, that though in the æther of the ancients there was nothing taken notice of but a diffuſed and very ſubtle ſubſtance; yet we are at preſent content to allow, that there is always in the air a ſwarm of ſteams moving in a determinate courſe betwixt the north pole and the ſouth; which ſubſtance we ſhould not probably have dreamed of, if our inquiſitive *Gilbert* had not happily found out the magnetiſm of the terreſtrial globe. And few, perhaps, would have imagined, that when an hunted and wounded deer has haſtily paſſed over a little graſs, he ſhould leave upon it ſuch determinate, though inviſible, effluviums, as ſhould, for many hours, ſo impregnate the air, as to betray the individual flying and unſeen deer; if there were no blood-hounds, upon whoſe peculiarly diſpoſed organs of ſmelling theſe ſteams are fit to operate. And it is ſtrange, that there ſhould be ſuch effluvia for a long time (perhaps a year or two together) reſiding in the air, that though our ſenſes diſcern them not, and though they have no operation upon other men; yet if they meet with perſons of a peculiar temperament, who by that, and by their formerly having had the plague, have attained a peculiar diſpoſition, that fits them to be wrought on by peſtilential ſteams, they may ſo operate upon them, that ſome of theſe perſons may be able to diſcern thoſe ſteams to be peſtilential. To give ſome countenance to which paradox, I will here annex two or three teſtimonies, the firſt of which I find thus ſet down among my Adverſaria. [Above three months before the late great plague began in *London*, in the year 1665, there came to Dr. *M.* a patient of his, to deſire his advice for her huſband; and the doctor having enquired what ailed him, ſhe anſwered, that his chief diſtemper was a ſwelling in his groin, and upon that occaſion added, that her huſband aſſured her of his being confident, that the next ſummer the plague would be very riſe in *London*; for which prediction he gave this reaſon, that in the laſt great plague he fell ſick of that diſeaſe; and he then had a peſtilential tumour.

So in two other plagues, that ſince happened, though much inferior to that great one, each of them had a riſing in his body to be its forerunner, and now having a great tumour in the forementioned place, he doubted not but it would be followed by a raging peſtilence, which accordingly enſued. Having heard much talk of ſomething of this nature, and being this morning caſually viſited by the doctor, a perſon of great veracity, I enquired of him, how much of it was true, and I received for anſwer the foregoing narrative.]

Monſieur de Roche-fort.

The ſecond is a very remarkable ſtory, which I remember that famous and excellent chirurgeon *Fabricius Hildanus* records of himſelf; namely, that having had a peſtilential tumour during a plague, that happened in his youth, if for many years after he chanced to go to, or ſo much as to paſs by, an houſe infected with the plague, he was admoniſhed of the particular diſeaſe, that reigned there, by a ſenſible pain in that part, where he had had a peſtilential tumour ſo long before.

The third teſtimony is afforded me by that curious obſerver of the changes, that happened as to the phænomena of diſeaſes at the famous ſiege of *Breda*, where this diligent phyſician, practiſing much among patients afflicted with malignant and peſtilential diſeaſes, was at length infected himſelf; whereupon he informs his readers; *Annotandum hic merito naturæ facultatem ad peſtis præſervationem momenti eſſe maximi. Obſervavi in meipſo contaminatos inviſente ſtatim inguen dolere vel axillas: afficiebatur aliquando caput, noctu inde ſudor, & ſeceſſus tres quatuorve. Hoc & aliis accidit, qui fideliter mihi retulerunt.*

If theſe ſtories were related by ordinary perſons of what happened to other men, the oddneſs of them might well tempt a wary man to ſuſpend his judgment: but the judiciouſneſs of the writers, and the profeſſion they were of, and their relating theſe as things, that did more than a few times happen to themſelves, may well be permitted to bring credit to their aſſertions. And theſe inſtances added to what has been already ſaid, may, I hope, excuſe me, if I think it not time miſpent, to conſider, whether there may not be other, and even unobſerved ſorts of effluvia in the air: to excite your curioſity and attention about which, rather than to declare a poſitive opinion, is that which is pretended to in what has been lately mentioned.

And whereas, *Pyrophilus*, I have in the former diſcourſe taken in the ſtructure and eſtabliſhed laws of the univerſe, as an help toward the giving an account of the coſmical attributes of things; I ſhall here alſo ingeniouſly confeſs to you, that I much fear, whether we have yet attentively enough taken notice either of the number, or the kinds of thoſe laws.

For as I am by ſome notions and obſevations inclined to think, that there may be a greater number even of the more general laws, than have been yet diſtinctly enumerated; ſo I think, that when we ſpeak of the eſtabliſhed laws of nature in the popular ſenſe of that phraſe, they may be juſtly and commoouſly enough diſtinguiſhed; ſome of them being general rules, that have a very great reach, and are of greater affinity to laws more properly ſo called, and others ſeeming not ſo much to be general rules or laws, as the cuſtoms of nature in this, or that particular part of the world; of which there may be a greater number, and thoſe may have a greater influence on many phænomena of nature, than we are wont to imagine.

And firſt, whereas the ſtructure of the world is a main help in our preſent diſquiſition; I ſhall venture to tell you, that though I do not only commend, but in divers caſes admire, the induſtry of aſtronomers and geographers, eſpecially of ſome later ones; yet they have not met with ſuch difficulties, that they have hitherto preſented us, rather a mathematical hypotheſis of the univerſe, than a phyſical, having been careful to ſhew us the magnitudes, ſituations, and motions of the great globes, ſuch as the fixed ſtars and the planets (under which one may compriſe the earth) without being ſollicitous to declare what ſimpler bodies, and what compounded ones, the terreſtrial globe we inhabit does, or may conſiſt of. And as of late years the diſcovery of the four planets about Jupiter, and the little moon (as ſome call it) that moves about Saturn, together with the phænomena of comets, have obliged the ſkilful to alter divers things in the theory of celeſtial bodies: ſo I know not, but that future diſcoveries by improved teleſcopes and other philoſophical inſtruments may reduce us to make changes in the grand ſyſtem of the univerſe itſelf, and in that which we conſider as the moſt important of the mundane bodies to us, the terraqueous globe we live on.

WHAT communication this may have with the other globes we call ſtars, and with the interſtellar parts of heaven, we have very little knowledge of, though I may elſewhere make it probable, that there may be ſome commerce or other: but without ſpeaking more particularly of that point, I confeſs I have ſome time ſuſpected, that there may be in the terreſtrial globe itſelf, and the ambient atmoſphere, divers, whether laws or cuſtoms of nature, that belong to this orb, and may be denominated from it, and ſeemed to have been either unknown to, or overſeen by both ſcholaſtical and mathematical writers. And firſt, I have often ſuſpected, whether there may not be in the maſs of the earth ſome great, though ſlow internal change (whether originated there, or produced by the help of other mundane globes) by conſidering, that almoſt in all countries, where obſervations have been made, there has been a plain and conſiderable alteration found in that, which is commonly called the variation (for it is rather the declination) of the ſea compaſs or magnetick needle, which is the diſtance, by which the needle declines eaſt or weſt from the true north pole. And whereas formerly, at or * near *London* the compaſs declined, as obſervations ſolemnly made, and upon record aſſure us, in the year 1580, above 11 degrees; in the year 1612, above 6 degrees; in the year 1633, no leſs than about 4 degrees; it has of late been found to have very little or no variation. And at a place within half a league of *London*, trying with a long and curious needle purpoſely made and poiſed, I could ſcarce diſcern any declination at all, and if the needle declined ſenſibly any way from the pole, it ſeemed to do ſo a little towards the other ſide of heaven, than that towards which it did decline before. And having † afterwards by the help of a meridian line, much prized for having been accurately drawn by eminent artificers, made an obſervation in *London* itſelf; though I made it with two inſtruments, whereof one was a choice one, differing from the former, and from one another; I could not ſatisfy myſelf, that I could diſcern the declination of the needle to exceed half a degree, if it amounted to ſo much. But ſince obſervations of this kind may prove more conſiderable than we yet know of, and ſince they ought to be made at diſtant places, I am contented to add here, by way of confirmation, that the *Cape of Good Hope* being one of the eminenteſt parts of the terreſtrial globe in reference to magnetiſms, the acquaintance I had with one of the ancienteſt and moſt experienced navigators of this part of *Europe*, invited me to addreſs myſelf to him, purpoſely to enquire of him, whether he had taken the variation of the compaſs at the *Cape of Good Hope*, and whether, if at all, he had taken it more than once: he anſwered, that he had often done it: whereupon aſking him what he found the variation to be, and whether he had obſerved any change of it in his ſeveral voyages, he replied, that when he was a young ſeaman, he obſerved the variation to be about two degrees weſtward, and afterwards during many years that he ſailed to and fro betwixt *Eaſt-India* and *Europe*, he found the variation to encreaſe by degrees; and whereas he had learned from ancient writings, and the tradition of old ſeamen, that before his time, they had found no variation at all, he about 15 years ago (which was the laſt time he took it) found it by accurate inſtruments, to be 6 degrees and about 48 minutes. So that during the time, that he practiſed the ſeas about the *Cape of Good Hope*, the variation ſtill weſtward had decreaſed near 5 degrees. Upon theſe grounds, which I may elſewhere have occaſion to confirm by further obſervations, I cannot but think it probable, that there may be agents, that we know not of, that have a power to give the internal parts of the terreſtrial globe itſelf a motion; of which we cannot yet certainly tell, according to what laws it is regulated, or ſo much as whether it be conſtantly regulated by certain laws or no. And what other changes, agents that can produce a change in the terreſtrial globe itſelf may make in this, or that part of it, who can inform us?

IN the next place, I conſider the great uncertainty and irregularity, that we have hitherto obſerved in the weight of the atmoſphere by our new ſtatical barometers, and much more ſenſibly by mercurial ones, without yet having diſcovered the cauſes of ſuch conſiderable alterations in the air, (ſave that in general they proceed for the moſt part from ſubterraneal ſteams) whoſe influences upon other things may be more conſiderable, than we have yet had opportunity to detect.

Monſieur de Rochefort.

IT is very remarkable what a late and ingenious writer, that lived in ſome of the American iſlands, relates about the hurricanes in thoſe parts; namely, that before the Europeans came thither, the inhabitants obſerved, that they had thoſe fatal tempeſts once in ſeven years and no oftener; afterwards, they were troubled with them but once in ſix years; and in proceſs of time, the unwelcome viſits of thoſe winds grew ſo frequent, that in my relator's time they came once a year; and, as a prodigy, they once obſerved two in one year; and afterwards three in another. I remember alſo, that meeting with an inquiſitive gentleman, that had lived in *New-England*, I deſired to know of him, whether in that part of the country, where he reſided, there were not a great change made in the very temperature of the climate? whereto he anſwered me, that there was, for it was grown much milder than formerly: and becauſe I doubted, whether this change might not have been, either accidental for a year or two, or apparently to the Engliſh, whoſe bodies

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* At *Limehouſe*.

† A. D. 1669.

dies by degrees might grow more accuſtomed to the coldneſs of the country, and leſs ſenſible of it; it was anſwered, that this change had been obſerved for many years after the Engliſh had planted a colony there, and that the change was manifeſtly perceived by the natives too, by the remiſſer operation of the cold upon running and ſtanding waters, which were formerly wont to be frozen at ſuch and ſuch times. And I ſhall add for confirmation, that having one day the honour to be ſtanding by his majeſty, when he received a ſolemn addreſs from *New-England*, delivered by the governor of a colony there; that very inquiſitive monarch, amongſt other queſtions, aſking him about the temperature of the air, he told his majeſty, in the preſence of divers that came from *America* with him, "that the climate had much "altered and loſt much of its former coldneſs "for divers years, ſince the Engliſh ſettled "there."

WHETHER this decrement of the ſharpneſs of the air will proceed, or how long it will continue, time will diſcover. But in the mean while, ſuppoſing with him the matter of fact to be true, and that the change depends not on any manifeſt cauſe; that, which is happened already, ſeems to me very conſiderable, ſince I have lighted on a book [*] written by [†] one of the ancient planters of *New-England*, by way of deſcription of that country; where, among other things, I find thoſe notable paſſages. The one in the ſeventh page: in former times, ſays he, the rain came ſeldom, but very violently, continuing its drops, which were great and many, ſometimes 24 hours togethers, ſometimes 48, which watered the ground for a long time after: but of late the ſeaſons are much altered, the rain coming oftner, but more moderately, with leſs thunder and lightnings, and ſudden guſts of wind. And the other in the 84th page; where ſpeaking of the heathen natives, he ſays, they acknowledge the power of the Engliſhman's God, as they call him, becauſe they could never yet have power by their conjurations to damnify the Engliſh either in body or goods: and beſides they ſay, he is a good God, that ſends them ſo many good things, ſo much good corn, ſo many good cattle, temperate rains, fair ſeaſons, which they likewiſe are the better for ſince the arrival of the Engliſh; the times and ſeaſons being much altered in ſeven or eight years, free from lightning and thunder, long droughts, ſudden and tempeſtuous daſhes of rain, and lamentable cold winds.

So that by this it appears, that this grateful decrement of the coldneſs and rudeneſs of that climate was already taken notice of ſo [‖] many years ago.

TO theſe relations may pertinently be ſubjoined a paſſage of the learned *Magnenus* in his ingenious little tract *de Mannâ* [‡]; where he very ſolemnly delivers this notable obſervation, that in the country he calls *Cenotria*, there was no manna to be found a little above three hundred years ago. And that in *Calabria* it ſelf, a province ſo famous for manna, that the beſt is denominated thence, and that furniſhes a great part of *Europe* with that odd drug, it is but ſince two ages, or thereabouts, that manna has fallen, or, as he expreſſes it, rained.

I know not whether it may be worth while to mention, after theſe more weighty obſervations, the œconomical traditions of houſe-wives; which I ſhould not think worth taking notice of in this place, but that having purpoſely enquired after the truth of it, of two very ſober perſons (much verſed in the art of making ſweatmeats) that have, eſpecially one of them, often tried it; they ſeriouſly affirmed to me, that they find the ſpots made in linnen by the juices of fruit, particularly of red currants in ſtraining bags, will beſt waſh out (nay ſcarce otherwiſe) at that time of the year, when thoſe fruits are ripe the enſuing year.

To which may be for affinity's ſake annexed, what is related by the ingenious French writer of the hiſtory *Des Iſles Antilles*[**], where he lived divers years; who ſpeaking of the fruit they there call *Acajou*, tells us, that the juice of ſome of the internal parts of it, though reputed an excellent remedy in fainting fits, is of ſuch a nature, that if it chance to fall upon a piece of linnen, it turns to a red ſpot; which laſts till the tree come to be again in flower. Which phænomena, if the length of time, and the heat and temperature of the air, uſual in the ſeaſons of producing bloſſoms, and ripening of fruits, be found to have little or no intereſt in their cauſation, may prove of ſome uſe in our preſent enquiry.

WHATEVER be the true cauſe of the ebbing and flowing of the ſea, yet at ſpring tides the motions of ſuch vaſt maſſes of matter as the great ocean, and moſt of the ſeas, are ſo conſtantly coincident with the new and full moon; and the more ſtupendous ſpring-tides have been in moſt places, ſo long obſerved to happen regularly enough about the æquinoxes, that it is worth an enquiry, (though I cannot here afford it one) whether theſe conſpicuous phænomena may not ſomewhat confirm the conjectures we are diſcourſing of.

AND when I remember, how many queſtions I have aſked navigators about the luminouſneſs of the ſea; and how in ſome places the ſea is wont to ſhine in the night as far as the eye can reach; at other times and places, only when the waves daſh againſt the veſſel, or the oars ſtrike and cleave the water; how ſome ſeas ſhine often, and others have not been obſerved to ſhine; how in ſome places the ſea has been taken notice of, to ſhine when ſuch and ſuch winds blow, whereas in other ſeas, the obſervation holds not ; and in the ſame tract of ſea, within a narrow compaſs, one part of the water will be luminous, whilſt the other ſhines not at all: when I ſay, I remember how many of theſe odd phænomena belonging to thoſe great maſſes of liquor I have been told of by very credible eye-witneſſes (whoſe narratives to me you may elſewhere meet with) I am tempted to ſuſpect, that ſome coſmical law or cuſtom of the terreſtrial globe, or, at leaſt, of the planetary vortex, may have a conſiderable agency in the production of theſe effects.

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[*] Intitled, *New-England*'s proſpect. [†] Mr. *W. Wood.* [‖] The book was publiſhed thirty-five years ſince.

[‡] Sanctiorum naturæ interpretum nullus fraxinum inter arbores gummiferas, aut reſiniferas recenſuit.

Illud omnino, quo *Altomatus* ſeſe jactare videtur, ignoravêre curioſiſſimi rerum indagatores, *Plinius*, *Galenus*, *Theophraſtus*, & qui mediam ætatem impleverunt viri doctrinâ diligentiâque celebres: quia ſcilicet illis temporibus multum pluebat in *Calabria* manna, quod à duobus tantummodo ſeculis legi cœptum. Dic amabo, *Altomate*, cur ante trecentos annos multum manna fuit in *Cenotriâ*: jam certe aderant pagi ibidem urbeſque vicinæ, neque vero feſelliſſet curioſam incolarum ſolertiam nihil plane video, quod pro te adduci poſſit ad hujus difficultatis evitandas anguſtias. *Magnenus de Manna*, *P. M.* 49.

[**] Hiſtoire Naturelle des Iſles Antilles. *Liv.* I. Chap. 6.

NOR am I ſure, that ſome ſubterraneal changes, or ſome yet unobſerved commerce between the earth and other mundane globes, has not an intereſt in the origin, continuance, and expiring of thoſe diſeaſes, that phyſicians call new, which invade whole countries (and ſometimes greater portions of the earth) and laſt very many years, if not ſome ages, before they come to be extinct. Of which ſorts of diſeaſes divers learned men have reckoned up divers, and whereof the venereal pox, at leaſt, as to its origine and ſpreading, is but too manifeſt and unhappy an inſtance ; whereto, according to ſome eminent doctors, we may add the rickets, a diſeaſe, which though ſcarce known in other countries, is here in *England* ſo fatal to children, which firſt (as is affirmed) diſcovered itſelf among us within the memories of multitudes of men yet alive : but of this perhaps more elſewhere.

IF I ſhould now further deſcend to the peculiar phænomena of particular regions, I muſt launch out into a diſcourſe I could not have the leiſure to finiſh. And therefore I ſhall only advertiſe you of two ſuſpicions more, that I hold not unfit to intimate to you, about the eſtabliſhed laws and cuſtoms of nature.

THE firſt of them is this, that I doubt thoſe, that are thought the grand rules whereby things corporeal are tranſacted, and which ſuppoſe the conſtancy of the preſent fabrick of the world, and courſe of things, are not altogether ſo uniform complied with, as we are wont to preſume ; at leaſt, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions. For what reaſon the wiſe author of nature pleaſed to permit, that it ſhould be ſometimes, as it were, over-ruled by the boiſterouſneſs (if I may ſo call it) and exorbitant motions of unruly portions of matter, I muſt not in this place (though I do it in another) inquire : but when I conſider the nature of brute matter, and the vaſtneſs of the bodies, that make up the world, the ſtrange variety of thoſe bodies, which the earth does comprize, and others of them may not abſurdly be preſumed to contain ; and when I likewiſe conſider the fluidity of that vaſt interſtellar part of the world, wherein theſe globes ſwim ; I cannot but ſuſpect there may be leſs of accurateneſs, and of conſtant regularity, than we have been taught to believe, in the ſtructure of the univerſe, and a greater obnoxiouſneſs to deviations, than the ſchools, who were taught by their maſter *Ariſtotle* to be great admirers of the imaginary perfections of the cœleſtial bodies, have allowed their diſciples to think. And in effect, to ſpeak only of the nobleſt of them, the ſun, and to paſs by about his motions the obſervation of the exacteſt aſtronomers, that natural days are not all of equal length (whatever the vulgar of philoſophers ſuppoſe to the contrary ;) and not to take notice of the great diſpute betwixt the eminenteſt aſtronomers, even of our times, about the anomaly attributed to the motion of the ſun's apogeum : to paſs over theſe things, I ſay, the ſun himſelf doth not only, from time to time, do what divers of our later aſtronomers ſtile to vomit our great quantities of opacous matter, (which are called his ſpots) ſome of them bigger, perhaps, than *Europe* or *Aſia*, but has had almoſt his whole face ſo darkened with them, (as about the end of *Cæſar*'s, and the beginning of *Auguſtus*'s government) that for about a year together, he was, as it were, under an eclipſe. To which, if we add thoſe cœleſtial comets, (for I diſpute not now about ſublunary ones) their number, vaſtneſs, duration, odd motions from orb to orb, (as the ancients would have ſpoken) and other phænomena, (whatever the cauſes of them be) it will appear, that even in the cœleſtial part of the world, all is not ſo regular and unvariable, as men have been made to believe.

I had ſome doubts, whether this might not be much confirmed by what has been related by ſome navigators, that have been in the ſouth-ſea, about certain black clouds, ſaid to move as regularly in the antartick hemiſphere, as the neighbouring ſtars themſelves ; to which ſome of our Engliſh ſeamen (whether firſt or no, I know not) have added, certain white clouds in the ſame hemiſphere move no leſs regularly. Of theſe relations, I ſay, I conſidered, whether ſome uſe might not be made to my preſent purpoſe ; but having made the beſt inquiry I could, of thoſe few perſons of note I could meet with, that were likely to inform me, I do not yet ſee cauſe to alledge theſe phænomena by way of arguments. But yet ſince I find, that even pilots, who have been frequently in ſome parts of the *Eaſt-Indies*, have not (whether becauſe they ſailed not far enough to the ſouthern pole, or upon ſome other ſcore) taken notice of them ; I ſhall ſubjoin as a part of natural hiſtory, not obvious to be met with, the beſt account I could procure of them ; which was from an obſerving captain of an Eaſt-India ſhip, with which he lately adventured to unfrequented parts of the ſouth-ſea.

THE ſubſtance of his anſwers to me, about the fore-mentioned phænomena, was this, that he had divers times ſeen in the ſouthern hemiſphere, and in that part of the milky way, which is not to be ſeen upon our horizon (for he ſays, the galaxy is either compleatly, or almoſt a circle) two or three places, that look like clouds, and move about the earth regularly with the white part of the circle in 24 hours. But by what he replied to ſome further queſtions, that I aſked him, I gathered, that if theſe be the black clouds, that navigators have ſpoken of, thoſe, that gave them the name of clouds, were probably much miſtaken; ſince, he anſwered me, that theſe are not black, but of a deep blue; which makes me ſuſpect them to be but perforations, if I may ſo ſpeak of the milky way, by which I mean parts of the azure-ſky, that are ſuffered to be ſeen by the diſcontinuations of the parts of the galaxy. And to this account of the dark clouds, his further anſwers gave me this of the white ones; which, he ſays, ſome call the Magellanick clouds; about which he related:

THAT he had divers times ſeen towards the ſouth-pole, the clouds, that ſome few navigators mention to be there, and to move about the pole in 24 hours.

THAT he began to diſcover them plainly, when he was in about 18 degrees, (as I remember) of ſouth latitude.

THAT they were white, in number three (though two of them be not very diſtant from each other) the greateſt being far from the ſouth pole; the other not many degrees remoter than that ſtar, which of the * conſpicuous ones, they reckon to be neareſt to the pole, though it be about eleven degrees diſtant from it.

BUT from this account of his I dare not, as I was intimating, conclude theſe to be ſuch clouds as they are taken for, becauſe, for ought I know, if they were looked on through a good teleſcope, they would be found conſtellations of ſmall and ſingly inconſpicuous ſtars, like thoſe of the galaxy, the belt of Orion, &c. But to be reſolved about theſe matters, it is not amiſs to expect further obſervations; the propoſed conjectures being made but upon a ſuppoſition of the truth and ſufficiency of the relations.

AND thus much for the firſt of the two ſuſpicions, that I above intimated, I would propoſe to you: the other is very different from it, and might ſeem contradictory to it, but that they belong not to the ſame caſes. For though I lately told you, I ſuſpected, that in ſome things, eſpecially relating to the lines, according to which, and the limits within which ſome great maſſes of matter are ſuppoſed to perform their motions, there is more accurateneſs fancied than there really is; yet I ſhall now add, that there are caſes, wherein I am not quite out of doubt, but that we may ſometimes take ſuch things for deviations and exorbitancies from the ſettled courſe of nature, as, if long and attentively enough obſerved, may be found to be but periodical phænomena, that have very long intervals between them. But becauſe men have not ſkill and curioſity enough to obſerve them, nor longævity enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, that ſpring not from any ſettled and durable cauſes. For the world, like a great animal producing ſome effects but at determinate ſeaſons, as nature produces not beards in men, till they have attained ſuch an age, and the menſes (as they call them,) uſe not to happen to women before they come to ſuch years, nor to laſt beyond ſuch other years of their life; as may be alſo obſerved within a far ſhorter compaſs of time in the growth and falling of ſtags horns and bucks: if the firſt man had lived but one year in the world, he would perhaps have thought the bloſſoming of trees in ſpring, and their bearing fruit in ſummer, but an accidental thing, and would have looked upon the eclipſe of the ſun, as a prodigy of nature; obſerving, that though every new moon, the ſun and ſhe came very near together, yet neither before nor after was there any ſuch terrible phænomena conſequent thereupon. And we ourſelves may eaſily remember, what ſtrange conjectures we had of the ſtrangely varying appearances of Saturn, for divers years after our teleſcopes firſt diſcovered them to us.

BUT moſt remarkable is that cœleſtial phænomenon afforded us by the emerging, diſappearing, and re-appearing ſtars of this age; which have been obſerved in the girdle of *Andromeda*, and in or about the ſwan's breaſt; (which is ſaid to have been ſeen in the year 1600, and to have vaniſhed in 1621.) and eſpecially that, which having about 25 years ago appeared for a while in the whale's neck among the fixed ones, and afterwards by degrees diſappeared, was looked upon by thoſe aſtronomers of that time, who did not out-live it, as a cœleſtial comet. But afterwards an ingenious Engliſh gentleman of my acquaintance having obſerved here (as well as the vigilant curioſity of ſome few later aſtronomers hath taken notice of elſewhere) the return of the like phænomenon in the ſame part of heaven; it begat much wonder in all (which was encreaſed by the ſlow diſappearing of it) and in ſome curious men a reſolution to have a watchful eye upon that part of the ſky. Since when the juſtly famous *Bullialdus*, and beſides ſome eminent foreign virtuoſi (whoſe names I know not) divers excellent perſons of our own nation having taken notice of it in the wonted place, (where I had ſometimes the ſatisfaction of ſeeing it;) theſe obſervations, and eſpecially the laſt diſappearance of a ſtar judged to have been placed among the fixed ones, and eſtimated to be of the fourth (if not the third) magnitude, have ſomewhat confirmed me in the ſuſpicion I am now treating of. For if this and the other new ſtars do continue to return periodically to the ſame part of heaven, where they have been already long ago ſeen; as at leaſt for as much as concerns this, its gradual increaſing after it firſt begins to ſhew it ſelf, and decreaſing afterwards, ſeem to promiſe; then I may with ſomewhat more of probability than before, ſuſpect, that there may be vortices beyond the concave ſurface of what we call the firmament; which ſuſpicion, if true, would much diſfavour the hypotheſis we now have about the ſyſtem of the world, and will favour what I conjectured as poſſible about periodical phænomena. And however; if either the new ſtar, without departing from its place, be only

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* This wary expreſſion keeps my relator from being contradicted by a curious modern aſtronomer, who tells of a ſtar not three degrees diſtant from the ſouthern pole; but then he ſays too, that it is a ſtar of but the fifth magnitude.

only ſometimes by degrees overſpread and hid by ſpots, like thoſe I formerly mentioned to have obſcured the ſun, which are afterwards by degrees diſſipated, as I at firſt ſuſpected; or if it have a dark hemiſphere as well as a light one, (or rather a greater part of its globe obſcure than luminous, as *Bullialdus* ingeniouſly conjectures) and by turning ſlowly about its own center and axis, doth ſometimes obvert to our eyes its luminous part, and ſometimes its dark part (as Jupiter is ſaid to do its belt-like ſpots, whence it muſt gradually both appear and diſappear; according to either of theſe two hypotheſes, (though not ſo much as in that which preceded them,) there will be reaſon to queſtion the great uniformity imagined to be in the cœleſtial bodies and motions; and to favour what has been propoſed about periodical mutations in the mundane globes; eſpecially ſince theſe phænomena argue, that even thoſe ſtars we call fixed, and have looked upon as ſo invariable, are ſubject to mutations great enough to be taken notice of by our naked eyes at ſo immenſe a diſtance. I ſhall not here proſecute this diſcourſe, becauſe I would not anticipate what I foreſee I ſhall have occaſion to ſay about the terreſtrial effluvia with their cauſes and effects in another diſcourſe *, but I think myſelf obliged to mind you in this place, that doubts and ſuſpicions are the only things promiſed by the title of this diſcourſe; and therefore I ſhall not quarrel with you, if you conjecture, that though the laſt propoſed ſuſpicion may prove well grounded in ſome caſes, yet in ſome others, the exorbitancies of the matter may, if they chance to be repeated, occaſion a new cuſtom, that may have the force of a law in this, or that part of the mundan globes; particularly in this terreſtrial one we inhabit: As waters, by their frequent overflowings of the banks, that cannot contain them, do ſometimes make themſelves new paſſages by their own deviations, and as it were, affect to run in the chanel they once made. And as it happens alſo in animals, that noxious humours having once found a vent at an iſſue or an ulcer, do conſtantly take their courſe that way. Which brings into my mind this odd obſervation, that having occaſion to paſs ſome years ago out of *England* into *Ireland*, traverſing the maritime county of *Waterford*, the convoy, that went with me, ſhewed me once in my way, at a pretty diſtance off, a mountain, from whoſe higher parts there ran precipitouſly a river (which by my eſtimate was pretty broad) that within but two or three years before, at furtheſt, firſt broke out without any manifeſt cauſe from a great bog, that had been immemorially at the top of that mountain, and to the wonder of the inhabitants, after the firſt eruption of the water, had ſupplied the country with a river ever ſince: the circumſtances of which new phænomenon, I would gladly, at a nearer diſtance, have obſerved, but the convoy was not fond of a curioſity ſo dangerous, in an enemy's country.

OTHER inſtances to the ſame purpoſe I cannot now conveniently ſtay to preſent you, having already made the conjectural part of this eſſay diſproportionate to the other: and I hope there is already enough ſaid in this latter part, to anſwer my deſign, which was to excite your curioſity to ſeek after ſome certainty touching the things doubted of; and ſtrive to enable yourſelf by watchful obſervations, ſomewhat to eaſe me of the troubleſome ſuſpicions I have confeſſed to you, by telling me, whether they are altogether groundleſs or not.

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* The reference here made, is to a Tract about the Effects and Cauſes of ſome unheeded Changes in the Air.

OF

OF THE TEMPERATURE OF THE SUBTERRANEAL REGIONS, As to HEAT and COLD.

ADVERTISEMENT.

THE two following tracts were deſigned to have been accompanied by three or four others, whereof the firſt treated about the temperature of the regions of the air, as to heat and cold, and had been premiſed to the two, that now come forth, had it not been judged more proper to reſerve them to accompany ſome other papers concerning the air. To the following tract about the ſubmarine regions, it is thought fit to adjoin ſome relations about the bottom of the ſea; to which was to have been added ſome obſervations concerning the ſaltneſs of the ſea: but in that treatiſe, ſome blanks having been left for particulars, which the author could not ſeaſonably find among his looſe papers to fill them up with, theſe that now appear, having no dependance on them, it was not thought fit they ſhould ſtay any longer for them.

But about theſe ſeveral tracts, this general advertiſement is to be here given, that being hiſtorical pieces, conſiſting chiefly, (though not only,) of ſuch particulars, as the author muſt owe to the informations of others, he would not ſtake his reputation for the truth of every one of them; contenting himſelf, to have performed what can be reaſonably expected of him; which is, that he ſhould carefully make his inquiries from credible perſons, who, for the moſt part, deliver their anſwer upon their own knowledge; and that he ſhould faithfully ſet down the accounts he procured from ſuch relators.

Of the TEMPERATURE *of the* SUBTERRANEAL REGIONS, *as to* HEAT *and* COLD.

CHAP. I.

IF when I uſed to viſit mines, I had thought of writing on the ſubject I am now about to treat of, and had deſigned to ſatisfy myſelf about the temperature of the ſubterraneal air, as much as I did about the other ſubjects I was then concerned to be informed of; I think I ſhould have enabled myſelf to deliver much more upon my own obſervation, than I ſhall now pretend to do. But though for the reaſon newly intimated, and becauſe of my being particularly ſubject to be offended by any thing, that hinders a full freedom of reſpiration, I was not ſolicitous to go down into the deep mines; yet after having diſcourſed of the temperature of the air above ground, I preſume it may not be improper or unwelcome, to ſay ſomething of the temperature of the ſubterraneal regions, and of the air reaching thither. For deep mines being places, which very few have had the opportunity, and fewer have had the curioſity to viſit, and of which I have ſcarce found any thing at all obſervable by claſſick authors, and by other writers, but very little, eſpecially that I think probable enough to make uſe of; I preſume it will not be unacceptable to you, if of regions ſo little frequented, and leſs known, I report what I have been able to learn (by diligent enquiry purpoſely made) from the credible relations of ſeveral eye-witneſſes differing in nation, and for the moſt part unacquainted with each other.

Though I do not think it abſurd to ſuſpect, that in ſome places of the earth, the peculiar conſtitution of the ſoil, and other circumſtances, may make it reaſonable to aſſign thoſe places fewer or more regions than three; yet ſpeaking in the general, the ternary number ſeems not inconvenient to be aſſigned to the ſubterraneal regions; not ſo much upon the ſcore of the analogy, that by this diviſion will be eſtabliſhed between the regions of the earth and of the air, as becauſe there ſeems to be a reaſon of the diviſion included in the diviſion itſelf. And indeed experience appears to favour it in the ſubterraneal cavity, that I have hitherto been able to procure an account of from any ocular witneſs, and (very few excepted) one of the deepeſt, that we yet know of in the world. And ſince it has been received for a rule among philoſophers, that, which is perfecteſt or compleateſt in its kind, ought to be the ſtandard, whereby the reſt are to be meaſured, or eſtimated; I ſhall begin the remaining part of this eſſay by a relation, that I obtained from a chymiſt, that had purpoſely travelled into *Hungary*, and other places, to viſit the mines thoſe parts are juſtly famous for; and who bringing me the honour of a compliment from a prince, to whom he belonged, gave me the opportunity of aſking him divers queſtions, his anſwers whereunto (which I preſently after put into writing,) afforded me the enſuing account.

CHAP. II.

THAT very near the orifice of the groove, he felt the air yet warm; but afterwards deſcending towards the lower parts of the groove, he felt it cold, until he came to ſuch a depth, as he had ſcarce attained by a quarter of an hour's deſcent, and that the cold he felt during this time ſeemed to him conſiderable, eſpecially when in deſcending he had reached to a good depth.

That after he had paſſed that cold region, he began by degrees to come into a warmer one, which increaſed in heat, as he went deeper and deeper. So that in the deeper veins he found the workmen digging with only a ſlight garment over them; and the ſubterraneal heat was much greater than that of the free air on the top of the groove, though it were then ſummer.

[What is here mentioned of a cold region in the earth, has been ſince confirmed to me by an ingenious phyſician, upon an obſervation made in another Hungarian mine, (near a town whoſe name I remember not,) that was not of gold, but copper, and of much leſſer deepneſs than that newly ſpoken of. For this relator anſwered me, that in going down, he felt a conſiderable degree of cold. And when I aſked, whether he found the like in his return upwards, he told me, he obſerved it then too. And when I further inquired after the extent of this cold region, he replied, that not expecting to be aſked about ſuch circumſtances, he had not taken particular notice of them; but thus much information my queſtions procured me, that he began to feel the above-mentioned coldneſs, when he could receive no more light at all by the mouth of the groove; and that this cold region laſted, till he came ſomewhat near the bottom, which was eſtimated to be about an 100 fathom or more diſtant (in a ſtreight line) from the top.)

This relation agrees well enough for the main with that ſhort, but conſiderable one of *Morinus*, which I elſewhere cite; who above forty five years ago, viſited the deep Hungarian mines in the month of July, and takes notice, that when he came down to the burrows, as he calls them, he did not find any heat, as at the mouth of the well, but the beginning of a very cold, as well as conſiderably thick region: though I eaſily believe him, when he confeſſes, that he felt it much the colder, becauſe he had left off his own cloaths, and put on the ſlight garments uſed there by the diggers. He further informs his reader, that when they had deſcended about 80 fathoms beneath the ſurface of the earth, he began to feel a breath of an almoſt lukewarm air; which warmth increaſed upon him, as he deſcended lower, pleaſing him not a little, becauſe it freed him from the troubleſome ſcents of his former coldneſs. Adding, that the overſeer of the mine, who conducted him, affirmed to him, as alſo the officers of other Hungarian mines unanimouſly told, that in all their mines, at leaſt all the deep ones, after a thick tract of cold earth, there ſucceeds a lower region, that is always hot. And that after they arrived at ſuch a depth, they felt not any more cold, but always heat, how deep ſoever they dig. And to add upon the by, though this learned man lay much weight upon antiperiſtaſis; yet in the next page to thoſe, that contain what I have been juſt now relating, he either very candidly or inconſiderately takes notice, that they informed him, that in their mines, whether more or leſs deep, they obſerved, that at ſome times in the year a ſomewhat intenſer heat was felt; and the two times, that he expreſly names, are thoſe oppoſitely qualified ſeaſons of ſummer and winter.

Having laid down theſe general narratives, I now proceed to conſider the earth's regions in particular; about which the ſum of what I yet have to propound, may be conveniently enough comprized in the four following propoſitions.

CHAP. III.

Proposition I.

"THE firſt region of the earth is very "variable, both as to bounds, and as to "temperature."

The former part of this obſervation will not be difficult to prove, ſince it will be eaſily granted, that the manifeſt operation of the ſun-beams is, *cæteris paribus*, greater, and reaches further in hot climates than in cold ones; in the midſt of ſummer, than in the depth of winter.

The ſecond part of the obſervation may be proved by the ſame arguments as the firſt; to which may be added, as to ſome places, the ſolidity or porouſneſs of the earth; as alſo the nature of ſome ſalts, marchaſites, and other bodies contained in it, which by their natural temperature may diſpoſe the ſoil to coldneſs or heat. As I ſhall have occaſion to ſhew, when I come to ſpeak of the ſecond region.

In the mean time I have this to obſerve further, that in this firſt region, the air is uſually more temperate, as to cold and heat, than that above the ſurface of the earth; and that this region is not wont to be conſiderably deep: both parts of which obſervation are capable of being made good by the ſame reaſons, and therefore I ſhall endeavour to prove them jointly.

That in the uppermoſt region of the earth it ſhould be leſs cold than above the ſurface, ſeems reaſonable to be allowed upon this conſideration; that the ſubterraneal cavities of the earth are ſheltered by the thickneſs of the ſides from the direct action of the ſun-beams, the winds, &c. and is alſo kept from an immediate, or at leaſt from ſo full a contact of the external air, when that is vehemently, either heated or refrigerated.

And firſt as to the heat of the ſun, that that does much leſs powerfully affect ſuch places as are ſheltered from its action by ſolid bodies, may appear by the conſervatories of ice and ſnow, wherein frozen water is kept in that ſtate during all the heat of ſummer, and that oftentimes in cavities, that are at no conſiderable depth beneath the ſuperficies of the earth. Nay I remember, that having had occaſion (for the perfecting of ſome concluſions I was trying) to keep ice many weeks after the froſty weather was gone, and a milder ſeaſon was come in, I was able to do it, contrary to the expectation of ſome curious men, without either digging to a notable depth in the ground, or building any ſubſtantial ſtructure over the cavity. For wanting conveniencies, I contented my ſelf, though it were in a champain place, with a pit ſomewhat broad at the bottom, of about four foot deep or leſs, whoſe mouth was ſheltered only by a little low thatched hovel, that was wide open to the north, and only ſcreened the mouth or vent of the little pit from the direct beams of the ſun. And though I will not deny, that in deep conſervatories of ſnow, the natural coldneſs of the earth, eſpecially in ſome places, may contribute to the effect; yet I remember, that diſcourſing once with a traveller and ſcholar, that was born in hot countries, of a conjecture of mine, that in an arched building, whoſe walls were ſufficiently thick, and whoſe air were carefully kept from all avoidable intercourſe with the external air, one may, without digging ſo much as a man's depth into the ground, make a ſufficient conſervatory for ice in very open and unſheltered places, and even ſuch as *Saliſbury-Plain* itſelf: diſcourſing, as I began to ſay, with this traveller about this conjecture, he told me, that at a place he named to me, in the ſouthern part of *France*, whoſe heat ſeemed to me to exceed that of divers parts of *Italy*, ſome curious perſons, that were reſolved at any rate to have ice in ſummer, though the ſoil were ſuch, that they could not dig four foot without meeting with water, were yet able to make uſe of conſervatories, by covering the brick-building they made over their pits, with clay and ſand, to a very conſiderable thickneſs, and taking care, that the only place, that ſhould permit acceſs to the outward air, ſhould be a ſmall northern door to go in and out at, fitted to ſhut exactly cloſe, and fenced with a little porch, furniſhed with another door. And by this means he affirms theſe gentlemen to reſerve the included ice, not only all the ſummer long, but ſometimes for two or three years together, the heat of that region making many of their winters too mild to recruit them with ice.

To all theſe things I ſhall add, that even where the intercourſe is not quite debarred, but left free enough betwixt the ſubterraneal and the ſuperior air, the operation of the ſunbeams may be very much leſs in a cavity though but ſhallow, beneath the ſurface of the ground than above it. For beſides that trials have informed me, that liquors, that differ in little elſe than in conſiſtence, will not ſo eaſily pervade each other, as a man would ſurmiſe, unleſs ſome external motion haſten their intimate mingling with one another; I remember, that one morning pretty late, having had the curioſity to deſcend into a pit, where they were digging out iron oar; though this cavity had no very narrow orifice, and was dug directly downwards, and exceeded not ten or twelve foot in depth, yet I found not the heat at all troubleſome, whilſt I ſtayed there, though the pit were in an open field, unſhaded by trees, and though the air abroad were much heated at that time of the year, which was in that ſeaſon (or at leaſt very near it) that is wont to be called the dog-days.

CHAP. IV.

AND as we have ſhewn, that the ſubterraneal air, even in the firſt region, is uſually much leſs heated, than the ſuperterreſtrial air; ſo we may alſo eaſily obſerve, that that inferior air is (*cæteris paribus*) wont to be much leſs refrigerated by the grand efficients of intenſe cold, than the ſuperior air.

I will not urge on this occaſion what I have obſerved by a ſurer way, than for ought I know has been before practiced, about the ſmoaking of ſome ſprings in froſty weather; becauſe I do not know, but that thoſe ſprings may have come from, or paſſed a good way through, ſome place very deep beneath the ſurface of the directly incumbent ground, and perhaps from a ſoil peculiarly fitted to warm them; whence the water may have derived a warmth conſiderable enough not to be quite loſt, till it began to ſpring out of the ground, where it needed only not to be quite cold, to appear to ſmoke; the intenſe coldneſs of the air making thoſe exhalations viſible in froſty weather, which would not be ſo in milder: as is evident in a man's breath, which appears like a ſmoke in ſuch weather, though it be not viſible in ſummer.

THAT therefore, which I ſhall propoſe in favour of our obſervation, is firſt taken from the nature of the thing, which may perſuade us, that the ſubterraneal air being though comparatively cool, yet indeed moderately warm in ſummer, ought not to be affected with winter's cold, ſo much as that contiguous to the ſurface of the earth, from whoſe immediate contact it is by a thick arch of earth, if I may ſo call it, defended; and that the cold reigns moſt in the free air and the ſuperficial parts of the terreſtrial globe, may appear by water's beginning to freeze at the top, not at the bottom. To which reaſon from the nature of the thing, I ſhall add only this from experience, that we ſee, that in cellars, that are arched and carefully kept cloſe from the communication of the outward air, beer, and other liquors may be kept from freezing in froſty and ſnowy weather. As I have obſerved in a cellar, that was but ſhallow, but well arched in a winter, that was ſharp to a wonder, and froze ſtronger liquors than beer in another cellar very near it, that differed not much from it in depth, but had not ſo thick and ſolid a roof. And that not only here in *England*, where the cold is leſs violent, but even in *Ruſſia* itſelf, where it is wont to be ſo extreme, it reaches not near ſo deep as one would think, I learned by inquiry purpoſely made of an ingenious phyſician, that lived at *Moſcow*, who anſwered me, that others, and he himſelf, did in that city keep all the winter long, not only their wine, but their beer from freezing in cellars, that were not above twelve or fourteen foot deep, but well covered above, and carefully lined with planks of fir, without any entrance, but a ſmall trap-door (commonly at the top) which was fitted ſo exactly to the orifice it was to cloſe, as to exclude, as much as was poſſible, all communication between the internal and external air, that the latter might not affect the former with its coldneſs.

I have indeed ſuſpected, that in ſome cellars, the comparative warmth we find there may be partly due to ſubterraneal exhalations, that are pent up in them; and perhaps too in ſome meaſure from the ſteams of the fermenting, or fermented liquors lodged in thoſe places. And I was ſomewhat confirmed in this ſuſpicion, by an information my inquiries obtained from the newly mentioned doctor, who told me, upon his own obſervation, that in one of the cellars he made uſe of at *Moſcow*, having occaſion to open the above mentioned trap-door, after the cellar had for a good while been kept very cloſe ſhut, there came out at the vent, that was thereby given, a copious ſteam in the form of ſmoke, which to them, who had their bodies affected with the external air, was very ſenſibly warm, and was almoſt unfit for reſpiration. Which circumſtance increaſed my ſuſpicion, that there might be among theſe ſteams ſome of the nature of thoſe, that have been obſerved to come from fermenting liquors, eſpecially wine, and ſo abound in ſome cellars, as almoſt to ſtifle thoſe, that ventured into thoſe vaults, and to kill ſome of them outright. Which effects the long abode of ſubterraneal ſteams in ſtagnating air, even in many places, where no metalline oars at all, nor other noxious minerals have been found, has enabled that air to produce. Of which divers ſad inſtances have been given within leſs than a mile of this place, upon men's firſt going down into pits or wells, that had not in a long time been opened or made uſe of: but this is here mentioned only upon the by; nor have we any neceſſity to fly to ſubterraneal exhalations, for the comparative warmth, that good cellars in general afford in froſty weather; ſince that phænomenon may be accounted for by the reaſon formerly given, that the cloſeneſs of the cavity, and the thickneſs of the ſides and roof, keep it from being vehemently affected with the cold of the ambient air.

I know it is pretended, that the warmth we ſpeak of, proceeds from an antiperiſtaſis: but not now to engage in a controverſy, that would take up too much time, it may here ſuffice to repreſent, that in our caſe there appears no neceſſity of recurring to it, the phænomenon being ſolvable by the region newly cited, which may be confirmed by this experiment, that in the vaulted cellar above-mentioned, wherein beer was kept from freezing, in an almoſt prodigiouſly ſharp winter, the included air, though ſenſibly warm to thoſe, that came out of the free air, had not ſo intended its native heat, as the aſſertors of antiperiſtaſis would have expected; being colder than the free air commonly is in that place, not only in the heat of ſummer, but in other ſeaſons, when the weather is temperate; as I was aſſured by comparing my own obſervations, made at other times, with the account brought me by a ſkillful perſon, whom I employed into that cellar at late hours, in one or two of the ſharpeſt nights of the forementioned cruel winter, with the ſame excellent ſealed weather-glaſs, that I had long kept ſuſpended within a ſtone's caſt of that place.

CHAP. V.

HAVING ſaid thus much about the earth's uppermoſt region, I now proceed to that, which lies next beneath it; whoſe temperature I cannot ſo conveniently give an account of, in leſs than two propoſitions, whereof the firſt is this;

PROPOSITION II.

“ THE ſecond region of the earth ſeems to “ be for the moſt part cold in compariſon of “ the other two.”

THIS propoſition may be confirmed partly by reaſon, and partly by experience.

AND firſt it ſeems conſonant to reaſon, that ſince the earth is naturally a body, conſiſting of groſs and heavy parts, that are uſually much leſs agitated, than thoſe of our organs of feeling, it ſhould as to ſenſe be cold; and that therefore that quality may be juſtly aſcribed to it, in that region, where by virtue of its ſituation it is kept from being conſiderably affected, either by the heat of the ſuperior air, or by that of the deep parts of the earth: which upper and lower heat are the two agents, that ſeem of all others the moſt likely to put its parts into an unuſual motion, and thereby change its natural temper.

THAT our propoſition is alſo confirmable by experience, may be gathered from the relations ſet down in the former part of this diſcourſe.

AND here it will be proper to take notice of the advertiſement intimated in the cloſe of our above delivered propoſition, that this coldneſs aſcribed to the ſecond region of the earth is to be underſtood comparatively to the other two. For otherwiſe, that even this earth is not, as many naturaliſts would have it, the *ſummum frigidum*, I gather from this, that I could never hear of any ice met with there, at any time of the year, though ſnow or hail may be produced in the middle region at differing, and ſometimes quite oppoſite ſeaſons of the year. Nay, I have not found by the anſwers, that were made me by thoſe, that have deſcended far enough into this region, that they found the cold any where very great, or that in ſome places they have found it at all conſiderable; as we ſhall ſee in the explication of the next propoſition. I know not, whether it will much ſtrengthen what has been ſaid, if I add, that I learned by enquiry of ſuch perſons as I lately mentioned, that at the mouth of deep grooves, in mines, the ſteams, that aſcend, do often feel warm; though the outward air, where the obſervation is made, be affected with the heat of ſummer. But though this probably argue, that if the middle region of the earth, through which theſe ſteams muſt aſcend, were very intenſely cold, they would be ſo refrigerated in their paſſage, as to feel rather cold than hot at their appearing above ground, eſpecially in ſummer: yet I ſhall not lay much weight (for ſome may perhaps be allowed it) upon this argument; becauſe I have not yet tried, how far a warm ſteam may be altered in its paſſage, through a cold conduit: not to mention, that in the earth, the paſſage by being directly upwards may be much the nimblier traverſed.

CHAP. VI.

THE ſecond propoſition relating to the temperature of the ſecond region of the earth may be delivered in theſe terms.

PROPOSITION III.

“ In ſeveral places, which by reaſon of their “ diſtance from the ſurface of the earth, one “ would refer to the middle region of it, the “ temperature of the air is very differing at the “ ſame times of the year.

I choſe to expreſs my ſelf thus, to prevent ſome ambiguities and objections, which I foreſaw, that ſhorter, but leſs clear and full expreſſions, might give occaſion to.

In the proof of our propoſition, both experience and reaſon may diſtinctly be imployed. And to begin with experience,

Whereas in the above recited deſcent into the Hungarian mines, there was obſerved a notably cold region of a conſiderable thickneſs, I have purpoſely procured accounts from divers perſons, that have here in *England* had occaſion, ſome of them, frequently to deſcend into deep pits or grooves of differing minerals, without finding by the narratives they made me, that they took notice of any notably cold part that they paſſed thorough; unleſs I particularly aſked a queſtion about ſuch a thing. But for ought I could gather from their ſpontaneous relations, they felt in ſummer-time a remiſſion of the heat of the external air, as ſoon as ever they began to deſcend; which warmth did not ſo far decreaſe, as to terminate in any notable coldneſs, before they came into a deeper part of the earth, where they are never troubled with that quality. And ſome of theſe relations I had from profeſſed miners, and was curious, that the relations I procured ſhould be of ſubterraneal parts ſeated in very differing parts of *England*, as well as of places not all, or moſt of them having veins of one and the ſame mineral. And I learned by particular inquiry from a practical mathematician, that was often imployed about lead mines, that at ſuch depths, as (according to *Morinus*) the ſecond region of the earth reaches to, he himſelf obſerved it to be ſenſibly warm at all ſeaſons of the year (for about that circumſtance, I was peculiarly ſollicitous to be ſatisfied.)

Nor is it unconſonant to reaſon, that the middle region of the earth, in the ſenſe meant in the propoſition, ſhould not be of the ſame temperature in all places; not only becauſe of the differences, which the climate may produce by reaſon of its being very much hotter, or very much colder in one place than in another: but from the peculiar conſtitution of the ſoil; to the conſideration whereof I ſhall here confine my ſelf.

Now this temperament of the ſoil it ſelf may be diverſified, not only by its greater or leſſer compactneſs (upon which account ſome ſoils are rocky or ſtony and others light and ſpongy) but from the nature of the ſprings or ſubterraneous liquors, that may abound in it, or ſtrain through it into the groove or pit, we ſuppoſe the obſerver to be in; and that eſpecially by the minerals, particularly ſalts, and marchaſites, that grow near the ſides of the well, or are brought thither by the waters.

To illuſtrate this, give me leave to conſider, that nature does not regulate her ſelf under ground by our imaginary diviſions; but, without taking notice of them, produces marchaſites, ſalts, and other minerals, moſt frequently perhaps in what we call the lower region of the earth; but yet ſometimes too in our upper region, and oftentimes in our middlemoſt region. Let us then ſuppoſe, that in ſome places of this laſt named region, there be a mine of that earth, that naturally abounds with embryonated nitre, or with ſome other ſalt, that is apt, eſpecially being diſſolved or moiſtened with water, (a thing very familiarly to be met with in mines;) to ſend out a refrigerating effluvium, or by its contact to cool the air. Let us alſo ſuppoſe, that by the ſides of another well of the ſame depth, there are ſtore of unripe minerals, that are in the proceſs of generation, or rather a great quantity of marchaſitical earth, if I may ſo call it, that is ſuch a ſubſtance, as I have met with in more than one place, copiouſly impregnated, and as it were blended with minerals of a marchaſitical nature; and yet of ſo open and looſe a texture, as not only water would in a few hours, but air alſo would not in very many evidently work upon it. And ſince during the time, that marchaſites are ſlowly diſſolving, it has been obſerved, according to what we have elſewhere delivered*, that many of them will conceive a very conſiderable degree of heat; will it not be very probable, that the temperature of the earth in the place, that abounds with theſe marchaſitical minerals, will be very warm in compariſon of the temperature of the other place, where the ſoil does plentifully produce nitrous, and other refrigerating bodies;

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* The tract here pointed at is a Diſcourſe of Subterraneal fires and heats.

'though both the places be ſuppoſed to be at the ſame diſtance from the ſurface of the earth, and conſequently in the ſame ſubterraneous region.

Upon the like grounds, it may alſo be ſuſpected, that in the ſame places the temperature may not be always the ſame, even upon the account of the ſoil. For I elſewhere ſhew, that ſome ſaline earths, eſpecially nitrous, and ſome minerals, that partake of the nature of marchaſites, admit a kind of maturation, and perhaps other changes, that ſeem to be ſpontaneous; and that ſuch changes happen the more notably in thoſe parts of ſuch bodies, that are expoſed to the air, as thoſe are, that chance to be placed at the ſides of the deep wells we are talking of. Which things being pre-ſuppoſed, it will not be abſurd to conceive, that the mineral, to which either heat or cold is to be referred, may be more copious, ripe, and operative at one time, than at another; or, that at length all the earth capable of being, as it were, aſſimilated by the mineral rudiments harboured in it, may be conſumed, or the mineral it ſelf may arrive at a perfection of maturity, which will make its texture ſo cloſe, as to be unfit to be penetrated, and wrought upon, as before, by the water or other liquor, that occaſioned its incaleſcence.

CHAP. VII.

I OMIT to ſpeak of the tranſient changes, that may be occaſioned in the temperature of the ſecond region of the earth by ſeveral accidents, and eſpecially by the ſubterraneal exhalations, that in ſome places and times copiouſly aſcend out of the lower regions of the earth. Nor ſhall I inſiſt upon any of the other cauſes of a more durable difference of temper in ſome parts of the ſecond region, ſuch as may be the vicinity of ſubteraneal fires in the third region that heat the incumbent ſoil; becauſe I would haſten to the third and laſt part of this diſcourſe; which yet I muſt not do, without premiſing this advertiſement, that I think myſelf obliged to ſpeak the more heſitantly and diffidently about the temperature of ſubterraneal air, becauſe mineraliſts have not had the curioſity to examine it by weather-glaſſes, which would give us much more truſty informations, than our ſenſe of feeling powerfully pre-affected by the cold or heat of the external air. I did indeed ſend fit inſtruments to ſome days journey from this place, to examine the air at the bottom of ſome of our deep mines; but through ſome unlucky caſualties upon the place, the attempt miſcarried. But when I ſhall (God aſſiſting) recover an opportunity, that I have ſince wanted, I hope an accurate ſealed weather-glaſs, joined with a portable baroſcope, will give me better information than mineraliſts have yet done. I ſay a ſealed weather-glaſs, becauſe though common thermoſcopes had been employed by miners, I durſt not rely upon them; being perſwaded by trials purpoſely made, as well as by the reaſon of the thing, of the fallaciouſneſs of ſuch thermoſcopes: for in them the included air is liable to be wrought upon, not only by the heat and coldneſs, but by the weight or preſſure of the external air. So that if a thermoſcope be let down from a very conſiderable height, at the top of which the ſtation of the pendulous liquor be well marked, that liquor will be found to have riſen, when the inſtrument reſts at the bottom, as if the included air were manifeſtly refrigerated; though the temper of the external air may be in both places alike, the cauſe of the pendulous liquor's riſing being indeed, that the aereal pillar incumbent on the ſtagnant liquor is higher and heavier at the bottom, where the inſtrument reſts, than that, which leaned upon it, at its firſt or upper ſtation nearer the top of the atmoſphere. From whence it will be eaſy to conclude, that at the bottom of a deep groove, where the atmoſpherical pillar, that preſſes the ſtagnant water, will be much longer and heavier than at the top, the air may appear by the inſtrument to be colder in places, where it is really much hotter, the increaſed weight of the incumbent air being more forcible to impel up the pendulous liquor, than the endeavour of expanſion procured in the included air by the warmth of the place is to depreſs it.

CHAP. VIII.

THAT, which challenges the third and laſt part of my diſcourſe, is the lowermoſt region of the earth, about whoſe temperature I ſhall comprize, what I have to ſay in the following propoſition.

Proposition IV.

"The third region of the earth has been "obſerved to be conſtantly and ſenſibly warm, "but not uniformly ſo, being in ſome places "conſiderably hot."

I mention, that the recited temperature has been obſerved in the lower region, becauſe I would intimate, that I would have the propoſition underſtood with this limitation, as far as has been yet (that I know of) obſerved. For almoſt all the deep grooves, that mineraliſts have given us accounts of, and wherein men have wrought long enough to take ſufficient notice of the temperature of the air, have been made in ſoils furniſhed with metalline oars, or other minerals, without which men would not be invited to be at ſo great a charge, as that of ſinking ſo very deep pits, and maintaining workmen in them. So that experience has yet but ſlenderly, or at leaſt not ſufficiently informed us of the temperature of thoſe parts of the third region of the earth, that are not furniſhed with ponderous minerals; and conſequently has not informed us of the temperature of the lowermoſt region in general; as will better appear by what I ſhall ere long repreſent.

Having premiſed this advertiſement about our propoſition, we may proceed to the diſtinct proof of the two parts or members it conſiſts of.

And to begin with the firſt, whatever the peripateticks teach of the innate coldneſs of the earth, eſpecially where it is remoteſt from the mixture of the other elements; yet having purpoſely enquired of ſeveral perſons, that viſited and alſo frequented the third region in differing countries, ſoils, and at differing depths under ground, and ſeaſons of the year, I did not perceive, that any of them had ever found it ſenſibly and troubleſomely cold in the third region of the earth. And on this occaſion I remember, I had ſome light ſuſpicion, that at leaſt in ſome caſes, the narrowneſs of the cavities, wherein the diggers were in divers places reduced to work, might make the warmth they felt proceed in great part from the ſteams of their own bodies, and perhaps of the minerals, and from the difficulty of cooling or ventilating the blood in an air clogged with ſteams. And I was the rather induced to think this poſſible, becauſe I had (even in metalline mines, that were but ſhallow and very freely acceſſible to the air) obſerved a ſtrong ſmell of the metal abounding there.

I have likewiſe found by ſeveral trials, that the exhalations, that proceed from the bodies of animals, do ſo vitiate the air they abound in, as to make it much leſs fit for their reſpiration, and to be apt to make them ſick and faint. Wherefore I thought it not altogether unfit to inquire, whether the heat of the ſubterraneal air, in ſuch places as have been newly mentioned, might not be referred to theſe cauſes. But I was anſwered in the negative; eſpecially by an inquiſitive perſon, that had been in the deepeſt and hotteſt mines, that have been viſited by any acquaintances of mine.

This way of accounting for the ſubterraneal warmth being laid aſide, it ſeemed, I confeſs ſomewhat difficult to conceive, how it ſhould be produced yet; two principal cauſes there are, to which I think we may probably refer the temperature of thoſe places, where the air is but moderately warm. To which a third is to be added; when we come to give an account, why ſome places are troubleſomely hot.

And firſt, why the coldneſs of winter ſhould not be felt in the lowermoſt region of the earth may be, that the air there is too remote from the ſuperterreſtrial air, to be much affected with thoſe adventitious cauſes of cold, that make that quality intenſe in the air above ground. But becauſe this reaſon ſhews rather, why it ſhould not be in the earth's lower region much colder in winter than in ſummer, but not why it ſhould be in all ſeaſons warm there; I ſhall add as a conjecture, that the poſitive cauſe of the actual warmth may proceed from thoſe deeper parts of the ſubterraneal region, which lie beneath thoſe places, which men have yet had occaſion and ability to dig. For it ſeems probable to me, that in theſe yet unpenetrated bowels of the earth, there are great ſtore-houſes of either actual fires, or places conſiderably hot, or, (in ſome regions) of both; from which reconditories (if I may ſo call them) or magazines of hypogeal heat, that quality is communicated, eſpecially by ſubterraneal channels, clefts, fibres, or other conveyances, to the leſs deep parts of the earth, either by a propagation of heat through the ſubſtance of the interpoſed part of the ſoil, (as when the upper part of an oven is remiſly heated by the ſame agents, that produce an intenſe heat in the cavity,) or by a more eaſy diffuſion of the fire or heat through the above-mentioned conveyances, as may be exemplified by the pipes, that convey heat in ſome chemical ſtructures:) or elſe, (which is perhaps the moſt uſual way,) by ſending upwards hot mineral exhalations and ſteams, which by reaſon of the comparatively heavy minerals they conſiſt of, and by reaſon of their being leſs diſperſed nearer the places whence they proceed, are uſually more plentiful in the deeper parts of the earth, and ſomewhat affect them with the quality, that they brought from the work-houſes where they were formed, and that they retain for ſome time after.

CHAP. IX.

THAT manifeſt ſteams oftentimes are found in grooves, eſpecially in deep ones, is evident by the damps, that infeſt moſt of them, and that in diſtant regions, as in ſeveral provinces of *Germany*, *Bohemia*, *Hungary*, &c. as alſo in ſeveral parts of *England*, in grooves, ſome of which I have received relations from the mine-men themſelves. By which it appears, that ſeveral of theſe exhalations aſcending from the entrails of the earth are ſulphureous and bituminous in ſmell, and in ſome grooves (one whereof I elſewhere mention my ſelf to have viſited) theſe ſteams are apt, actually to take fire.

The warmth of many ſubterraneal exhalations, I think, may be made further probable by ſome other obſervations. For though theſe newly mentioned are not to be rejected, and may be employed for want of better; yet I have ſeveral times queſtioned, whether I ought to acquieſce in them alone. For I do not think the eaſy inflammableneſs of bodies to be always a ſure proof of the actual ſenſible warmth of the minute parts it conſiſts of, or may be reduced into. For though ſalt-petre be very inflammable, yet being by a ſolution in fair water reduced to inviſible corpuſcles, it highly refrigerates that liquor. Nor have I obſerved its fumes, (when far from the fire,) to have any heat ſenſible to our touch. And the like may be ſaid of the exhalations of highly rectified ſpirit of wine; which yet we know is itſelf totally inflammable. Nay I know not, whether (for a reaſon elſewhere declared) copious exhalations may not aſcend from the lower parts of the earth, and yet be rather cold than hot. For, in another paper, I mention a way by which I made a mixture, that plentifully enough emitted ſteams, of whoſe being rather of a cold, than a hot nature, there was this probability, that the mixture whence they aſcended, even whilſt its component ingredients were briſkly acting upon one another, was not only ſenſibly, but conſiderably, cold.

One main thing therefore, that induces me to aſſent to the opinion, whereto the former inſtances do but incline me, is, that having purpoſely inquired of an obſerving man, that frequented deep mines, (wherein he had a conſiderable ſhare,) he anſwered me, that he plainly obſerved the fumes, that came out of the mouths of the deep pits, to be actually and ſenſibly warm, and, that in a warm ſeaſon of the year. And *Morinus* (above cited) ſpeaking of the deep Hungarian mines, makes it the firſt epithet of the copious exhalation, that aſcended from the bottom, that it was hot. And a few pages after he ſays, that at the mouth of the well, the aſcending fumes were ſenſibly hot in ſummer itſelf. And the ſame arguments, that I have elſewhere given to ſhew, that there are very hot places, and, as it were æſtuary in the bowels of the earth, may ſerve to make it probable, that the ſteams aſcending thence may be actually warm.

That alſo in many places of the earth, where no grooves are dug, and no viſible exhalations are taken notice of, they may yet pervade the ſoil, and exerciſe ſome operations of warmth, may be probable by this, that the experienced *Agricola* himſelf reckons it among the ſigns of a latent mineral vein, that the hoar-froſt does not lie upon that tract of the ſurface of the earth, under which a vein (though perhaps very deep) runs. The like directions I have known given by the ſkilful in *England*, for the diſcovery of places, that contain coal-mines. And I remember a near relation of mine ſhewed me a great ſcope of land of his, which (though in an outward appearance likely to be as cold as any place thereabouts,) he affirmed, would not ſuffer ſnow to lie upon it above a day or two in the midſt of winter.

The probability of which relation was confirmed to me by the anſwer I received from a very ingenious gentleman, who lives among mines, and is not a little concerned in ſome of them. For having inquired of him, what he had obſerved about the lying, or not lying of the ſnow on the mineral ſoils near the place of his reſidence; he replied, that in ſome of them, he did not take notice of any peculiar indiſpoſition to let the ice and ſnow continue on them: which I conceive may proceed, either from the want of ſuch minerals in the ſubjacent parts, as were then in the ſtate of incaleſcence; or elſe from this, that (according to what we have elſewhere obſerved about the ſnow on *Ætna*) the direct aſcenſion of the hot ſteams was hindered by ſome layers of rocks or ſtone, through which the ſteams could not penetrate, or could do it but ſo ſlowly, as to looſe their actual warmth by the way. But this gentleman added, that in other places, near that of his abode, and ſuch as he knew to have mineral veins beneath them, he obſerved, that the ſnow, (nor the ice) would ſcarce continue at all upon the ſurface of the ground, even in an extraordinary cold winter.

It will be a conſiderable inſtance to our purpoſe, if it be indeed true, which ſome learned men have written, that near the gold-mines in *Hungary*, the leaves of the trees (eſpecially thoſe, that reſpect the ground) are oftentimes found enobled with a golden colour from the metalline exhalations of the gold mines; which, one would think, muſt by reaſon of their ponderouſneſs need a conſiderable heat to elevate them, eſpecially into the open air. But though doubting of this relation, as not made by mineraliſts or accurate obſervers, I inquired about it of a perſon, whoſe curioſity carried him purpoſely to viſit thoſe mines, I was anſwered, that he could not be a witneſs to the truth of the obſervation; yet he told me an obſervation (which I elſewhere mention) that doth not diſcountenance that tradition.

If it be objected, that what has hitherto been ſaid about latent fires and heats in the bowels of the earth, will give an account of the warmth only of thoſe places, that are within reach of the action of ſuch magazines of heat, which probably may be wanting in many places of the earth; I ſhall readily confeſs, that as I firſt made this objection to myſelf, ſo I do not yet diſcern it to be unreaſonable; and, that for ought I know, if men had occaſion to dig as deep, and be as far converſant in many other low places of the earth, where there are no ſigns of minerals, as they have done, where the hopes of actual diſcovery of veins of metals, and other minerals worth working, have invited then, divers places in the third region of the earth would be met with, that would be deſtitute of the warmth, that has hitherto been generally found in places of the ſame region, that either abound with minerals themſelves, or are near ſome of the deep and latent æſtuaries above-mentioned.

And as for thoſe parts of the third region of the earth, which men feel not only warm, but troubleſomely hot, that incommodious degree of heat ſeems not, (at leaſt in ſome places) to be derivable from the two above-mentioned cauſes; which muſt, (to produce ſo conſiderable an effect,) be aſſiſted by a third cauſe more potent than themſelves: which ſeems to be the incaleſcence there is produced in many mines, and other places, by the mutual action of the component parts promoted by water of immature and more looſely contexed minerals, eſpecially ſuch as are of a marchaſitical nature. That ſuch an incaleſcence may by ſuch a way be produced in the bowels of the earth, I have elſewhere ſhewn (in my diſcourſe of ſubterraneal fires and heats) by the examples of ſuch incaleſcences producible in mineral bodies here above ground. That marchaſites, which for the moſt part abound in vitriol, are bodies very fit to procure this ſubterraneal heat, may be confirmed not only by the ſulphureous and ſaline parts they abound with, and by this, that many of them may be wrought on, as we have tried, both by ſimple water, and even by moiſt air, which argues the reſolubleneſs of their conſtitution; but alſo by this, that having purpoſely inquired of a gentleman, that went out of curioſity to viſit one of the deeper Hungarian mines, he confirmed to me what I had otherwiſe been informed of, by anſwering me, that in the lower parts of the mine, he had gathered vitriol, that appeared above ground to be of a golden nature; and, that in a cave, that is on one ſide of the groove, in the deep gold mine near *Cremnitzo*, the corroſive ſmell is ſo ſtrong and noxious, that men have not dared to dig out the native gold it richly with, being deterred by the ill fate of divers, that ventured to work in it. Adding, that though he paſſed by it in great haſte, yet he could not avoid the being offended by the noiſome exhalations. And on this occaſion, it will not be, I preſume, diſliked, if I illuſtrate what I was ſaying of immature minerals, by ſubjoining, that having aſked this chemiſt, whether the vitriol he found very deep under ground were all ſolid, or ſome of it ſoft? he affirmed, that as he gathered it, he found ſome of it ſoft. And to ſatisfy my curioſity, to know whether it continued that yeilding conſiſtence? he farther told me, that it was ſoft in the deeper part of the mine; but when he had brought it into the ſuperterreſtial air, it hardened there, and appeared to have nine divers golden ſtreaks in it.

CHAP. X.

ONE thing there is, which muſt not be here omitted, though it will probably be great news to thoſe, that philoſophize only in their ſtudies, and have not received information from any that viſited the deeper parts of the earth. The phænomenon is this, that the diggers in mines, having found by unwelcome experience, that in deep grooves, the air (unleſs ventilated and renewed) does in a ſhort time become unfit for reſpiration, have been put upon this expedient, to ſink, at ſome convenient diſtance from the groove where the miners work, another pit, by ſome called a vent pint, that uſually tends directly downwards (though ſometimes it make angles) to which our Engliſh mine-men do in ſeveral parts of this kingdom give differing names, whereof the moſt ſignificant ſeems to be that given it in the lead mines of *Derbyſhire*, where they call it an air-ſhaft, and are wont to make it 40, 50, and ſometimes 80 or 100 paces off; and, as one of the chief and ſkilful miners there informed me, as deep as the groove or well; (though I find, that the beſt German and ſome Engliſh miners think a leſs depth will often ſuffice) from this air-ſhaft to the groove the men work in there paſſes a channel, or, if I may ſo call it, ventiduct, to convey the air from the former to the latter; which is that, that *Agricola* ſometimes (for he employs not the term always in the ſame ſenſe) denotes by his cuniculus; and which though differingly named by our miners in ſeveral parts of *England*, is in the above-mentioned lead mines called a drift, becauſe the air does uſually in the form of wind drive through it; and thereby enables the workmen to breathe freely and conveniently enough at the very bottom of the well. On this occaſion I remember, that a very obſerving man, who much frequented theſe mines, told me, that at the depth of no leſs than about 200 yards, he found, that by the help of the air-ſhaft, the air was not only very commodious for reſpiration, but temperate as to heat and cold. And when I further aſked, what time of the year it then was? he told me it was about the latter end of Auguſt, and the beginning of September.

Lib. V. & VI. de re metall.

NOW that, which ſeems to me to deſerve a farther and accurate obſervation about the motion and temperature of the air in theſe artificial under-ground cavities, is a relation of *Agricola*'s, which (though he be the moſt claſſick author we have about mines) has not, that I know of, been taken notice of, in him. For this experienced writer, though in his treatiſe * *de ortu & cauſis ſubterraneorum*, he only ſays indefinitely, that by means of the cuniculus or drift, which connects the air-ſhaft and the well, that air, which comes in at one of thoſe two, paſſes out at the other; yet in his fifth book, *de re metallicâ*, he gives a more particular and odd account of the courſe of the air in theſe not over-clear terms, *aer autem exterior ſe ſuâ ſponte fundit in cava terræ, atque cum per ea penetrare poteſt, rurſus evolat foras. Sed diverſâ ratione hoc fieri ſolet; etenim vernis & æſtivis diebus in altiorem puteum influit, & per cuniculum vel foſſam latentem permeat, ac ex humiliori effluit; ſimiliter iiſdem diebus in altiorem cuniculum infunditur, & interjecto puteo defluit in humiliorem cuniculum, atque ex eo emanat. Autumnali verò & hyberno tempore contra in cuniculum vel puteum humiliorem intrat, & ex altiori exit: verum ea fluxionum aeris mutatio in temperatis regionibus fit in initio veris, & in fine autumni: in frigidis autem, in fine veris & in initio autumni.* To which he adds, † that which is more remarkable, that the air in both the mentioned times, before its wonted courſe come to be durably ſettled, uſes to be for the ſpace of a fortnight liable to frequent changes, ſometimes flowing into the upper or higher groove or drift, and ſometimes into the lower, and paſſing out at the other. If this obſervation conſtantly hold, though but in ſome deep mines, it may hint ſome odd inquiries about conſiderable and periodical changes in the ſubterraneal parts of the earth, or in the air, or in both; which, though they have not yet been conſidered, deſerve to be ſo. I have endeavoured to learn, whether any ſuch thing has been obſerved in ſome deep lead mines, whence I have procured divers informations about other particulars. But a very obſerving perſon, that had the chief hand in contriving the ſubterraneal ſtructures there, aſſured me, that both winter and ſummer, the current air, went conſtantly the ſame way; the air entring in at the mouth of the air-ſhaft, and coming out at the perpendicular groove, which takes its denomination from a cave, (or *caſa putealis*) uſually built over the orifice of it, to ſhelter the workmen from rain, and other inconveniences.

AND ſince the writing of this, I found in *Morinus* (his relation already mentioned) a paſſage, that may ſomewhat illuſtrate the darkly expreſſed obſervation of *Agricola*. For the lately mentioned author writes, that in the deep Hungarian mines he viſited, the outward air paſſed, firſt, through the boroughs, and ſo through by-ways, if I may ſo call them, that tended not directly downwards, reached at length to the bottom of the well, or perpendicular groove, whence, together with the ſteams proceeding from the mine, it aſcended ſtrait upwards. But *Morinus* taking no notice at all of *Agricola*'s obſervation about the differing courſe of the ſubterraneal air at differing ſeaſons of the year, though, as I find by what he writes elſewhere, it was ſummer when he viſited the mines, and ſo what he reports, agrees well with one part of what *Agricola* ſeems to ſay; yet, as to the other, and principal part of his obſervation, he ſays not any thing. And the ſenſible heat he aſcribes to the ſteams aſcending out of the perpendicular well, leaves it ſomewhat dubious, what intereſt the rarefaction of the air by the ſubterraneous heat may have in the phænomena we have been diſcourſing of.

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* Idcirco ſcrobes, putei cuniculi effoſſi complentur exteriore aere. Atque ipſum in eos influere imprimis hyemali tempore evidens eſt in duobus puteis, ad quorum utrumque ex modico intervallo cuniculus aliquis pertinet. Nam aer in unum continuo influit, rectàque per cuniculum permeat & tranſit ad alterum; atque ex eo rurſus evolat foras.

† Sed aer utroque tempore, anteaquam curſum ſuum illum conſuetum conſtanter teneat plerumque quatuor decem dierum ſpatio crebas habet mutationes, modò in altiorem puteum vel cuniculum influens, modo in humiliorem.

But to return to what I was ſaying before I had occaſion to mention *Morinus*. Which perhaps it will not be impertinent to add, that I learned by inquiry, that the air-ſhafts and the wells were in theſe mines much of a depth. But I hope before long to have accounts of what happens in other mines, in other parts of *England*, as to the courſe of the ſubterraneal air, eſpecially when its iſſuing out of the well or the air-ſhaft depend not on the changes of the winds, that blow above ground: and I wiſh the curious would employ the like endeavours in other countries.

For indeed, what I have hitherto diſcourſed in this treatiſe, is accommodated but to the ſcant information I have hitherto received; and therefore ought to be rectified, or confirmed, by farther informations, if they can be procured.

In the mean time, I think, I may probably enough gather from the paſſed diſcourſe, that though in ſome mines, three ſubterraneal regions, and their diſtinguiſhing attributes, may be not inconveniently aſſigned; yet generally ſpeaking of the whole body of the terreſtial globe, as far as we know it, both the bounds and the temperature of the regions of the earth, as well as thoſe of the air, are various and uncertain enough.

And much leſs have we any certain knowledge of the temperature of the more inward, and, if I may ſo ſpeak, the more central parts of the earth; in which, whether there be not a continued ſolidity, or great tracts of fluid matter, and whether or no differing regions are to be diſtinguiſhed, and what their number, order, thickneſs, and qualifications may be, we are as yet ignorant, and ſhall, I fear, long continue ſo; for it is to be noted (with which obſervation I ſhall conclude) that what has been hitherto diſcourſed belongs only to the temper of thoſe ſubterraneal parts, to which men have been enabled to reach by digging. It is true indeed, that ſome mines, eſpecially in *Germany* and *Hungary*, are of a ſtupendous depth, in compariſon of the generality of ours, and of the more obvious cavities of the earth; yet I find it boaſted in a diſcourſe, written purpoſely of the various mines in the world, that the rich mine at *Sueberg* is 400 yards deep: and they are ſcarce believed, that relate one Hungarian mine, which they viſited to be 400 fathom; which, though double the depth of the former, reaches not to half a mile. But the deepeſt of all the mines, that I have as yet read or heard of from any credible relator, is that, which the experienced *Agricola*, in the tract he calls *Bermannus*, cap. 12. mentions to be at *Cotteberg*. But this itſelf, though it reach to above 500 fathom, that is, 3000 foot, yet this prodigious depth does not much exceed half a mile, and falls ſhort of three quarters; [*] and how ſmall a part is that of the whole depth of the terreſtial globe? whoſe ſemidiameter, if we admit the recent account of the learned *Gaſſendus*, is reckoned at 4177 Italian miles; in compariſon of which, as I was ſaying, how ſmall a thing is a depth, that falls very ſhort of a ſingle mile?

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[*] Licet variæ de ambitu terræ opiniones ſint, nobis tamen propemodum conſtet, eſſe ipſam milliarium Italicorum 26255, quod in maximo ad terræ ſuperficiem circulo reſpondeant uni gradui milliaria proximè 73. &c. Gaſſend. Inſtit. Aſtronom. lib. 2. cap. 13.

OF THE TEMPERATURE OF THE SUBMARINE REGIONS,

As to HEAT and COLD.

CHAP. I.

THOUGH the Ariſtotelians, who believe water and air to be reciprocally tranſmutable, do thereby fancy an affinity between them, that I am not yet convinced of; yet I readily allow of ſo much affinity betwixt thoſe two fluid bodies, as invites me (after having treated of the temperature of the aerial regions) to ſay ſomething of that of the ſubmarine regions: which name of ſubmarine, though I know it may ſeem improper, I therefore ſcruple not to make uſe of, becauſe even among the generality of learned men, uſe has authorized the name of ſubterraneous places. For as theſe are not by this name, and indeed cannot in reaſon be ſuppoſed to be beneath the whole body of the earth, but only the ſuperficial parts of it; ſo by the appellation of ſubmarine regions it is not to be ſuppoſed, that the places ſo called are below the bottom of the ſea, but only below the ſurface of it.

BUT to come from words to things, I preſume it will not be expected, that I, that never pretended to be a diver, ſhould give of the regions, I am to treat of, an account built on my own obſervations; and I hope it may gratify a reaſonable curioſity about a ſubject, of which claſſick authors are ſo very ſilent, and about which philoſophers ſeem not ſo much as to have attempted any experiments (for want of opportunities and means to make them.) I offer the beſt information I could ſupply myſelf with, by purpoſely converſing with perſons, that have dived, ſome without, and ſome by the help of engines. To which I have added ſome reports, that I judge fit to be allowed, made me by perſons, that had converſed with the divers upon thoſe African and Indian-coaſts, where the moſt famous and expert are thought to be found.

AND I the rather report the anſwers and relations my inquiries procured, becauſe the informations they give us concern a ſubject conſiderable as well as vaſt, about which nevertheleſs I among many others am not in a condition to ſatisfy at all my curioſity by trials of my own making; and becauſe alſo, what I ſhall ſay will probably ſpoil the credit of the vulgar error, that in all deep water, of which the ſea is the chiefeſt, the lowermoſt are ſtill the warmeſt parts, unleſs in caſe that in ſome very hot climates, or ſeaſons, the ſuperficial ones happen to be a little warmed by the extraordinary or violent heat of the ſun.

CHAP. II.

THOUGH the air and the earth have been diſcriminated as to temperature, into three regions; yet the informations I have hitherto met with, invite me to aſſign to the ſea any more than two. The former of which may be ſuppoſed to reach from the ſuperficies of it, as far downwards, as the manifeſt operation of the variouſly reflected and refracted beams of the ſun, or other cauſes of warmth penetrate; from which to the bottom of the ſea, the other region may be ſuppoſed to extend.

ACCORDING to this diviſion, the limits of this upper region will not be always conſtant; for in the torrid zone, and other hotter climates, it will, *cæteris paribus*, be greater than in the frigid zone or in the temperate zones; and ſo it will be in ſummer than in winter; and in hot weather than in cold; ſuppoſing in theſe caſes the heat to come from the ſun and air, and not, as ſometimes it may do, from the ſubterraneal exhalations.

THE ſame cauſes are likewiſe proper, as it is manifeſt, to alter the temperature, as well as the bounds of this region; but this temperature may alſo be changed in ſome few places, by at leaſt two other cauſes; the one is the differing conſtitution of the ſoil, that compoſes the ſhore, which may affect the neighbouring water, if it do extraordinarily abound with nitre, looſely contexed marchaſites, or other ſubſtances capable conſiderably to encreaſe or leſſen the coldneſs of the water. Another, though unfrequent cauſe, may be the figure and ſituation of the leſs deep parts of the ſhore, which may in ſome ſort reverberate the heat, that proceeds from the ſun; and upon ſuch an account may either add to the warmth, or allay the coldneſs, that would elſe be found in the neighbouring water. For whatever the ſchools are wont to teach about the intereſt of the attrition of air in the heat produced by the ſun beams, I have elſewhere ſhewn by experiments, that thoſe beams may conſiderably operate upon bodies placed quite under water.

BESIDES theſe two caſes, that may occaſion exceptions to the general obſervation; I intimated by the words, *at leaſt*, that there might be others. Becauſe, to mention now but one example, though it ſeem probable from what I have elſewhere delivered concerning the ſubterraneal fires and heats, that may in ſome places be met with, even beneath the bottom of the ſea, that the phænomenon I am going to recite may be reduced to the cauſes newly intimated; yet I am not abſolutely certain, but that in this caſe, where to ſome others may perhaps be found reſembling, ſome other cauſe than thoſe hitherto mentioned may produce or concur to the effect. The relation here meant is afforded us by the following paſſage, taken out of the voyage of Monſieur *de Monts*, into *New-France*, (whereof he went to be governor) where the relator thus recites his obſervation: About the eighteenth day of june we found the ſea-water during three days ſpace very warm, and by the ſame warmth our wine alſo was warm in the bottom of our ſhip; yet the air was no hotter than before. And the 21ſt of the ſaid month, quite contrary, we were two or three days ſo much compaſſed with miſts and cold, that we thought ourſelves to be in the month of january, and the water of the ſea was extreme cold; which continued with us, until we came upon the bank by reaſon of the ſaid miſts, which outwardly did procure this cold unto us. This effect he attributes to a kind of antiperiſtaſis in the following part of his narrative; which I ſhall not now either tranſcribe or examine.

CHAP. III.

AND thus much being briefly noted touching the upper region of the ſea, and the requiſite cautions (that may perhaps extend further than it) being premiſed; it remains, that I take notice of the temperature of the lower region, which, in one word, is cold; unleſs in ſome few places to be preſently mentioned. For water being in it's natural or moſt ordinary ſtate a liquor, whoſe parts are more ſlowly agitated than thoſe of men's organs of feeling, muſt be upon that account cold as to ſenſe; and conſequently it need not be ſtrange, that thoſe parts of the ſea, which are too remote to be ſenſibly agitated by the ſun-beams, or wrought upon by the warmth, which the air and upper parts of the earth may from other cauſes receive, ſhould be felt cold by thoſe that deſcend into it; unleſs in thoſe few places, where the coldneſs may be either expelled or allayed by hot ſprings, or ſubterreſtial exhalations, flowing or aſcending from the ſubjacent earth, or the lower parts of the ſhore, into the incumbent or adjacent parts of the water.

To juſtify my aſcribing of this coldneſs to the ſecond, or lower region of the ſea, I ſhall now ſubjoin ſome relations I procured from perſons, that had occaſion to go down into it, or otherwiſe take notice of its temperature in very differing regions of the world, and at very unequal depths.

AND firſt as to the temperature of the lower region in the northern ſea, I had the opportunity to converſe often, and ſometimes to oblige a man bold and curious enough, who for ſome years got the beſt part of his ſubſiſtence by deſcending to the bottom of the ſea in an engine, whoſe ſtructure I elſewhere deſcribe, to ſeek for, and recover goods loſt in ſhip-wrecked veſſels. This perſon I diligently examined about divers ſubmarine phænomena, about which his anſwers may be elſewhere met with. And as to the temperature of the lower parts of the ſea (the knowledge of which is that alone, that concerns us in this place) he ſeveral times complained to me of the coldneſs of the deep water, which kept him from being able to ſtay in it ſo long as he might have been put into a condition of doing by the goodneſs of his engine; for I remember, that he related to me, that he ſtaid once betwixt an hour or two, at a depth, that was no greater than 14 foot and a half upon the coaſt of *Sweden*, in a place, that was near the ſhore; and I afterwards learned, that he ſtaid much longer in a deeper place; (uſe having probably made the cold more ſupportable to him.) He told me then, that about two years before, he was engaged by a good reward to go down with his engine to the bottom of the ſea to fetch up ſome goods of value out of a ſhip, that had been caſt away there within about a miles diſtance from a very little iſland, and, if I miſtake not, about ſix miles from the ſhore. He further anſwered me, that though he felt it not at all cold on the ſurface of the water, (his attempt being made in june) yet about the depth of the ſhip, it was ſo very cold, that he felt it not ſo cold in *England's* winter and froſty weather. And he told me, that an exceſſive cold was there felt, not only by him, but by very ſturdy men, who invited by his example would needs alſo go down themſelves to participate and promote the hoped for diſcovery. He told me alſo, that the upper water did but cool and refreſh him; but the deeper he went, the colder he felt it, which is the more conſiderable, becauſe he had ſome times occaſion to ſtay at 10 fathoms or even 80 foot under water. And I ſince found, that he informed divers virtuoſi, that purpoſely conſulted him, that he found the coldneſs of the water encreaſe with its depth; and gave that for the reaſon, why he could not ſtay ſo many hours as otherwiſe he might, at the bottom of the ſea. Adding, that before his engine was well fitted, he was once ſo covered over with it, that he was forced to touch the ground with his hands and feet, and the neighbouring parts, to which he found a coldneſs communicated by the fundus he leaned upon; though the cloſeneſs of his diſordered engine made the other, and (whilſt he was in that poſture) upper parts of his body, of a very differing temper.

AN inquiſitive perſon of my acquaintance, that made a long ſtay in the *Northern America* (at about two or three and forty degrees of Latitude) and diverted himſelf often with ſwimming under water, anſwered me, that though he ſcarce remembered himſelf to have dived above two fathoms beneath the ſurface of the ſea; yet even at that ſmall depth, he obſerved the water to encreaſe in coldneſs, the lower he deſcended into it. Which argues, that though the ſun-beams do often penetrate plentifully enough to carry light to a great depth under water, yet they do not always carry with them a ſenſible heat; and that, at leaſt, in ſome places, the upper region of the ſea reaches but a little way.

THE coldneſs of the climate in theſe weſtern parts of *Europe*, and the want of conſiderable inducements to invite men to dive often to any great depth into our ſeas, has kept me from being able to procure many obſervations about the temperature of their lower region; but upon the hotter coaſts of *Africk*, and the *Eaſt-Indies*, the frequent invitations men have to dive for coral, pearls, and other ſubmarine productions, have made it poſſible for me to get more numerous obſervations; ſome of which I ſhall now annex.

CHAP. IV.

MEETING with a perſon of quality, who had been preſent at the fiſhing of coral upon the ſhoar of *Africa*, and who was himſelf practiſed in diving, I inquired of him, whether he found the ſea upon the African coaſt to be much colder at a good depth, than nearer the ſurface; whereto he anſwered me, that though he had ſeldom dived above three or four fathoms deep, yet, at that depth, he found it ſo much colder than nearer the top of the water, that he could not well endure the coldneſs of it.

AND when I farther aſked him, whether, when he was let down to the bottom of the ſea, in a great diving bell (as he told me he had been) he felt it very cold, though the water could not come immediately to touch him; he replied, that when the bell came firſt to the ground, he found the air in it very cold, though after he had ſtaid a-while there, his breath and the ſteams of his body made him very hot.

THAT alſo at a greater depth in thoſe hotter climates, the ſea-water is ſenſibly cold, may be thus made probable: inquiring of a famous ſea-commander, who had been upon the African coaſt, to what depth he was wont to ſink his bottles to preſerve his wine any thing cool in that exceſſive hot climate, he anſwered me, that in the day time he kept it in a tolerable temper ſo as to be drinkable, by keeping it in the bottom of the ſhip, and in ſand; but in the morning he had it cool enough by ſinking his bottles over night into the ſea, and letting them hang all night at 20 or 30 fathom deep under water.

INQUIRING alſo of an intelligent gentleman, that was imployed to the river of *Gambra*, and ſailed up 700 miles in it, in a ſmall frigate, whether he had obſerved, that in the ſea, even of thoſe hot climates, wine may be preſerved cool; he told me, that it might, and, that by the means I hinted to him, which was to let down, when the ſhip came to an anchor in the evening, ſeveral bottles full of wine (they uſed that of *Madera*) exactly ſtopped to ten, twelve, or fourteen fathoms deep; whence being the next morning drawn up, they found the wine cool and freſh (as if the veſſels had been in theſe parts drawn up out of a well) provided it were preſently drank, for if that circumſtance were omitted, the heat of the air on the upper part of the water would quickly warm the liquor.

I remember too, that having met with a man of letters, that ſailed to the *Eaſt-Indies* in a Portugal-caraet, I learned by inquiry of him, that it was the practice in that great veſſel for the captain and other perſons of note, whilſt they paſs through the torrid zone, to keep their drink, whether wine or water, cool, by letting it down in bottles to the depth of 80, 90, and ſometimes an hundred fathom or better, and letting it ſtay there a competent time; after which, he told me, he found it to be exceeding cool and refreſhing.

LASTLY, to ſatiſfy myſelf as far as I could, to how great a depth the coldneſs of the ſea reached; meeting an obſerving traveller, whoſe affairs or curioſity had carried him to divers parts, both *Eaſt* and *Weſt-Indies*, I inquired of him, whether he had taken notice of any extraordinary deep ſoundings in the vaſter ſeas, to which being anſwered, that ſome years ago, ſailing to the *Eaſt-Indies*, in a very great ſhip, over a place on the other ſide the line, that was ſuſpected to be very deep, they had the curioſity to let down 400 fathom of line, and found they needed no leſs. Whereupon I inquired of him, whether he had taken notice of the temperature of the ſounding lead as ſoon as it was drawn up: to which he told me, that he, and ſome others did; and that the lead, which was of the weight of about 30, or 35 lb. had received ſo intenſe a degree of coldneſs, as was very remarkable; inſomuch, that he thought, that if it had been a maſs of ice, it could not have more vehemently refrigerated his hands: and when I aſked in what climate this obſervation was made, he told me, it was in the antarctick hemiſphere, but at a great diſtance from the line. As indeed, I concluded by ſome circumſtances he mentioned to me, that it was about the 35th degree of ſouthern latitude.

CHAP. V.

THESE are the chief relations I have hitherto been able to procure about the temperature of the ſea; which, if they be ſo confirmed by others, as that we may conclude they will generally hold, it will not be irrational to conceive, that in reference to temperature, thoſe two fluids, air and water, may have this in common, that where their ſurfaces are contiguous, and in the neighbouring parts, they happen to be ſometimes cold, ſometimes hot, as the particles they conſiſt of chance to be more or leſs agitated by the variouſly reflected ſun-beams, or more or leſs affected by other cauſes of heat. But that part of the air, which they call the ſecond, and is ſuperior to the firſt, as alſo the lower region of the ſea, being more remote from the operation of thoſe cauſes, do retain their natural, or more undiſturbed temperature, which, as to us men, is a conſiderable degree of coldneſs, the agitation of their ſmall parts being uſually in thoſe regions much inferior to that of the ſpirits, blood, and other parts of our organs of feeling. So that the regions of the water and air ſeem to anſwer one another, but in an inverted order of ſituation; and the analogy might perhaps be carried further, if I had time and opportunity to do it in this place. And here I ſhall not diſſemble, that I was ſomewhat perplexed by meeting with a traveller, that had viſited the Eaſt-indian coaſt, near the famous Cape of *Comory*: for aſking him ſome queſtions touching the neighbouring ſea, I gathered from his diſcourſe, that he concluded from that of ſome divers, that the ſea near *Ceylon* was warmer at the bottom than at the top. And when I thereupon aſked him, whether this happened not in their winter, he replied, that it was indeed winter, though not with us, yet with them. It occurred indeed to my thoughts on this occaſion, that perhaps in a part of the torrid zone ſo near the line as about 80 degrees, if the ſea were not of a conſiderable depth, the heat of the two not far diſtant ſhores of *Coromandel* and *Ceylon* might have no ſmall influence upon the temperature of the water. I conſidered alſo, which did not a little weigh with me, that in divers parts of the *Eaſt-Indies*, and even in a region bordering upon *Coromandel*, where an ingenious acquaintance of mine lived ſome years, it has been obſerved, that winter and ſummer are not ſo much diſcriminated by cold weather and hot, as by very rainy weather and very dry. Nay, in ſome places the ſultry heat of the climate is more complained of, in what they call their winter than their ſummer. So that there will be no neceſſity to recur to an antiperiſtaſis occaſioned by the coldneſs of the winter. I thought too, that it may perhaps be without abſurdity ſuſpected, that as the bottom of the ſea in this place had a peculiar conſtitution, that fitted it more than others for the copious production of pearls; ſo there might be ſome peculiarity in the nature of the ſubjacent ſoil, or there may be ſome ſubterraneal fire or heat beneath it, which may occaſion an unuſual warmth in that part of the ſea, by which cheriſhing warmth, perhaps, ſuch abundance of ſhell fiſhes teeming with pearls may be invited to ſettle there, rather than in any of the neighbouring places. But with all theſe conjectures I ſhould not have been ſo well ſatiſfied, as with the anſwer I afterwards obtained by a gentleman, whoſe curioſity had carried him to be an aſſiduous ſpectator of the famous pearl-fiſhing, near the iſland of *Manar*, between that and the coaſt of *Coromandel*, which reaches near, if not fully to the Cape of *Comory*. For this perſon having had much converſation with the divers for pearls, not only learned from them, that they found the water very ſenſibly cold at the bottom, which in ſome places he eſtimated to be 80 or 100 fathom deep; but obſerved divers of them at their return to the boats, to be ready to ſhake with cold, and haſten to the fires, that were kept ready for them in little cabbins upon the ſhore: which relation being accompanied with divers circumſtances of credibility, and arguing, the perſon that made it to have been acquainted with the report above-mentioned, and had met with ſome, that had dived in the place whereto it had relation, made me conclude, that as to that report, ſomething extraordinary had happened in that place; or, that there was ſome miſtake of him to whom it was made; or, that divers did not deſcend to a ſufficiently conſiderable depth.

If I had been furniſhed with opportunity, I would have engaged ſome ingenious navigators to examine the temperature of the ſubmarine regions, both of differing ſeaſons of the year, eſpecially the hotteſt part of ſummer, and coldneſs of winter, and with hermetically ſealed weather-glaſſes, in order to the diſcovery of ſuch particulars as theſe, whether there be in ſome ſeas any ſuch varying differences of temperature, as may invite us at leaſt in ſome places, to make more than two ſubmarine regions: whether the ſubmarine coldneſs do at the bottom of the ſea, or elſewhere, either equal or ſurpaſs that degree, which we here find ſufficient to freeze common water: whether the parts of the ſea-water are ſtill the colder, as they are the deeper: and whether or no this increaſe of coldneſs be regular enough to be reducible to any ſettled proportion. But for the reſolving of theſe and the like queſtions, I did not cauſeleſly intimate, that a ſealed weather-glaſs was to be employed; for I take a common one to be altogether unfit for ſuch purpoſes; not only becauſe the ſea-water would mingle with ſuch liquors as are wont to be employed in it, for that inconveniency I could eaſily remedy, by ſubſtituting, as I have ſeveral times done in other caſes, mercury inſtead of ordinary liquors; but chiefly, becauſe the incumbent ſea water would gravitate upon the reſtagnant liquor of the weather-glaſs, and thereby render its informations falſe or uncertain. According to what I have had occaſion to obſerve in another tract.

Whereto, that there may not in this place be any need to recur, I ſhall add a ſlight experiment, that I made for the ſatiſfaction of ſome ingenious men not well acquainted with hydroſtaticks, or not rightly principled in them. And this trial I ſhall the rather mention, becauſe many will not allow water to preſs upon mercury immerſed therein, this being a far more ponderous liquor than that; and others will expect, that the included air, having no place to eſcape out at, ſhould reſiſt the aſcenſion of the ſubjacent mercury, more than indeed it will. We made then a ſmall weather-glaſs differing from common ones, beſides the bigneſs, in that it was furniſhed with mercury inſtead of water; and in that we employed to contain the ſtagnant mercury a glaſs vial with a narrow neck, wherein, by a piece of cork or two, the ſtem of the glaſs ball was well faſtened that this globular part of the inſtrument might not be lifted up when it was under water. Then having by applying cold water to the outſide of the ball endeavoured to reduce the air to the ſame temper with the water, or at leaſt to an approaching degree of coldneſs; and having taken notice of the ſtation of the mercury in the ſhank or ſtem above-mentioned, we did, by ſtrings tied about the neck of the ſmall vial, let the inſtrument gently down into a large tall glaſs body, filled with fair water, that the liquor and veſſel being both tranſparent, we might eaſily perceive the motions of the mercury in the ſlender pipe. By which means it appeared, that, as the thermometer deſcended deeper and deeper into the water, the mercury was preſſed up higher and higher in the ſtem. And that it may not be ſuſpected, that this aſcenſion proceeded only, or chiefly, from the refrigeration of the air by the water, I ſhall add to what I have juſt now noted, that though the coldneſs of the water may well be ſuppoſed uniform, as at leaſt to ſenſe; yet the whole inſtrument being leiſurely removed ſometimes to the upper ſurface of the water, ſometimes to the lower, the riſing and falling of the quickſilver in the ſlender pipe was ſuitable to the depth of its ſurface, or its diſtance beneath that of the water. (The like experiment we might have tried with a thermoſcope furniſhed with water, and let into oil, or with deliquated ſalt of tartar and pure ſpirit of wine inſtead of mercury and water; if we had been furniſhed with ſufficient quantities of thoſe liquors, and had judged it to be requiſite.) But this circumſtance I thought fit to admoniſh the ſpectators of, that it is not to be expected, that the mercury ſhould riſe as much in proportion, when it is (for example) a foot under water, as when it is but two or three inches; becauſe, according as the inſtrument is let down deeper, and the air crouded into a leſs room, the ſpring of that compreſſed air becomes the ſtronger, and makes the more reſiſtance. Which advertiſement agreed well with the experiment, whoſe other phænomena I paſs over as not pertinent to this place, where I would only juſtify what I ſaid of the unfitneſs of weather-glaſſes made, (though with other liquors) after the common ways for making the ſubmarine trials I propoſed.

BUT till ſuch artificial obſervations can be obtained, we may from what has been above delivered probably gather, that though the lowermoſt of the ſubmarine regions be very ſenſibly cold, yet water, at leaſt that of the ſea, does not by theſe phænomena appear to be the *ſummum frigidum*. Though I have been ſeveral times able to produce ice in ſalt-water, yet I find not by any obſervation, that there has been ice met with, and generated at the bottom of the ſea, under which the earth has been found unfrozen by our divers; and appears to be ſoft at depths exceedingly ſurpaſſing the greateſt they have reached; as is evident by the mud, gravel, &c. fetched from the bottom of the ſea by ſounding plummets, let down to 80 or 100 fathom, or even a greater depth, whereof examples may be met with in the journals of navigators: nay, my curioſity procured me this account, from the ſober commander of a ſhip, that came this year from the remoter parts of the great ocean, that at about 35 degrees of ſouthern latitude, the tallow, with which his ſounding lead was anointed, brought him up grey ſand from the immenſe depth of no leſs than two hundred and twenty fathom. But to this obſervation it is juſt to annex this caution, that we cannot ſafely conclude from men's finding no ice at the bottom of the ſea, that the cold there cannot be very intenſe; for, as I have found by more than one relation (*elſewhere recited) that, whatever the ſchools ſurmiſe, the ſea is at leaſt as ſalt at the bottom, as at the top; ſo I have more than once tried, that ſalt water will without freezing admit a much greater degree of cold than is neceſſary to turn freſh water into ice.

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* Notes about the ſaltneſs of the ſea.

RELATIONS
ABOUT THE
BOTTOM of the SEA.

SECTION. I.

I DO not pretend to have viſited the bottom of the ſea; but ſince none of the naturaliſts, whoſe writings I have yet met with, have been there any more than I; and it is great rarity in thoſe cold parts of *Europe* to meet with any men at all, that have had at once the boldneſs, the occaſion, the opportunity, and the ſkill, to penetrate into thoſe concealed and dangerous receſſes of nature, much leſs to make any ſtay there; I preſume it will not be unpleaſant, if about a ſubject, of which, though none of thoſe very few naturaliſts, that write any thing at all, write otherwiſe than by hear-ſay, I recite in this place, what I have learned by enquiry from thoſe perſons, that among the many navigators and travellers I have had opportunity to converſe with, were the likelieſt to give me good information about theſe matters.

It would be needleſs here to take notice, that the ſea is uſually cold and ſalt at the bottom; nor to repeat thoſe other things, that I have already delivered in other diſcourſes. I ſhall therefore begin what I have to ſay in this, by relating, that one of the chief things, that I was ſolicitous to enquire after about the bottom of the ſea, was, the inequality I ſuppoſed to be in the ſoil. For though the ſurface of the ſea, when it is not agitated by the winds, appears very plain and level, and though it be indeed, at leaſt in this, or that particular ſea, ſphærical and (phyſically ſpeaking) concentrical to the earth; yet I could not think it probable, for reaſons not neceſſary to be here diſcourſed of, that the bottom, the ſuperficies of the ground, or of the veſſel, that contained it, ſhould be either flat or level, or regularly concave.

To ſatisfy myſelf about this matter, I enquired of a perſon, that had viſited the famous pearl-fiſhing at the little iſland of *Manar* (near the rich iſle of *Ceylon*) in the *Eaſt-Indies*, and had by his ſtay there much opportunity to ſee divers at their work, and converſe with them. By the anſwers of this man, who was a ſcholar, I learned, that the divers had aſſured him, that they found the floor of the ſea, if I may ſo call it, in divers places, exceedingly unequal, in ſome places being flat, in others aſperated with crabby rocks a conſiderable height, and elſewhere ſinking into precipitous depths, in which they found it very cold.

Besides the recited teſtimonies of the divers, I enquired of ſeveral pilots and other navigators, that had made long voyages, what gradual or abrupt inequality they had obſerved at their ſoundings in very neighbouring places; it being eaſy to be gathered from thence, whether the ſea were there uniformly deep, or did at leaſt, with ſome regularity, alter its depth by degrees; or whether, as I ſuſpected, there were not at the bottom of the ſea, hilly places, and ſteep præcipices, and perhaps deep vallies or wells, as we obſerve in the diſcovered part of the terreſtrial globe.

By theſe inquiries, I obtained ſeveral obſervations, whereof the moſt material are thoſe that follow:

First, an ancient ſea-commander, that had many years frequented *Africa* and the *Indies*, told me, as others had done before, that when they ſailed in the ocean very far from ſight of land, they did not often put themſelves to the trouble of ſounding; but that as far as they had ſounded, he had uſually found the depth of the ſea, to increaſe or decreaſe gradually, without very great irregularities, excepting ſome places, inſtancing particularly in the excavation, that makes the bottom of the ſea, within ſight of the *Cape of Good Hope*, where though for the moſt part, he found the water to deepen more and more, as he ſailed farther from ſhore; yet in one place, he and others had met with a bank (as he conceived it to be) at a conſiderable diſtance from the ſurface of the water. So, that though when they were as they imagined near the edge of that bank, they found but a moderate number of fathoms, yet when ſailing a very little way farther, they had gone beyond it, they found the ſea of an immenſe depth. In ſhort, I gathered from his anſwers, that in the greater ſeas, he had found, for the moſt part, the ground at the bottom, to fall away by degrees; but nearer the ſhores, that is, within a moderate number of leagues, he obſerved in divers places, that the ſubmarine ground was very unequal, and had as it were, hills and præcipices.

A man of letters, that had ſailed both to the *Eaſt* and *Weſt-Indies*, and in divers other regions beſides, and had made ſome of his voyages in ſhips of ſuch great burthen, as obliged the mariners to be very frequent and careful in ſounding, informed me, that ſometimes at conſiderable diſtances from ſhore, he had obſerved the ſea to be 20, 30, or perhaps 40 fathom deeper when they caſt the ſounding lead from one ſide of the ſhip, than it had been juſt before, when they had ſounded from the other; and from other things, that he told me, I found myſelf much confirmed in the above propoſed opinion.

HEARING of a ſea-captain, of extraordinary ſkill in maritime affairs, that was come home this year from *Eaſt-India*, his reputation made me endeavour to have a little conference with him about the ſubject of this diſcourſe; but his occaſions haſtening him to another place, before I could ſend to him, I procured from the chief perſons, that employed him, a ſight of ſome notes touching his laſt voyage, which he had left with them; hoping to find there ſomething at leaſt about the ſoundings of ſo accurate a ſeaman; and accordingly I met with a paſſage, very pertinent to my purpoſe, and worthy to be here tranſcribed.

FEBRUARY 12. After our obſervation, (he means a former one very agreeable to this) ſeeing the ground under us, we heaved the lead, and had but 19 fathom rocky ground, then haled by N. N. E. the wind at N. W. and found out water to ſhoal from 19 to 10 and 8 fathom hard coral ground, then ſuddenly deepened again from 8 to 20 and 22 fathom ſandy ground, and then ſuddenly ſaw rocks under us, where we had but 7 fathom, and the next caſt 14 fathom again. And ſo having run N. N. E. from 6 in the morning until 12 at noon about 19 mile, we deepened our water, from 16 to 25, and the next caſt, no ground with 35 fathom of line.

LASTLY, having opportunely met with an ancient navigator, who paſſes for the moſt experienced pilot in our nation for an Eaſt-Indian voyage; I aſked him about his own obſervations concerning theſe unequal ſounding, I was anſwered, that he had not only met with them elſewhere, but, that not far from the mouth of our channel, he had ſometimes found the bottom of the ſea ſo abrupt, that in ſailing twice the length of the ſhip, he had found the water deepen from 30 fathom to a hundred, if not alſo much more.

SINCE I received theſe relations, having the honour to diſcourſe with a noble perſon, who has divers times deſervedly had the command of Engliſh fleets, and is no leſs curious than intelligent in maritime affairs, I took the opportunity to inquire of his lordſhip, whether he had not obſerved the bottom of the ſea to be very unequal in neighbouring places? To which he replied, that he had found it exceedingly ſo. And to ſatisfy me, that he ſpoke not upon meer conjecture, he told me, that ſailing once with his fleet, even in our channel, he perceived the water to make a ripling noiſe (as the ſeamen call it) as the *Thames* does under *London-Bridge*. So that he was afraid they were falling upon ſome ſhoal, the water being 12 or 14 fathom deep, and going on a little farther, he caſt out the plummer again, and found it about 30 fathom. He added, that he made divers ſuch obſervations, but took notice of ſuch ripling waters, only when the tide was ebbing: and yet in a deep ſea meeting with the like appearance in the upper part of the water, and thinking it improbable, that there ſhould be any ſhoal there, he ordered the depth to be ſounded, and found it to exceed 30 fathoms; and after he had paſſed on a very little farther, he found the ſea ſo deep, that he could not fathom it with his ordinary line.

SECTION. II.

ANOTHER thing obſerved at the bottom of the ſea is the great preſſure of the water there againſt other bodies. For what ever men may philoſophize in their ſtudies, and may conclude from the principles, that are generally received about the non-gravitation of water in its proper place, yet experience ſeems very little to favour that general doctrine.

FOR firſt I remember, that having cauſed a pretty large cylinder of glaſs, that was open only at one end, to be ſo depreſſed into a large glaſs-veſſel full of water, with a conveniently applied weight of lead, that none of the air could get out, I could eaſily diſcern through the liquor and veſſels, which were all tranſparent, that as the inverted cylinder deſcended deeper and deeper, the external water compreſſed the impriſoned air, and aſcended higher and higher in the cavity of the cylinder, againſt whoſe ſide we had beforehand placed a row of marks, whereby to take notice of the gradual aſcent and deſcent of the internal water.

SECONDLY, having inquired of two ſeveral obſerving perſons, whereof one had with a diving engine viſited the bottom of the ſea in a cold northern region; and the other had done the like in an engine much of the ſame ſort, upon the coaſt of Africk; I found their relations to agree in this, that the deeper they deſcended into the ſea, the more the air they carried down with them was compreſſed, and the higher the water aſcended above the lip, or brim of the engine into the cavity of it.

BUT I ſhall now add a more conſiderable experiment or two, to the ſame purpoſe. For diſcourſing one day with an engineer of my acquaintance, that had been often at ſea, and loved to try concluſions, of a way I had thought of, to make ſome eſtimate of the preſſure of the water at a conſiderable depth beneath the ſurface, and ſhew, that the preſſure is great there; he told me, he could ſave me the labour of ſome trials by thoſe he had made already, and aſſured me, that having divers times opportunity to ſail near the ſtreights mouth, over a place where the ſea was obſerved to be of a notable depth, he had found, that if he had let down, with a weight into the ſea, not a ſtrong round glaſs bottle, but a vial, ſuch as the ſeamen uſe to carry their brandy and ſtrong waters in; ſuch a veſſel, which might contain a pint or quart of water, would, when it come to be ſunk 40 fathom under water, if not ſooner, be ſo oppreſſed, by the preſſure of the incumbent, and lateral water, as to be thereby broken to pieces.

HE alſo averred to me, that having exactly cloſed an æolipile of metal, and with a competent weight, ſunk it to a great depth in the ſea, as to forty, fifty, or ſixty fathom deep, when he pulled it up again, he found to his wonder, that the great preſſure of the water had in divers places cruſhed it inwards. And though I had ſome ſuſpicion, that the coldneſs of the ſea, at ſuch a depth, might, by weakening the ſpring of the included air, ſomething contribute to the effect, yet I did not admire the event, having divers years before had a thin æolipile of copper cruſhed inwards by the preſſure of a much lighter fluid than ſea-water.

SECTION III.

ANOTHER thing obſerved in the bottom of the ſea is the tranquillity of the water there, if it be conſiderably diſtant from the ſurface. For though the winds have power to produce vaſt waves in that upper part of the ſea, that is expoſed to their violence; yet the vehement agitation diminiſhes by degrees, as the parts of the ſea, by being deeper and deeper lie more and more remote from the ſuperficies of the water. So that the calm being leſs and leſs diſturbed towards the bottom of the water, if that lie conſiderably deep, the water is there either calm, or ſcarce ſenſibly diſturbed.

BUT that is for the moſt part to be underſtood of places at ſome diſtance from the ſhore; for oftentimes, in thoſe that are too near it, the progreſs of the waters being rudely checked, and other cicumſtances concurring, the commotion of the water is ſo great, that it reaches to the very bottom, as may appear by the heaps of ſand, the amber, and, in ſome places, the ſtones, that are wont to be thrown up by the ſea, in and after ſtorms.

THE above mentioned calmneſs of the ſea at the bottom will (I doubt not) appear ſtrange to many, who admiring the force of ſtormy winds, and the vaſtneſs of the waves they raiſe, do not, at the ſame time, conſider the almoſt incomparably greater quantity, and weight of water that muſt be moved, to make any great commotion at the bottom of the ſea, upon which ſo great a maſs of ſalt water, which is heavier than freſh, is conſtantly incumbent. Wherefore for the proof of the propoſed paradox, I will here ſet down a memorable relation, which my inquiries got me from the diver, elſewhere mentioned, who by the help of an engine could ſtay ſome hours under water.

THIS perſon then being aſked, whether he obſerved any operation of the winds at the bottom of the ſea, where it was of any conſiderable depth? anſwered me to this purpoſe, that the wind being ſtiff, ſo that the waves were manifeſtly ſix or ſeven foot high above the ſurface of the water, he found no ſign of it at 15 fathom deep; but if the blaſts continued long, then it moved the mud at the bottom, and made the water thick and dark. And I remember he told me, which was the circumſtance I chiefly deſigned, that ſtaying once at the bottom of the ſea very long, where it was conſiderably deep, he was amazed at his return to the upper parts of the water, to find a ſtorm there, which he dreamt not of, and which was raiſed in his abſence, having taken no notice of it below, and having left the ſea calm enough when he deſcended into it.

FOR farther confirmation, I ſhall add, that having inquired of a great traveller, who had aſſiſted at a rich pearl-fiſhing in *Eaſt Indies*, whether he had not learned by his converſation with the divers, that ſtorms reach not to the bottom of the ſea, if it be of any conſiderable depth; he anſwered, that he had ſeen the divers take the water, when the ſea was ſo very rough, that ſcarce any veſſels would hazard themſelves out of ports; that thoſe returning divers told him, that at the bottom they had found no diſturbance of the water at all. Which is the more conſiderable, becauſe of the ſituation of that place where they dive for pearls; for this is near the ſhore of *Manar*, and that it ſelf is ſeated between the great iſland of *Ceylon*, and the vaſt cape of *Comori*; and though it may be much nearer the former, is not yet far diſtant from the latter. Which ſituation and the neighbourhood of the vaſt Indian ocean, on the one ſide of *Ceylon*, and the great gulph of *Bengala*, (antiently *Sinus Gangeticus*) on the other, makes the place, where the pearls are fiſhed for, exceeding likely to be ſubject to very troubled ſeas.

IT will perhaps be thought no ſlight addition to the fore-going arguments, if I here add, that meeting one day with an ancient and expert ſeaman, whom his merit had advanced to conſiderable employments in his profeſſion, I was confirmed by the enquiries I made of him, not only in the opinion I had about the calmneſs of the bottom of the ſea, but alſo, that the operation of good gales of wind, does oftentimes not reach to near ſo conſiderable depths into the ſea, as hath been hitherto ſuppoſed, even by navigators themſelves. For he aſſured me, that having ſometimes ſailed in great ſhips, that drew much water, as about 12 or 15 foot, he had dived to the keel of the ſhips, when they were under ſail, and obſerved the agitations of the water to be exceedingly diminiſhed, and grown very languid, even at that ſmall diſtance, from the upper part of the waves. And he farther anſwered, that when in *America* he learned to dive of the Indians, they taught him by their examples,. to creep along by the rocks and great ſtones, that lay near the ſhore, at the bottom of the water, to ſhelter themſelves from the ſtrokes, and other ill effects of the billows, which near the ſhore, and where the ſea was ſo ſhallow, as it was there, did often hurt and endanger ſwimmers and unſkilful divers. But when they were, by this means, got farther from ſhore and into deeper water, they would ſecurely leave the ſhelter, they had till then then made uſe of, and ſwim within a few yards of the ſurface of the and commotions of the upper parts of the water.

BUT laſtly, for further ſatisfaction, I had the opportunity to make inquiry about this matter of a great ſea commander, who has both an extraordinary curioſity to make marine obſervations, and an unuſual care in making of them accurately, I found the opinion countenanced by his anſwer, which was in ſhort; that he had lately been at a place, where the ſea was often tempeſtuous enough, and that they found by a ſure mark, that the ſtorm did not reach with any efficacy four fathom beneath the ſurface of the water.

About the tranquillity of the lower parts of very deep waters I had a ſuſpicion, which, though I fear it might ſeem ſomewhat extravagant, becauſe I have not met with it in authors; yet I thought it worth examining for the uſe it might be of, if reſolved, in reference to the ebbing and flowing of the ſea.

I made therefore a ſolicitous inquiry, whether the tides did reach to, or near the bottom of the deeper ſeas, but found it exceeding difficult, by reaſon of men's want of curioſity to obtain any ſatisfaction about a problem, that moſt navigators I have converſed with did not ſeem to have ſo much as dreamed of. But thus much I found indeed, by inquiring of an engineer, who was curious of marine obſervations, that a famous ſea-commander of his his acquaintance, being alſo a great mathematician, had affirmed to this relator, that he had divers times obſerved, that when he let down his plummet to a great depth, but yet not to reach ground, it would be quickly carried by a motion quite contrary to that of the ſhallop, whence they ſounded, and very much quicker than it; but I had this only at ſecond hand. Alſo, if I miſ-remember not, I was informed by a ſkilful obſerver, that commanded many of our Engliſh men of war, that he had, near the Sound, obſerved the upper and lower parts of the water to move with a conſiderable ſwiftneſs quite different ways; but not having committed this relation to writing, I dare not build much upon it. And among the anſwers I had received, and written down concerning thoſe matters, all that I can yet find among my adverſaria is a relation, which though ſingle, will not be unworthy to be tranſcribed in this place, becauſe the perſon who gave it me, is one of the ancienteſt and moſt experienced pilots of our nation.

This perſon therefore aſſured me, that ſailing beyond the *Cape of Good Hope*, into the *South-Seas*, he made trials of the motion of the upper part of the water above the lower; where ſometimes caſting out a large and heavy plummet, he let it down to ſeveral depths ſhort of 50 fathom, without any ſenſible operation upon the motion of the boat, or ſhallop, he ſtood in to make the trial; but when he let down the plummet lower, to about an hundred fathom or more, then he found, that though the plummet reached not to the bottom of the water, yet upon the ſcore of the ſtanding water beneath, the ſuperior water would make the boat turn towards the tide or current, as if it lay at anchor, and the water would run by the ſide of the boat, at the rate of about three miles an hour: thus far this diligent obſerver. But how far the inequality of the ſoil at the bottom of the ſea, and how far various depth of the water, and ſome other circumſtances, may alter the caſe, and make it hard to determine, what ought to be aſcribed to tides, and what to currents, are things, which I will by no means be poſitive in, till I can meet with further information.

[Since the writing of this, happening to meet with one, that ſpent ſome time at a famous eaſtern pearl-fiſhing, and aſked him, whether he had inquired of the divers about the problem lately propoſed, and whether the ſea were there deep enough to make obſervations of that kind: to the latter part of which queſtion he replied, that in ſome places it was of a very conſiderable depth, and fit to make the obſervation in; and to the former he anſwered, that he had inquired of the divers, who affirmed to him, that ſometimes at the bottom of the deep waters, there ſeemed to be a ſtagnation of the Sea for a great depth, ſo that till ſuch a height they could riſe directly upwards, but that at other heights, they would be carried away by the leſs deep waters; ſo as to be found, when they came to emerge a great way off from that point of the ſurface, which was perpendicular to that place at the bottom, whence they began to aſcend.]

NEW PNEUMATICAL EXPERIMENTS ABOUT RESPIRATION.

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TITLE I.

Obſervations made about the laſting of ducks included in the exhauſting receiver.

NATURE having, as Zoologiſts teach us, furniſhed ducks and other water fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they have occaſion to dive, to forbear for a pretty while reſpiring under water without prejudice; I thought it worth the trial, whether ſuch birds would much better than other animals endure the abſence of the air in our exhauſted receiver. The accounts of, which trials were, when they were made, regiſtered as follows.

EXPERIMENT I.

WE put a full grown duck (being not then able to procure a fitter) into a receiver, whereof ſhe filled, by our gueſs, a third part, or ſomewhat more, but was not able to ſtand in any eaſy poſture in it: then pumping out the air, though ſhe ſeemed at firſt (which yet I am not too confident of, upon a ſingle trial,) to continue well ſomewhat longer than a hen in her condition would have done; yet within the ſhort ſpace of one minute ſhe appeared much diſcompoſed, and between that and the ſecond minute, her ſtrugling and convulſive motions increaſed ſo much, that, her head alſo hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which we preſently reſcued her by letting in the air upon her: ſo that this duck being reduced in our receiver, to a graſping condition, within leſs than two minutes, it did not appear, that, notwithſtanding the peculiar contrivance of nature, to enable theſe water birds to continue without reſpiration for ſome time under water, this duck was able to hold out conſiderably longer than a hen, or other bird not aquatick, might have done: and to manifeſt, that it was not cloſeneſs and narrowneſs of the veſſel, in reference to ſo bulky an animal, that produced in the ſubject of our trial the great and ſudden change above-recited, we ſoon after included the ſame bird in the ſame receiver, and having by a ſpecial way cemented it on very cloſe, we ſuffered her to ſtay thus ſhut up with the air for five times as long as formerly (by our gueſs, helped by a watch) without perceiving her to be diſcompoſed; and ſhe would probably have continued longer in the ſame condition, if my patience and leiſure would have held out ſo long, as ſhe could have done in that priſon.

EXPERIMENT II.

HAVING at the ſeaſon of the year procured a duckling, that was yet callow, we conveyed her into the ſame receiver, wherein the former had been included, and obſerved, that, though for a while ſhe appeared not much diſquieted, whilſt the air was pumping out of the glaſs, yet before the firſt minute was quite ended, ſhe gave manifeſt tokens of being much diſordered; and the operation being continued a while longer, ſhe grew ſo much worſe, that ſeveral convulſive motions, ſhe fell into before a ſecond minute was expired, obliged us to let in the air upon her, whereby ſhe quickly recovered,

N. B. I determine not, whether it be proper in this place to add, that when the receiver was pretty well exhauſted, the included bird appeared to the ſpectators manifeſtly bigger, than before the air was withdrawn, eſpecially about the crop, though that was very turgid before. And to manifeſt, that in this duck, as in the former, the convulſions, that uſed to be immediately followed by death, proceeded from the withdrawing of the ambient air, and not from the clogging of it; we kept the ſame duckling in the ſame receiver very cloſe, to keep out all external air, and to keep in the excrementitious ſteams of her body for above 6 minutes, without perceiving her to grow ſick upon her impriſonment; which yet laſted above thrice the time, that ſufficed to reduce her in the abſence of the air to a gaſping condition.

N. B. IT not being intended, that ducks and other water fowl, ſhould any more than other birds, live in an exceeding rarified air, but only be able to continue upon occaſion a pretty while under water, it may ſuffice, that the contrivance of thoſe parts, which relate to reſpiration, be ſo far fitted for the purpoſe, as we ſhall ſee it is, when we come to the tenth title.

TITLE II.

Of the phænomena afforded by vipers included in an exhauſted receiver.

CONSIDERING, that vipers are animals endowed with lungs (though of a different ſtructure from thoſe of men, dogs, cats, and birds, &c.) and that their blood is, as to ſenſe, actually cold; I thought it might, upon both theſe accounts, be very well worth trying, what effect the withdrawing and abſence of the air would have upon animals ſo conſtituted. I therefore made divers trials, ſome of which did not diſpleaſe me; but I know not by what misfortune the memorials of them were loſt, except two or three, which were not perfect, that I ſhall here ſubjoin.

EXPERIMENT I.

Jan. 2, 166$\frac{2}{3}$. WE included a viper in a ſmall receiver, and as we drew out the air, ſhe began to ſwell, and afforded us theſe phænomena.

1. IT was a good while after we had left pumping, before the viper began to ſwell ſo much as to be forced to gape, which afterwards ſhe did.

2. THAT ſhe continued, by our eſtimate, above two hours and half in the exhauſted receiver, without giving clear proof of her being killed.

3. THAT after ſhe was once ſo ſwelled, as to be compelled to open her jaws, ſhe appeared ſlender and lank again; and yet very ſoon after appeared ſwelled again, and had her jaws disjoined as before.

EXPERIMENT II.

WE took a viper, and including her in the greateſt ſort of ſmall receivers, we emptied the glaſs very carefully, and the viper moved up and down within, as if it were to ſeek for air, and after a while, foamed a little at the mouth, and left off that foam ſticking to the inſide of the glaſs: her body ſwelled not conſiderably, and her neck leſs, till a pretty while after we had left pumping; but afterwards the body and neck grew prodigiouſly tumid, and a bliſter appeared upon the back. An hour and an half after the exhauſtion of the receiver, which we then by trial found to be pretty ſtaunch, the diſtended viper, did give by motion manifeſt ſigns of life; but we obſerved none afterwards. The tumor reached to the neck, but did not ſeem much to ſwell the under-chap, both the neck, and a great part of the throat, being held betwixt the eye and the candle, were tranſparent enough, where the ſcales did not darken them. The jaws remained mightily opened, and ſomewhat diſtorted; the epiglottis with the rimula laryngis, which remained gaping, was protruded almoſt to the farther end of the nether-chap. As it were from beneath this epiglottis came the black tongue, and reached beyond it, but ſeemed by its poſture not to have any life, and the mouth alſo was grown blackiſh within: but the air being re-admitted after 23 hours in all, the viper's mouth was preſently cloſed, though ſoon after it was opened again, and continued long ſo; and ſcorching or pinching the tail made a motion in the whole body, that argued ſome life.

EXPERIMENT III.

April 25. To theſe experiments upon vipers I ſhall add one, made upon an ordinary harmleſs ſnake.

WE included ſuch an animal, together with a gage, in a pretty portable receiver, which, being exhauſted, and well ſecured againſt the ingreſs of the air, was laid aſide in a quiet place, where it continued from 10 or 11 of the clock in the forenoon, till about nine the next morning; and then my occaſions calling me abroad, I looked upon the ſnake, which, though he ſeemed to be dead, and gave no ſigns of life upon the ſhaking of the receiver; yet, upon holding the glaſs a convenient diſtance from a moderate fire, he did in a ſhort time manifeſt himſelf to be alive by ſeveral tokens, and even by putting forth his forked tongue. In that condition I left him, and, by reaſon of ſeveral avocations, came not to look upon him again till the next day early in the afternoon; at which time he was grown paſt recovery, and his jaws, which were formerly ſhut, gaped exceeding wide, as if they been ſtretched open by ſome external violence.

TITLE III.

Of the phænomena afforded by frogs in an exhauſted receiver.

Sept. 9, 1662. THE ſame conſiderations, that induced me to make ſeveral trials upon vipers, did alſo invite me to make ſeveral upon frogs; the ſucceſs of ſome of which the following notes will declare.

EXPERIMENT. I.

WE took a large luſty frog, and having included her in a ſmall receiver, we drew out the air, and left her not very much ſwelled, and able to move her throat from time to time, though not ſo faſt, as when ſhe freely breathed from the exſuction of the air. She continued alive about two hours, that we took notice of, ſometimes removing from the one ſide of the receiver to the other; but ſhe ſwelled more than before, and did not appear by any motion of her throat, or thorax, to exerciſe reſpiration, but her head was not very much ſwelled, nor her mouth forced open. After ſhe had remained there ſomewhat above three hours, (for it was not three half hours) perceiving no ſign of life in her, we let in the air upon her, with which the formerly tumid body ſhrunk very much, but ſeemed not to have any other change wrought in it; and though we took her out of the receiver, yet in the free air itſelf, ſhe continued to appear ſtark dead. Neverthele ſs, to ſee the utmoſt of the experiment, having cauſed her to be laid upon the graſs in a garden all night, the next morning we found her perfectly alive again.

EXPERIMENT II.

June 29, 1660.

ABOUT 11 of the clock in the forenoon, we put a frog into a ſmall receiver, containing about 15¼ ounce troy weight of water, out of which we had tolerably well drawn the air, (ſo that when we turned the cock under water, it ſucked in about 13¼ ounce of water:) the frog continued in it (the receiver all the while under water) lively enough until about 5 of the clock in the afternoon, when it expired. The frog at the firſt ſeemed not to be much altered by the exſuction of the air, but continued breathing both with her throat and lungs.

EXPERIMENT III.

Sept. 6, 1662.

WE included into a pretty large receiver a couple of frogs newly taken, the one not above an inch long, and proportionally ſlender; the other very large and luſty. Whilſt the air was drawing out, the leſſer frog ſkipped up and down very lively, and, ſomewhat to our wonder, clambered up ſeveral times to the ſides of the receiver, inſomuch, that he ſometimes reſted himſelf againſt the ſide of the glaſs. When his body ſeemed to be perpendicular to the horizon, if not in a reclining poſture, he continued to ſkip up and down a while after the exſuction of the air; but within a quarter of an hour (meaſured by a minute watch) we perceived him to lie ſtark dead with his belly upwards. The other frog, that was very large and ſtrong, though he began to ſwell much upon the withdrawing of the air, and ſeemed to be diſtreſſed, by his frequently leaping up after the air was drawn out, which he did not before, yet being; as we ſaid, very luſty, he held out half an hour, at which time it was remarkable, that the receiver, though it had held out againſt the preſſure of the outward air, during, that ſpace of time, notwithſtanding, that a piece of it had been cracked out, and was mended, with a cloth deeped in cement, yet at the end of the half hour, the weight of the outward air ſuddenly beat it in, and thereby brought the impriſoned frog a reprieve, which hindered us from bringing the experiment to an iſſue.

EXPERIMENT IV.

Sept. 11.

WE took a ſmall frog, and having conveyed her into a very ſmall portable receiver, we began to pump out the air. At firſt ſhe was lively enough, but when the air began to be conſiderably withdrawn, ſhe appeared to be very much diſquieted (leaping ſometimes after an odd manner, as it were, to get out of the uneaſy priſon, but yet not ſo, but, that after the operation was ended, and the reciver taken off, the frog was perfectly alive, and continued to appear ſo (if I am not miſtaken) near an hour, though the abdomen was very much, and the throat ſomewhat extended; this latter part having alſo left, that wonted panting motion, that is ſuppoſed to argue and accompany the reſpiration of frogs. At the end of about three quarters of an hour, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which by that time was ſtrangely ſwelled, did not only ſubſide, but ſeemed to have a great cavity in it, as the throat alſo proportionably had; which cavities continued, the frog being gone paſt all recovery.

EXPERIMENT V.

April 14.

A large frog was conveyed into a plated receiver, and the air being withdrawn, her body by degrees was diſtended; as appeared very notably, when by a caſual ſpringing of a leak, the air got in again, and made her look much more lank and hollow than ever. The receiver with the gage were kept under water near ſeven hours, becauſe I was obliged to ſtay long abroad; at the end of which coming home, I found the receiver ſtaunch, but the frog dead and exceedingly ſwelled: upon the letting in of the air, ſhe became more hollow and lank than ever.

N. B. I have purpoſely, both under this title, and ſome others, ſubjoined ſome trials, whoſe events are not altogether ſuch, as others recited under the ſame head, which would invite one to expect; but I purpoſely do it, not only to be true to the impartiality, I propoſed to myſelf, in writing theſe narratives, but to awaken the curious to conſider and obſerve, what variety of phænomena in ſuch trials may be attributed to the ſeaſon of the year, wherein they are made; and to ſtrength, bulk, age, peculiar conſtitutions, &c. that relate to the reſpective animal, on which the experiments are made; beſides what things may on other account be fit to be alſo conſidered.

TITLE IV.

Of the phænomena afforded by a new kittened kitling in the exhauſted receiver.

BEING deſirous to try, whether animals, that had lately been accuſtomed to live, either without any, or without a full reſpiration, would not be more difficult or ſlowly killed by the want of the air, than others, which had been been longer uſed to a free reſpiration; we took a kitling, that had been kittened the day before, and put it into a very ſmall receiver (that we gueſſed to hold about a pint or leſs,) that it might be the ſooner exhauſted. As ſoon as the pump began to play, I took notice of the time, and found by a watch, that marks minutes and quarter minutes, within one minute or little more, after the air firſt began to be withdrawn, that the little animal, who in the mean time had gaſped for life, and had ſome violent convulſions, lay as dead, with his head downwards, and his tongue out; but upon letting in of the air, he did in a trice ſhew ſigns of life, and being taken out of the receiver quickly recovered: and to allow him the benefit of his good fortune, we ſent for a kitling of the ſame age and litter, which being put into the ſame receiver, quickly began, like the other, to have convulſions, after which he lay as dead; but obſerving very narrowly, I perceived ſome little motions, which made me conclude him alive; which I ſoon found I had cauſe to do. For though we continued pumping, and could not perceive, that the engine leaked more than in the former experiments; the kitling began to ſtir again, and after a while had ſtronger and more general convulſions than before; until at the end of full ſix minutes, after the exſuction of the air was begun, the animal ſeeming quite dead, the outward air was re-admitted into the receiver, which not reviving him as it had done the other, he was taken out of the veſſel, and lay with his mouth open, and his tongue lolling out, without any ſenſible breathing, and pulſation; until having ordered him to be pinched, the pain or ſome internal motion, produced by the external violence done to him, made him immediately give manifeſt ſigns of life, though there was yet no ſenſible motion of the heart, or the lungs; but afterwards gaping and fetching his breath in an odd manner, and with much ſtraining, as I have ſeen ſome fœtus's do, when cut out of the womb, he, little by little, within about a quarter of an hour recovered: wherefore thinking it ſevere to make him undergo the ſame meaſure again, we ſent for another, kittened at the ſame time, and incloſing that alſo in the receiver, obſerved, that divers violent convulſions, as it were gaſping for breath, into which he began to fall at the ſecond or third ſuck, ended in a ſeeming death, within about a minute and a half. But being made more diffident by the late experiments, I cauſed the pump to be plied, and the rather, becauſe I had a mind to obſerve, whether, when the air was from time to time drawn away, there would not, upon the opening of the ſtop-cock to let it out, appear ſome ſudden ſwelling, greater or leſs, of the body of the animal, by the ſpring and expanſion of ſome air (or aerial matter) included in the thorax, or the abdomen. Such an inflation (though not great) we thought we obſerved; but until farther trial, I dare not acquieſce in it. A while after, notwithſtanding our continuing to pump, the kitling gave manifeſt ſigns of life, which was not until it had endured divers convulſions, as great as thoſe of the firſt fit, if not greater. When 7 minutes from the beginning of the exhauſtion were compleated, we let in the air; upon which the little creature, that ſeemed ſtark dead before, made us ſuſpect, that he might recover; but though we took him out of the receiver, and put aqua vitæ into his mouth, yet he irrecoverably died in our hands.

THESE trials may deſerve to be proſecuted with farther ones, to be made not only with ſuch kittens, but with other very young animals of different kinds; for by what has been related, it appears, that thoſe animals continued 3 times longer in the exhauſted receiver, than other animals of that bigneſs would probably have done.

TITLE V.

Some trials about the air, uſually harboured and concealed in the pores of water, &c.

IT might aſſiſt us to make the more rational conjectures about the phænomena of divers of our experiments, if we knew (ſomething near) what quantity of aerial ſubſtance is uſually found in the liquors we employ about them, eſpecially in that moſt common of them, water. And therefore, though it be very difficult (if at all poſſible) to determine the proportion of the air, that lurks in water, with any thing of certainty, many circumſtances making it ſubject to vary very much; yet to make the eſtimate, I eaſily could, where none at all, that I know of, hath been hitherto made by any man, I conſidered, that it might afford us ſome light, if we diſcovered, at leaſt, what proportion as to bulk, the air latitant in a quantity of water would have to the liquor it came from, when the aerial particles ſhould be gathered together into one place. For, though about this union, and the ſpring that may be conſequent to it, ſome doubts may be ſuggeſted, which I have not now time to diſcuſs; yet I ſuppoſed, that, at leaſt, ſome diſcoveries would by this way be made, though not of the true proportion between the air and the water, yet about two or three particulars, in due time to be taken notice of.

TO find inſtruments, which would any way accommodate our purpoſe, proved a very difficult work; ſo that among other things, that we were fain to do, this was one, that to evince how little the air, latitant in water, did appear to leſſen the bulk of that water, if it were ſuffered to fly away in an open tube; we ſuffered it to eſcape in an exhauſted receiver, without any artifice to catch it; by which trial the water did not part with any thing of its bulk, that made a diminution ſenſibly to the eye. Wherefore we endeavoured to make this loſs viſible by ſome other trials, of which I can find but a few haſty memorials among my looſe entries.

A chemical pipe ſealed at one end, and 36 inches, or ſomewhat leſs, in length, was filled with water, and inverted into a glaſs veſſel, not two inches in diameter, but $\frac{1}{4}$ of an inch, or little more in depth. Theſe glaſſes being conveyed into a fit receiver, and the air being leiſurely pumped out, and ſomewhat ſlowly re-admitted, the numerous bubbles, that had aſcended, during the operation, conſtituted at the top, an aerial aggregate, mounting to $\frac{8}{10}$, wanting about 100 part of an inch.

PRESENTLY after, the tube (by and by to be deſcribed) was filled again with the ſame water, and inverted; and the water being drawn down to the ſurface of the veſſeled water, and the air let in again, the water was impelled up to the very top, within a tenth and half a tenth of an inch.

Theſe are two experiments.

THE tube for meaſuring the air latitant in water was 43 inches and $\frac{1}{4}$ above the ſurface of the ſtagnant water: the air collected out of the bubbles at the top of the water was the firſt time $\frac{1}{4}$ of an inch, and ſomewhat better; the ſecond time we eſtimated it but $\frac{1}{2}$ and $\frac{1}{16}$. The firſt time the water in the pipe was made to ſubſide full as low as the ſurface of the reſtagnant water; the ſecond time the loweſt, we made it ſubſide, ſeemed to be four or five inches above the ſurface of the water in the open veſſel.

MATTER of fact thus recited would afford divers difficulties worthy to be conſidered, which I have not leiſure to diſcuſs; eſpecially, the odd thing, that happens to the aerial particles of water: for though, whilſt they lay concealed in the water, they took up ſo little room in it, that it was inſenſible; and when they were permitted to eſcape out of the tube, the water was not manifeſtly diminiſhed by their receſs; yet when they were aſſociated at the top of the tube, their aggregate did ſometimes maintain a place, that was conſiderable enough in reference to the capacity of the whole tube; though I muſt here advertiſe, that this aggregate did, at the top of the tube, poſſeſs more room than its bulk did abſolutely require; becauſe it was ſomewhat defended from the preſſure of the atmoſphere, by the weight of the ſubjacent cylinder of water, which might be about three or four foot long.

QUERE, Whether any conſiderable proportion of bubbles will be afforded by the ſame liquor, if it be ſuffered to continue in the glaſs for ſome competent time, after it has been once, or oftner, freed from bubbles already?

QUERE, How far it may be worthy our conſideration, whether, in common water, there may not be concealed air enough to be of uſe to ſuch cold animals as fiſhes; and whether it may be ſeparable from the water, that ſtrains through their gills?

BUT though I was at firſt content to make uſe of this way of eſtimating the air concealed in water; yet, when I came where I could be a little better accommodated with glaſſes, I bethought myſelf of a ſmall inſtrument, that would much better diſcloſe the wonderful plenty of the aerial particles I deſigned to diſcover. The ſtructure and uſe of this glaſs, may be eaſily enough underſtood by the recital of the firſt experiment, that was made with it, whereof take the following tranſcript.

WE provided a clear round glaſs, furniſhed with a pipe or ſtem of about nine inches in length, the globulous part of the glaſs being on the outſide about three inches and half in diameter: the pipe of this glaſs was within an inch of the top, melted at the flame of a lamp, and drawn out for two or three inches as ſlender as a crow's quill, that the decrement of the water upon the receſs of the air, harboured in its pores, might, if any ſhould happen, be the more eaſily obſerved and eſtimated. Above this ſlender part of the pipe, the glaſs, as was before intimated, was of the ſame largeneſs, or near it, with the reſt of the pipe, that the aerial bubbles, aſcending through the ſlender part, might there find room to break, and ſo prevent the overflowing, or loſs of any part of the water.

THIS veſſel being not without difficulty and ſome induſtry filled, till the liquor reached to the top of the ſlender part, where not being uniformly enough drawn out, it was ſomewhat broader than elſewhere; we conveyed the glaſs, together with a pedeſtal for it to reſt upon, into a tall receiver, and pumping out the air, there diſcloſed themſelves numerous bubbles, aſcending nimbly to the upper part of the glaſs, where they made a kind of froth, or foam; but by reaſon of the above-mentioned figuration of the veſſel, they broke at the top of the ſlender part, and ſo never came to overflow.

THIS done, the pump was ſuffered to reſt a-while, to give the aerial particles, lodged in the water, time to ſeparate themſelves, and emerge; which when they had done a pretty while, the pump was plied again, for fear ſome air ſhould have ſtolen into ſo large a receiver. Theſe viciſſitudes of pumping and reſting laſted for a conſiderable time, till at length the bubbles began to be very rare, and we weary of waiting any longer: ſoon after which, the external air was let into the receiver, and it appeared ſomewhat ſtrange to the ſpectators, that notwithſtanding ſo great a multitude of bubbles, as had eſcaped out of the water, I could not by attentively comparing the place, where the ſurface of the water reſted at firſt, to which a mark had been aſſixed, with that where it now ſtood; I could not, I ſay, diſcern the difference to amount to above, if ſo much, as an hair's breadth; and the chief operator in the experiment profeſſed, that, for his part, he could not perceive any difference at all.

THUS far for the narrative of the trial made by water: but, that was not the only liquor, into whoſe aerial particles I deſigned by our little inſtrument to inquire; and therefore filling a glaſs of the ſame ſhape, and much of the ſame bigneſs, with claret wine, and placing it upon a convenient pedeſtal, in a tall receiver, we cauſed ſome of the air to be pumped out; whereupon, in a ſhort time there emerged, through the ſlender pipe, ſo very great a multitude of bubbles, that were darted, as it were, upwards, as did not a little, both pleaſe and ſurprize the beholders; but it forced us to go warily to work, for fear the glaſs ſhould break, or the wine overflow. Wherefore we ſeaſonably left of pumping, before the receiver was any thing near exhauſted, and ſuffered the bubbles to get away as they could, till the preſent danger was over-paſſed; and then from time to time, we pumped a little more air out of the receiver, till we were weary, the withdrawing of a moderate quantity of air at a time ſufficing, even at the latter end, to make the bubbles not only copiouſly, but very ſwiftly to aſcend (by a minute watch) for above a quarter of an hour together.

THE little inſtrument, made uſe of about theſe trials, being deſigned to examine, among other things, the quantity of bubbles lurking in ſeveral liquors, is to be applied to ſpirit of wine and chemical oils, that are more ſubtile liquors than wine itſelf. And ſome circumſtances of our trials made us think, that it might be worth examining, what kind of ſubſtance may be obtained by this way of handling aerial and ſpirituous corpuſcles. But of the other uſes of our inſtrument elſewhere.

TITLE VI.

Of ſome phænomena, afforded by ſhell-fiſhes in an exhauſted receiver.

EXPERIMENT I.

AN oyſter being put into a very ſmall receiver, and kept in long enough to have ſucceſſively killed three or four birds or beaſts, &c. was not thereby killed, nor, for ought we could perceive, conſiderably diſturbed; only at each ſuck we perceived, that the air contained between the two ſhells broke out at their commiſſure; as we concluded from the foam, which at thoſe times came forth all round that commiſſure. About twenty four hours after, coming to ſee in what condition this oyſter was, I found, that both this, and another, that had been put at the ſame time into the receiver, were alive; but how long afterwards they continued ſo, I did not obſerve.

EXPERIMENT II.

THAT ſame day we put a pretty large craw-fiſh into a pretty large receiver, and found, that though he had been injured by a fall before he was brought thither, yet he ſeemed not to be much incommoded by being included, till the air was in great meaſure pumped out; and then its former motion preſently ceaſed, and he lay as dead; till, upon the letting in a little air into the receiver, he began forthwith to move afreſh. And upon the withdrawing the air again, he preſently, as before, became moveleſs. Having repeated this trial two or three times, we took him out of the receiver, where he appeared not to have ſuffered any harm.

EXPERIMENT III.

BUT I thought it not unlikely, that there may be ſome ſuch inequality in the ſtrength or vivacity of animals, as to ſuch kind of experiments as ours, that it might be well worth while in ſeveral caſes to re-iterate our trials. And on this occaſion, I ſhall here add, that having put an oyſter into a vial full of water, before we included it in the receiver, that through the liquor the motion of the bubbles, expected from the fiſh, might be the more pleaſantly ſeen and conſidered; this oyſter proved ſo ſtrong, as to keep itſelf cloſe ſhut, and repreſſed the eruption of the bubbles, that in the other did force open the ſhells from time to time; and kept in its own air as long as we had occaſion to continue the trial.

EXPERIMENT IV.

MOREOVER a craw-fiſh, that was thought more vigorous, being ſubſtituted in the place of the former craw-fiſh, though once he ſeemed to loſe his motion together with the air, yet afterwards he continued moving in the receiver, in ſpight of our pumping: whether, becauſe there was ſome unperceived leaking, that hindered a ſufficient exhauſtion of the air; or becauſe this particular animal was more ſtrong, or vivid, than the other, we could not poſitively determine.

TITLE VII.

Of the phænomena of a ſcale-fiſh in an exhauſted receiver.

THE following experiment is far from being the firſt, that was made on a ſcale fiſh in our vacuum; but in regard, that in the receivers, wherein thoſe trials were made, the external air could not be kept out near ſo long, and ſo well as in the veſſel I am about to mention; I judged it well worth the pains to obſerve, what would happen to a fiſh in an exhauſted veſſel, where it ſhould be kept for ſome hours together from all ſupply of freſh air. And therefore I made ſeveral trials to that purpoſe; whereof, that, which I think the moſt conſiderable, was regiſtered as follows:

WE took a receiver, ſhaped almoſt like a bolt-head, containing by eſtimation near a pint, and the globulous part of it being almoſt half full of water, we put into it, at the orifice (which was pretty large) a ſmall gudgeon, about three inches long, which, when it was in the water, ſwam nimbly up and down therein. Then having drawn out the air ſo well, that we gueſſed by a gage, that about nineteen parts of twenty, or more might be exhauſted, we ſecured ourſelves, that the regreſs of the air ſhould not injure our experiment; about which we obſerved theſe particulars.

FIRST, The neck of the glaſs being very long, though there appeared great ſtore of bubbles all about the fiſh; yet the reſt of the water, notwithſtanding the withdrawing of ſo much air, as has been mentioned, emitted no froth, and but few bubbles.

SECONDLY, The fiſh both at his mouth and gills did, for a great while, diſcharge ſuch a quantity of bubbles as appeared ſtrange, and for about half an hour or more (for much longer I had not opportunity to watch it;) when ever he reſted a-while, new bubbles would adhere to many parts of his body (as if they were generated there) eſpecially his fins and tail; ſo that he would appear almoſt beſet with bubbles; and if, being excited to ſwim, he was made to ſhake them off, he would quickly, upon a little reſt, be beſet with new ones as before.

THIRDLY, Almoſt all the while he would gape and move his gills, as before he was included; though towards the end of the time that I watched, it often happened, that he neither took in, nor emitted any aerial particles, that I could perceive.

FOURTHLY, After a while he lay almoſt conſtantly with his belly upwards, and yet would in that poſture ſwim briſkly as before.

FIFTHLY, Nay, after a while he ſeemed to be more lively than at firſt putting in: whether by reaſon, that by diſcharge of ſo many bubbles, which by their diſtenſion, perhaps put him to pain, he found himſelf relieved, or for ſome other cauſe, I examine not.

HAVING occaſion to go abroad, I returned about an hour and a half after he had been ſealed up, and found him almoſt free from bubbles, and with his belly upwards, and ſeeming ſomewhat tumid, but yet lively as before. But an hour and a quarter after that, when riſing from dinner, I went to look upon him again, he ſeemed to be moveleſs, and ſomewhat ſtiff; yet, upon ſhaking the glaſs, obſerving ſome faint ſigns of life in him by ſome languid motions, he attempted to make when excited to them, I opened the receiver under water, to try, if that liquor and air would recover him; and the external water ruſhing in, till it had filled the vacant part of the ball, and the greateſt part of the ſtem too, the fiſh ſunk to the bottom of it, with a greater appearance than ever of being alive; in which ſtate, after he had continued a pretty while, I made a ſhift, by the help of the water he ſwam in, to get him through the pipe into a baſon of water, where he gave more manifeſt ſigns of life; but yet for ſome hours lay on one ſide or other, without being able to ſwim, or lie on his belly, which appeared very much ſhrunk in, as if ſomething during the time of its being ſealed up had been broken in his body, or his belly had been exceedingly diſtended, beyond reſtitution to its former tone.

ALL the while he continued in the baſon of water, though he moved his gills as before he had been ſealed up, yet I could not perceive, that he did, even in his new water, emit, as formerly, any bubbles, though two or three times I held him by the tail in the air, and put him into the water again; where at length he grew able to lie conſtantly upon his belly, which yet retained much of its former lankneſs; and though it be now about, or above twenty four hours, ſince he was firſt included, he continues yet alive.

(POSTSCRIPT. He lived in the baſon eight or ten days longer; though divers gudgeons ſince taken died there in much fewer days.)

TITLE VIII.

Of two animals included, with large wounds in the abdomen, in the pneumatical receiver.

EXPERIMENT I.

Sept. 12. A SMALL bird, having the abdomen opened almoſt from flank to flank, without injuring the guts, was put into a ſmall receiver, and the pump being ſet to work, continued for ſome little time without giving any ſigns of diſtreſs; but at the end of about a minute and a half from the begining of the exhauſtion, ſhe began to have convulſive motions in the wings: and though the convulſions were not univerſal, or did appear violent, as is uſual in other birds, from whom the air is withdrawn by the engine, yet at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable; notwithſtanding which, we did not find any notable alteration in the lungs, and found the heart, or, at leaſt, the auricles of it, to be yet beating, and ſo it continued for a while after.

EXPERIMENT II.

Sept. 12. WE took alſo a pretty large frog, and having, without violating the lungs or the guts, made two ſuch inciſions in the abdomen, that the two curled bladders or lobes of the lungs came out almoſt totally at them, we ſuſpended the frog by the legs in a ſmall receiver; and after we had pumped out a good part of the air, the animal ſtruggled very much, and ſeemed to be much diſordered; and when the receiver was well exhauſted, ſhe lay ſtill for a while, as if ſhe had been dead, the abdomen and thigh very much ſwelled, as if ſome rarefied air, or vapour, forcibly diſtended them. But as, when the frog was put in, one of the lobes was almoſt full, and the other almoſt ſhrunk up; ſo they continued to appear, after the receiver had been exhauſted; but upon letting in of the air, not only the body ceaſed to be tumid, but the plump bladder appeared for a while, ſhrunk up as the other, and the receiver being removed, the frog preſently revived, and quickly began to fill the lobe with air.

TITLE IX.

Of the motion of the ſeparated heart of a cold animal in the exhauſted receiver.

WITHOUT diſcuſſing the opinions of learned men about the connection and dependency of the motions of the blood, and beating of the heart, I thought it might give me a ſufficient inducement to make the following experiment, that ſeveral ſorts of animals would be preſently killed in our vacuum by the withdrawing of the air; and even the inſects mentioned in the formerly publiſhed digreſſion about reſpiration, though they alſo were not totally deprived of life by the abſence of the air, yet they were of viſible motion: wherefore, ſome good hint or other being to be hoped for from the diſcovering, whether or no a ſeparated heart, which is but a part of an animal, would continue its motion in our vacuum; we made ſome trials to that purpoſe, whoſe ſucceſs I find thus ſet down.

EXPERIMENT I.

THE heart of an eel being taken out, and laid upon a plate of tin in a ſmall receiver, when we perceived it to beat there, as it had done in the open air, we exhauſted the veſſel, and ſaw, that, though the heart grew very tumid, and here and there ſent forth little bubbles, yet it continued to beat as manifeſtly as before, and ſeemed to do ſo more ſwiftly; as we tried by numbering the pulſations it made in a minute, whilſt it was in the exhauſted receiver and when we had re-admitted the air, and alſo when we took it out of the glaſs, and ſuffered it to continue its motion in the open air. The heart of another eel, being likewiſe taken out, continued to beat in the emptied receiver, as the other had done.

EXPERIMENT II.

THE heart of another eel, after having been included in a receiver, firſt exhauſted, and then accurately ſecured from leaking, though it appeared very tumid, continued to beat there an hour; after which, looking upon it, and finding its motion very languid, and almoſt ceaſed, by breathing a little upon that part of the glaſs, where the heart was, it quickly regained motion, which I obſerved a while; and an hour after, finding it to ſeem almoſt quite gone, I was able to renew it by the application of a little more warmth. At the end of the third hour, coming to look at it once more, a bubble, that appeared to be placed between the auricle and the heart, ſeemed to have now and then a little trembling motion; but I found it ſo faint, that I could no more by warmth excite it, ſo as plainly to perceive the heart to move: wherefore I ſuffered the outward air to ruſh in, but could not diſcern, that thereby the heart regained any ſenſible motion, though aſſiſted with the warmth of my breath and hands.

TITLE X.

A compariſon of the times, wherein animals may be killed by drowning, or withdrawing of the air.

To help myſelf and others to judge the better of ſome difficulties concerning reſpiration, I thought it might be uſeful, that we compared together the times, wherein animals may be killed by that want of reſpiration, which, in thoſe that are drowned, is cauſed by the water that ſuffocates them, and that other want, which proceeds from withdrawing the ambient air. Of the latter of theſe, a ſufficient number of inſtances is to be met with among our other experiments, and therefore, I ſhall now ſubjoin about the former the more trials, becauſe this compariſon hath not, that I know of, been yet thought on by any.

Experiment I.

Sept. 10. A green-finch, having his legs and wings tied to a weight, was gently let down into a glaſs-body filled with water; the time of its total immerſion being marked: at the end of half a minute after that time, the ſtrugglings of the bird ſeeming finiſhed, he was nimbly drawn up again, but found quite dead.

Experiment II.

Whereupon a ſparrow, that was very luſty and quarrelſome, was tied to the ſame weight, and let down after the ſame manner; but though he ſeemed to be under water more vigorous than the other bird, and continued ſtruggling almoſt to the very end of half a minute, from the time of his being totally immerſed, (during which ſtay under water, there aſcended, from time to time, pretty large bubbles from his mouth) yet notwithſtanding that as ſoon as ever the half minute was compleated, he was drawn up, we found him, to our wonder, irrecoverably gone.

Experiment III.

A ſmall mouſe, being held under water by the tail, emitted from time to time, divers aerial bubbles out of his mouth, and at laſt, as one of the ſpectators affirmed, he ſaw at one of his eyes: being taken out at the end of half a minute and ſome ſeconds, he yet retained ſome motions; but they proved but convulſions, which at laſt ended in death.

“By what is related under the firſt title, it “does not appear, that water-fowl, at leaſt, “that ducks could, in our receivers, endure the “want of air much longer than other birds: “but now to ſhew, that the contrivance of “nature is not inſignificant, as to the enabling “them to continue much longer under water, “without freſh air, than the land birds above-“mentioned, it will not be amiſs to ſubjoin “the two following experiments.”

Experiment IV.

We took the duck mentioned in the firſt title, and ſo tied a conſiderable weight of lead to her body, as it did not hinder her reſpiration, and yet would be ſure to keep her down under water; which we had found, that a ſmall weight would not do by reaſon of her ſtrength, nor yet a great weight, if tied only to her feet, in ſuch a middle-ſized tube as ours was, becauſe of the height of her neck and beak. With the above-mentioned clog, the duck was put into a tub full of clear water, under whoſe ſurface ſhe continued about a minute by my watch, quietly enough, but afterwards began to appear for a while much diſturbed; which fit being over, our not perceiving any motion in her made us, at the end of the ſecond minute, take her out of the water, to ſee in what condition ſhe was, and finding her in a good one, after we had allowed her ſome breathing time to recruit her ſelf with freſh air, we let her down again into the tub, which in the mean time had been filled with freſh water, leaſt the other, which had been troubled with the ſteams and foulneſs of the duck’s body, might either haſten her death by its being infected with them, or hinder our diſcerning what ſhould happen, by its being opacated by them.

The bird being thus under water, did after a while, begin, and from time to time continue, to emit divers bubbles at her beak. There alſo came out at her noſtrils, divers real bubbles from time to time; and when the animal had continued about two minutes, or better under water, ſhe began to ſtruggle very much, and to endeavour either to emerge, or change poſtures; the latter of which ſhe had liberty to do, but not the former. After four minutes, the bubbles came much more ſparingly from her: then alſo ſhe began to gape from time to time, (which we had not obſerved her to do before,) but without emitting bubbles; and ſo ſhe continued gaping until near the end of the ſixth minute, at which time all her motions, ſome of which were judged convulſive, and others, that had been excited by our rouzing her with a forceps, appeared to ceaſe, and her head to hang careleſly down, as if ſhe was quite dead. Notwithſtanding which, we thought fit for greater ſecurity, to continue her under water a full minute longer, and then finding no ſigns of life, we took her out, and being hung by the heels, and gently preſſed in convenient places, ſhe was made to void a pretty quantity of water, of which, whether any had been received into the lungs themſelves, we had not time and opportunity to examine. But all the means, that were to recover the bird to life, proving ineffectual, we concluded, ſhe had been dead a full minute before we removed her out of the water: ſo that, to ſum up the event of our experiment, even this water bird was not able to live in cold water, without taking in freſh air, above ſix minutes; which is but $\frac{1}{10}$ of an hour.

EXPERIMENT V.

THE duckling mentioned in the firſt title, and ſecond experiment, having a competent weight tied to her legs, was let down into a tub of water, which reached not above an inch or two higher than her beak: during the moſt part of her continuance, there came out ſtore of bubbles at her noſtrils; but there ſeemed to come out more and greater from a certain place in her head, almoſt equidiſtant from her eyes, but ſomewhat leſs remote from her neck than they. Whilſt ſhe was kept in this condition, ſhe ſeemed frequently to endeavour to dive lower under the water, and after much ſtruggling, and frequent gaping, ſhe had divers convulſive motions, and then let her head fall down backward, with her throat upwards. To which moveleſs poſture ſhe was reduced at the end of the third minute, if not a little ſooner; but a while after there appeared a manifeſt, but tremultous motion in the two parts of her bill, which continued for ſome time, but afforded no circumſtances, whereby we could be ſure, that they were not convulſive motions: but theſe alſo ceaſing upon the end of the fourth minute, the bird was taken out and found irrecoverable.

EXPERIMENT VI.

A viper, that was kept ſo many hours in an exhauſted receiver, till it was concluded to be ſtark dead, and to have been ſo for a good while, was nevertheleſs reſolutely hindered by me from being thrown away, till I had tried, what could be done by keeping it all night in a glaſs-body upon a warm digeſtive furnace. Whereupon this viper was found the next morning, not only to be revived, but to be very lively, ſo as to invite me to make with her, without ſeeking for another, the following experiment.

WE put her into a tall glaſs-body, fitted with a cork to the orifice of it, and depreſſed with weight, ſo that ſhe could come at no air. In this caſe we obſerved her from time to time; and after ſhe had been ducked a-while, ſhe lay with very little motion for a conſiderable ſpace of time. At an hour and a quarter ſhe often put out her black tongue: at near four hours ſhe appeared much alive, and, as I remember, about that time alſo put out her tongue, ſwimming all this while, as far as we obſerved, above the bottom of the water. At the end of about ſeven hours or more, ſhe ſeemed yet to have ſome life in her, her poſture being manifeſtly changed in the glaſs, from what it was a-while before; unleſs that might proceed from ſome difference made in her body, as to gravity and levity. Not long after, ſhe appeared quite dead, her head and tail hanging down moveleſly, and directly towards the bottom of the veſſel, whilſt the middle of the body floated as much as the above-mentioned cork would permit it.

HASTE maketh me pretermit the mention of divers things ſuggeſted by what hath been delivered upon the preſent title. But this one thing would be taken notice of, that, though ſome of the above-mentioned animals ſeem, by the relations we have given of them, to have been a little ſooner deſtroyed by drowning, than any we have mentioned were by our engine, that is no ſure proof, that ſuffocation does kill animals faſter than the deprivation of air, they are expoſed to in our engine. For in drowning, that which deſtroys is applied to its full vigour at the firſt, and all at once; whereas, our receivers being made for ſeveral purpoſes, the deprivation of the air, that they make, cannot be made all at once, but the air muſt be pumped out by degrees; ſo that till the laſt the receiver will be but partly emptied. For confirmation of which, I have this to allege, that, having in the preſence of ſome virtuoſi provided for the nonce a very ſmall receiver, wherein yet a mouſe could live ſometime, if the air were left in it, we were able to evacuate it at one ſuck, and by that advantage we were enabled, to the wonder of the beholders, to kill the animal in leſs than half a minute.

THE CONTINUATION OF THE EXPERIMENTS CONCERNING RESPIRATION.

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A PREFACE concerning theſe EXPERIMENTS.

THOUGH, to ſhun prolixity, the preface, which the author had made to all he wrote about reſpiration, have been purpoſely omitted; yet there are ſome few points ſo neceſſary to be taken notice of, that it is thought unfit to leave them wholly untouched. For, the following experiments being not at firſt written for the preſs, and thrown by for many years, till they were very haſtily gathered together, and in ſome places ſupplied with others, little leſs haſtily annexed, to make ſome neceſſary ſupplies, the reader muſt not expect in ſuch a caſual tract, (which the author confeſſes to be one of the moſt imperfect, and immethodical of all his compoſures) any thing but novelty and truth, and an earneſt deſire to be ſerviceable in an inquiry ſo important to mankind, to the curious in general, and eſpecially to phyſicians, who, by the encouraging mention they have made of his former endeavours in this kind, have invited him to add theſe many new experiments to thoſe few, they had hitherto exerciſed their wits upon; and, to leave them the more freedom to do ſo, he purpoſely forbore to confirm, or confute any hypotheſis, or ſo much as propoſe any of his own; declaring it to be his aim, not to eſpouſe, or make a party, but to communicate to the curious ſome matters of fact, that are new; and in an hiſtorical way impartially delivered. No more of preface is now to be added, but that it is thought fit, for prevention of ambiguity, to give this advertiſement, touching the ground of the title of *Vacuum Boylianum*, to be met with in theſe experiments; that, as learned men, both Engliſh and foreigners, in their writings, have familiarly, for diſtinction-ſake, employed the titles of *Machina Boyliana*, and *Experimenta Boyliana*; ſo the author, that writ theſe, for the moſt part in haſte, and for his own memory, did for diſpatch-ſake, call the abſence of the air, procured in his receivers, our vacuum; whence by analogy was framed the *Vacuum Boylianum*, which he therefore thinks the leſs improper, becauſe, to call it vacuum abſolutely, would be judged by many a declaring himſelf a vacuiſt, who does not yet own the being either of their opinion, or a downright pleniſt; or elſe he muſt be troubleſome to the reader and himſelf, by frequently explaining, what ſort of vacuum he underſtands; whereas he declares once for all, that by the *Vacuum Boylianum*, he means ſuch a vacuity or abſence of common air, as is wont to be effected or produced in the operations of the *Machina Boyliana*.

TITLE XI.

Of the accidents, that happened to animals in air, brought to a conſiderable degree, but not near the utmoſt one, of rarefaction.

IN the generality of our pneumatical experiments upon animals, it ſuited with our purpoſes, to rarefy the air as much, and for the moſt part as faſt, as we could: but I had other trials in deſign, wherein an extraordinary degree of rarefaction, but yet not near the higheſt, to which the air might be brought by our engine, ſeemed likelieſt to conduce to my inquiries, and particularly ſeemed hopeful to afford ſome light, in reference to thoſe diſeaſes and diſtempers, that are thought primarily to affect the reſpiratory organs; or, to depend upon ſomething amiſs in reſpiration.

WHEREFORE having gages, by the help of which ſuch experiments might be much better performed, than elſe they could, I attempted ſeveral of them; ſome of whoſe ſucceſſes I find in the following memorials.

EXPERIMENT I.

Aug. 16. A linnet being put into a receiver, capable to hold about four half pints of water, the glaſs was well cloſed with cement and a cover, but none of the air was drawn out with the engine, or otherwiſe. And though no new air was let in, nor any change made in the impriſoned air; yet the bird continued there three hours, without any apparent approach to death; and though it ſeemed ſomewhat ſick, yet being afterwards taken out, it recovered, and lived ſeveral hours.

EXPERIMENT II.

Aug. 18. FROM the above-mentioned receiver about half the air was drawn out, a linnet being then in the glaſs, and in that rarified air, (which appeared by a gage to continue in that ſtate) the bird lived an hour and near a quarter, before it ſeemed in danger of death; after which, the air being let in without taking off the receiver, ſhe manifeſtly recovered, and leaped againſt the ſide of the glaſs: being taken out into the open air, ſhe flew out of my hand to a pretty diſtance.

EXPERIMENT III.

Sept. 9. WE conveyed into a receiver, capable to hold four half pints of water, a lark, together with the gage, by the help whereof we pumped out of the receiver, three quarters of the air, that was in it before: then heedfully obſerving the bird, we perceived it to pant very much, ſo that a learned phyſician (from whom I yet diſſented) judged thoſe beatings to be convulſive: having continued thus for a little above a minute and a half, the bird fell into a true convulſive motion, that caſt it upon the back. And although we made great haſte to let in the air; yet, before the expiration of the ſecond minute, and conſequently, in leſs than half a minute from the time immediately preceding the convulſion, the lark was gone paſt all recovery, though divers means were uſed to effect it.

EXPERIMENT IV.

Sept. 9. PRESENTLY after we put into the ſame receiver a green-finch, and having withdrawn the air, until it appeared by the gage there remained but half, we preſently began to obſerve the bird, and took notice, that, within a minute after, ſhe appeared to be very ſick, and ſhaking her head, threw againſt the inſide of the glaſs a certain ſubſtance, which I took to be vomit, and which afterwards appeared ſo: upon this evacuation the bird ſeemed to recover, and continue pretty well (but not without panting) until about the end of the fourth minute, at which growing very ſick, ſhe vomited again (ſhaking her head as at firſt,) but much more unqueſtionable than before, and ſoon after, eat up again a little of her vomit; at which time (whether that contributed to her recovery or no) ſhe very much recovered. And though ſhe had in all three fits of vomiting, yet for the laſt ſeven or eight minutes, that we kept her in the receiver, ſhe ſeemed to be much more lively than was expected; which may in part be attributed to a little air, that by accident got in, though it were immediately pumped out again. At the end of a full quarter of an hour from the firſt exhauſtion of the receiver, the bird appearing not likely to die in a great while, and the engine being needed for other uſes, we took out the bird, and thereby put a period to the experiment.

EXPERIMENT V.

I now thought it fit to try, whether, though a viper would not hold out very many hours in air, brought to as high a rarefaction, as we could bring it by our engine, yet to that cold and vivacious animal, a very ſmall proportion of air, in compariſon of what was neceſſary to hot animals, would not ſuffice to keep it alive for a conſiderable time: the narration of the experiment I find regiſtered as follows.

A viper lately bought of the perſon, that at this ſeaſon uſes to take new ones, almoſt from day to day, was included together with a gage in a portable receiver, capable to hold about three pints and half of water. This veſſel being exhauſted, and ſecured againſt the regreſs of the air, the impriſoned animal was obſerved from time to time; and obſerved not only to be alive, but nimbly to put out, and to draw back, its tongue about 36 hours after it was ſhut up; for which reaſon we continued the veſſel longer in the ſame ſhady place; where at the end of 60 hours, looking upon her, as I was going to bed, ſhe appeared very dull and faint, and not likely to live much longer: and the next morning being by ſome occaſions carried abroad, and coming to look upon the glaſs preſently after dinner, I found her ſtark dead, with her mouth opened to a ſtrange wideneſs; wherefore ſuffering water to be impelled by the outward air into the cavity of the receiver, to obſerve how far that veſſel was then emptied of air, we found by the water, that was driven in, and afterwards poured out again, and meaſured, that 4 parts of 5, or rather 5 of 6 of the veſſeled air (if I may ſo call that, which was ſhut up in the receiver) had been pumped out; ſo that in an air ſo rarified as to expand itſelf to 5 or 6 times its former and uſual dimenſions, our viper was able to live 60 hours, that we are ſure of, and perhaps might a pretty while longer. April 12.

A digreſſive experiment concerning reſpiration upon very high mountains.

TO illuſtrate what I have taken notice of in the printed experiments about the unfitneſs for reſpiration, obſerved by the learned *Acoſta* in the high mountains of *Pariacaca*, I ſhall here add, what I have had the curioſity and occaſion to learn from divers travellers, whom I purpoſely conſulted about theſe matters; whereof you will eaſily believe, that not many of them have had opportunity to give accounts. Meeting with an eccleſiaſtical perſon, that had viſited thoſe high mountains of *Armenia*, (on one of which, becauſe of their height, the tradition of the natives will needs have the ark to have reſted;) I aſked him, whether thoſe mountains are as really ſo high as is given out, and whether at the top of that he viſited, he found any difficulty of breathing. To the firſt part of which queſtion he anſwered; that they were really exceeding high (which he might well judge of, having been upon ſome of the moſt famous both in *Europe*, *Aſia* and *Africa*;) and that he could not come to the top, becauſe of the unpaſſable ſnows: and to the ſecond part he replied, that whilſt he was in the upper part of the mountain, he plainly perceived, that he was reduced to fetch his breath much oftener than he was wont; and than he did before he aſcended the hill, and after he came down from it. And upon my inquiring, whether or no that difficulty of breathing might not be accidental, or peculiar to him, he told me, that he himſelf having expreſſed ſome wonder to find himſelf ſo ſhort-winded, the people told him, that it was no more than happened to them, when they were ſo high above the plain; it being a common obſervation among them. And I was the more inclined, both to make inquiry about theſe matters, and to believe what he ſaid, becauſe what he related of their being covered with ſnow, and of an odd temperature of air, I had learned before from a traveller of another nation than this perſon, and a ſtranger to him.

The ſame churchman, being aſked by me, whether he had not in ſome part of *Europe* made the like obſervation (of the difficulty of breathing) told me, that he had done it upon the top of a mountain in the country of *Cevennes*, in or near the province of *Languedoc*; which may ſerve to confirm what I am about to relate from the mouth of a learned traveller, that was upon the top of one of the Pyreneans, that is not very remote from the mountains we ſpeak of.

This gentleman, who was a perſon curious and intelligent, being brother-in-law to one of the chief lords of thoſe parts, was by him invited, about the beginning of September, to viſit a neighbouring mountain, that is at leaſt one of the higheſt of the Pyreneans, which is commonly called *Pic de Midi*, upon whoſe top, where a tent was ſpread for them, they ſtayed many hours. His anſwers to the other queſtions I aſked him, are elſewhere related; all that concerns this place being, that I find this ſet down among my adverſaria; viz. I alſo inquired of him, whether they found the air at the top as fit for reſpiration as common air, which he told me they did not, but were fain to breath ſhorter, and oftener than uſual; and becauſe I ſuſpected, that might come from their motion, I aſked, whether they obſerved it to ceaſe, when they came down to the bottom of the hill, which he told me they plainly did; beſides that they ſtayed many hours at the top, too long to continue out of breath.

But that I may not here conceal any thing, that may conduce to the diſcovery of the truth in the matter under conſideration, I ſhall here add, that I did ſometimes think it worth further inquiry, whether the ſickneſs, if not alſo the difficulty of breathing, that ſome have been obnoxious to, in the uppermoſt parts of Pariacacha, and perhaps, ſome other high mountains, may not be imputed, not ſo preciſely to the thinneſs and rarity of the air, in places ſo remote from the lowermoſt part of the atmoſphere, as to include certain ſteams of a peculiar nature; which, in ſome places, the air may be imbued with? In favour of which ſuſpicion, I remember, that inquiring once of an intelligent man, who had lived ſeveral years in the Iſland of *Teneriff*, whether he had been at the top of the *Pic* of that name, and what he had there taken notice of about the air? he anſwered me, that he had attempted to go up to the top of the mountain, but, that though ſome of the company were able to do ſo, he and ſome others, before they had reached ſo high, grew ſo ſick upon the operation they felt of the ſharp air, and ſulphurous exhalations which infected it, that they were fain to ſtay behind their companions, he having already found this effect of thoſe piercing ſteams upon his face, which, when he made me this relation, was of a fair complexion, that the ſkin began to be of a pale yellow, and even his hair to be diſcoloured.

TITLE XII.

Of the obſervations produced in an animal, in changes as to rarity and denſity made in the ſelf-ſame air.

In the experiments hitherto recited, the animals, that were recovered from a gaſping condition, have been ſo, by letting in freſh air upon them, and not the ſame, that had been withdrawn from them. Wherefore I thought it very requiſite to try, whether the ſame portion of air, without being renewed, would, by being expanded much beyond its uſual degree, and reduced to it, ſerve to bring an animal to death's door, and revive him again; ſince by the ſucceſs of ſuch a trial it would notably appear, that the bare change of the conſiſtence of the air, as to rarity and denſity, may ſuffice to produce the abovementioned effect.

But to deviſe a way to put this experiment in practice appeared no eaſy matter; ſince it required a receiver, that ſhould be tranſparent, and be capable of changing its bulk, without ſuffering any air to get in or out.

To ſurmount theſe difficulties, the firſt thing I thought on was, to take a fine limber and clear bladder of a ſheep or hog, made more tranſparent by being anointed with oil, which was done on the outſide, that the ſmell of it might leſs offend the animal to be included. Then we clipped off as much of the bladder at the neck, as was judged abſolutely neceſſary to make an orifice capable of letting in a mouſe; that ſort of animals being, by reaſon of their ſmalneſs, the fitteſt of thoſe furniſhed with lungs and hot blood, we could procure. And whereas it ſeemed very difficult, when the neck of the bladder was cut off, to make up ſo large an orifice without wrinkles, at which the rarified air may eſcape; to obviate this inconvenience, we provided a round ſtick ſomewhat leſs than the orifice, that, the wood being laid over with a cloſe and yielding cement, (for pitch, or the like common ſtuff will not always ſerve the turn) we might be able to tie the bladder faſt and cloſe enough upon the thus fitted ſtopple.

AND now to reduce theſe things to practice, and by their help make our deſigned experiment, we included a mouſe into a receiver made according to this way, leaving in the bladder as much air, as we thought might ſuffice him for a long time, as the experiment was to laſt. Then putting this limber or extenſible receiver, if I may ſo call it, into an ordinary one of glaſs, and placing this engine near a window, that we may ſee thro' both of them; the air was by degrees pumped out of the external receiver, (as for diſtinction ſake I ſhall call it) and thereupon the air included in the bladder did proportionably expand itſelf, and ſo diſtend the external receiver, till being arrived at a degree of rarefaction, which rendered it unfit for the included mouſe's reſpiration, I perceived, though with ſome difficulty, in this animal, the ſigns of his being in great danger of ſudden death. Whereupon the outward air being haſtily let into the external receiver, compreſſed the ſwelled bladder to its former dimenſions, and thereby the included air to its former denſity, by which means the fainting mouſe was quickly revived. Having given him ſome convenient time of reſpite, the experiment was reiterated with the like ſucceſs, and we doubted not, but the third trial we made would have ended as the two former did; but that, whilſt we were conſidering of the ſickneſs of the mouſe, which, by reaſon of ſome opacity that could ſcarce be avoided in the wrinkled bladder, was not as to its degree ſo eaſily taken notice of, it grew irrecoverable by the ſubſequent condenſation of the air.

N.B. The confirmation of this by further experiments will properly fall under another title.

TITLE XIII.

Of an unſucceſsful attempt to prevent the neceſſity of reſpiration by the production, or growth, of animals in our vacuum.

HAVING had frequent occaſions to obſerve, how quickly thoſe animals, whoſe blood is actually warm, did expire in our vacuum; and that even thoſe animals, with lungs, whoſe blood was actually cold, were not able to live any conſiderable time there; I thought it very well worth while, and yet extremely difficult, to try, whether there might not be ſome ways yet unpractiſed, either to make ſuch animals as nature endows with lungs, live without reſpiration, or at leaſt, to bring ſuch inſects, and other animals, as can already live without air, to move alſo without it in our vacuum.

THEREFORE conſidering with my ſelf what happens to infants, and other young animals, in the womb, and even after they come from thence, if they continue to be wrapped up in the ſecundines; though as ſoon as they are brought into the free air, they may be preſently killed by being kept from breathing: conſidering alſo, what I elſewhere relate of the ſlow expiration of a very young kitling in our vacuum, together with the long want of reſpiration, which cuſtom enables ſome divers to endure: conſidering theſe things, I ſay, though I know, that ſomewhat may be objected to ſhew, that theſe inſtances are not altogether full to my purpoſe; yet they, among other things, invited me to think, that the leaſt unlikely projects, that occurred to my barren invention, would be theſe that follow.

FIRST, I thought fit to try, whether the ſeeds of reſpiring animals might be either hatched, or otherwiſe brought to produce young ones, in our vacuum. For, if that could be compaſſed, I ſhould obtain my end.

NEXT, in caſe of my failing in the former attempt, and that, which is to be after a few lines propoſed, I thought fit to try, whether at leaſt I could not bring the eggs of inſects to hatch or be animated, or aurelias, as they call them, that were already alive, turn (according to the courſe of nature) into winged inſects, as flies, or butter-flies: of which trials, and thoſe of the former ſort, the account properly belongs to another place, where I relate the ſucceſs of theſe and other attempts to produce plants and animals in our vacuum.

BUT thirdly, conſidering, that nature has ſo ordered it, that frogs, though when they are grown big enough to deſerve that name, they be amphibious animals, endowed with lungs; yet before they attain to that pitch, they live wholly in the water like fiſhes; I thought it the moſt expeditious, and leaſt improbable attempt we could make, to try, whether or no this animal, being as a fiſh brought to live, either in our vacuum, or at leaſt in highly rarefied air, would not continue to do ſo, after its lungs ſhould be perfectly formed. Wherefore, though I foreſaw, and foretold the difficulty, that would be met with in the proſecution of this experiment, namely, that the aerial bubbles, that would be diſcloſed in ſuch ſoft bodies upon the withdrawing of the preſſure of the ambient, would ſo violate the ſlight texture of thoſe tender animals, as to hinder them from living long, or moving freely; yet I thought it very fit to attempt the trial, whereof I find this account among my adverſaria.

EXPERIMENT I.

WE took a good company of tadpoles, and put them with a convenient quantity of water into a portable receiver of a round figure, and obſerved, that at the firſt exſuction of the air they did riſe to the top of the water, though moſt of them ſubſided again, till the next exſuction raiſed them. They ſeemed by their active and wrigling motion to be very diſcompoſed. The receiver being exhauſted, they continued reſtleſs, moving all of them in the top of the water; and though ſome of them ſeemed to endeavour to go to the bottom, and dived ſome part of the way, eſpecially with their heads, yet they were immediately buoyed up again. Within an hour or little more, they were all moveleſs, and lay floating on the water: wherefore I opened the receiver, upon which the air ruſhed in, and almoſt all of them (which were many) preſently ſunk to the bottom, but none of them recovered to life.

EXPERIMENT II.

A little after theſe, we included a leſſer number of tadpoles in a ſmaller glaſs, which was alſo exhauſted with the like circumſtances with the former. And when I found the other tadpoles to be dead, I haſted to theſe, which did not, except perhaps one, give any ſign of life, but upon letting in the air, theſe having not been long kept from it, ſome few of them did recover, and ſwam up and down lively enough for ſome time; though after a while they alſo died.

EXPERIMENT III.

SOME years after I repeated the ſame experiment in a portable receiver of a convenient kind; and though after the exhauſtion was perfected, the tadpoles did for a while move briſkly enough on the top of the water, none of them appearing able to dive or ſwim under water, yet coming to look on them at the end of an hour, they ſeemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the air upon them, yet all the effect of it was, that the moſt of them immediately ſunk to the bottom, as the reſt of them did a very little while after; none of them, that I could obſerve, recovering any vital motion.

EXPERIMENT IV.

THERE remains an experiment, which I often judged as well more hopeful as more noble, if I could procure an opportunity to bring my deſign to a trial, which I have found it very difficult to do; nevertheleſs I was able to do it once, though not fully as I deſired, yet not altogether without ſucceſs.

WE procured then, and with much ado, ſome of thoſe odd inſects, which I elſewhere deſcribe, whereof gnats have by ſome ingenious men been obſerved to be generated about the end of Auguſt, or beginning of September. Theſe for ſome weeks live all together in the water, as tadpoles do, ſwimming up and down therein, till they are ripe for a tranſmigration into flies: which it ſelf is ſo great a rarity in nature, as makes theſe little creatures recompence to our curioſity the trouble, they often give our faces and hands. Suppoſing then, that if I could get ſome of theſe, and include them, being of thoſe inſects they call aquatilia, and ſo minute as they are, they may live a great while in the receiver without air, and in the mean while attain the period, which, according to nature's courſe, is wont to turn them into flies, which might come forth winged creatures into a medium not furniſhed with common air, as others of their kind enjoy; ſuppoſing, I ſay, that theſe inſects would afford me ſome information about theſe particulars, having upon much watching met with four or five of them after a ſhower of rain, that dropped from a houſe into a veſſel laid on purpoſe for it, we included them with ſome of their water into a ſmall glaſs receiver, which being very exactly cloſed, we kept in a ſouth-window, where theſe little creatures continued to ſwim up and down for ſome few days, without ſeeming to be much incommodated by ſo unuſual an habitation; and at the end of that time, and much about the ſame day, they diveſted the habit they had, whilſt they lived as fiſhes, and appeared with their exuviæ or caſt-coats under their feet, ſhewing themſelves to be perfect gnats, that ſtood without ſinking upon the ſurface of the water, and diſcovered themſelves to be alive by their motion, when they were excited to it: but I could not perceive them to fly in that thin medium; to which inability whether the viſcoſity of the water might contribute, I know not, though they lived a pretty while, till hunger or cold deſtroyed them. Something in this experiment may deſerve ſerious reflections; which I cannot ſpare time to offer at.

A digreſſive experiment, concerning the expanſion of blood, and other animal juices.

FOR ſome purpoſes, relating partly to reſpiration, and partly to other enquiries, I thought fit to endeavour to obtain, what information could be procured, of the conſiſtence and diſpoſition to expand itſelf of blood, and other animal liquors; in purſuance of which the enſuing trials, among others, were undertaken.

THE warm blood of a lamb or a ſheep, being taken as it was haſtily brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was in a wide mouthed glaſs put into a receiver, made ready for it; and the pump being early ſet on work, the air was diligently drawn out; but the operation was not always, eſpecially at firſt, ſo early manifeſt, as the ſpirituouſneſs of the liquor made ſome expect: yet this hindered not, but, after a long expectation, the more ſubtle parts of the blood would begin to force their way through the more clammy ones, and ſeem to boil in large cluſters, ſome as big as great beans or nutmegs; and ſometimes, to the wonder of the by-ſtanding phyſicians, the blood was ſo volatile, and the expanſion ſo vehement, that it boiled over the containing glaſs; of which, when it was put in, it did not, by our eſtimate, fill above a quarter. Having alſo included ſome milk warm from the cow, in a cylindrical veſſel of about four or five inches high, though the operator were induced to pump a great while before any intumeſcence appeared in the milk, yet afterwards, when the external air was fully withdrawn, the white liquor began to boil in a way, that was not ſo eaſy to deſcribe, as pleaſant to behold: and this it did for a pretty while with ſo much impetuoſity, that it threw up ſeveral parts of itſelf out of the wide mouthed glaſs, that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A yet greater diſpoſition to intumeſcence, we thought, we obſerved in the gall, which was but ſuitable to the viſcoſity of the texture.

Note, that the two foregoing experiments were made with an eye caſt upon the enquiry, that I thought might be made; whether, and how far the deſtructive operation of our engine upon the included animal, might be imputed to this, that upon the withdrawing of the air, beſides the removal of what the air's preſence contributes to life; the little bubbles generated upon the abſence of the air in the blood, juices, and ſoft parts of the body, may by their vaſt number, and there conſpiring diſtenſion, variouſly ſtrengthen in ſome places, and ſtretch in others, the veſſels, eſpecially the ſmaller ones, that convey the blood and nouriſhment; and ſo by choaking up ſome paſſages, and vitiating the figure of others, diſturb or hinder the due circulation of the blood? not to mention the pains, that ſuch diſtenſions may cauſe in ſome nerves, and membranous parts, which by irritating ſome of them into convulſions, may haſten the death of animals, and deſtroy them ſooner, by occaſion of that irritation, than they would be deſtroyed by the bare abſence or loſs of what the air is neceſſary to ſupply them with. And to ſhew, how this production of bubbles reaches, even to very minute parts of the body, I ſhall add on this occaſion, hoping, that I have not prevented myſelf or any other, what may ſeem ſomewhat ſtrange, what I once obſerved in a viper, furiouſly tortured in our exhauſted receiver, namely, that it had manifeſtly a conſpicuous bubble moving to and fro in the wateriſh humour of one of its eyes.

Another digreſſive experiment belonging to the ſame title.

To ſhew, that not only the blood and liquors, but alſo the other ſoft parts, even in cold animals, have aerial particles latitant in them; we took the livers and heart of an eel, as alſo the head and body of another fiſh of the ſame kind, cut aſunder croſs ways ſomewhat beneath the heart; and putting them into a receiver, upon the withdrawing of the air we perceived, that the liver did manifeſtly ſwell every way, and that both the upper and lower parts did ſo likewiſe; and at the place where the diviſion had been made, there came out in each portion of the fiſh divers bubbles, ſeveral of which ſeemed to come from the *medulla ſpinalis*, or the cavity of the back-bone, or the adjoining parts; and the external air being let in both the portions of the eel preſently ſhrunk, ſome of the ſkins ſeeming to be grown empty or flaccid in each of them.

TITLE XIV.

Of the power of aſſuefaction to enable animals to hold out in air, by rarefaction made unfit for reſpiration.

"THE power of aſſuefaction in other "caſes, made me think it very well "worth trying, what it would do in reſpiration; "and the rather, becauſe I preſumed, it might "prove an experiment of good uſe, if we "ſhould diſcover, that by a gradual accuſtomance an animal may be brought to live, "either in a much thinner air, or much longer in the ſame air, than at firſt he could. "But in regard, that to make ſuch a trial perſpicuouſly enough, the opacity of the bladder made uſe of in the former title was like "to be an impediment, I deviſed another way "to obviate that inconvenience, which may, I "hope, be competently underſtood, by the "heedful peruſal of the following trials."

EXPERIMENT I.

We included in a round vial with a wide neck, (the whole glaſs being capable of containing about 8 ounces of water) a young and ſmall mouſe, and then tied ſtrongly upon the upper part of the glaſs's neck a fine thin bladder, out of which the air had been carefully expreſſed, and then conveyed this phantaſtical veſſel into a middle ſized receiver, in which we alſo placed a mercurial gage (adjuſted by our elſewhere mentioned ſtandard;) this done, the air was by degrees pumped out, until it appeared by the gage, that there remained but a fourth part in the external receiver (as for diſtinction ſake I call it) whereupon the air in the internal receiver expanding itſelf, appeared to have blown the bladder almoſt half full, and the mouſe ſeeming very ill at eaſe by his leaping, and otherwiſe endeavouring to paſs out at the neck of his uneaſy priſon; we did, for fear the over thin air would diſpatch him, let the air flow into the external receiver, whereby the bladder being compreſſed, and the air in the vial reduced to its former denſity, the little animal quickly recovered.

EXPERIMENT II.

A while after, without removing the bladder, the experiment was repeated, and the air, by the help of the gage was reduced to its former degree of rarefaction, and the mouſe, after ſome fruitleſs endeavours to get out of the glaſs, was kept in that thin air for full 4 minutes; at the end of which he appeared ſo ſick, that, to prevent his dying immediately, we removed the external, and took out the internal receiver: whereupon, though he recovered, yet it was not without much difficulty, being unable to ſtand any longer upon his feet, and for a great while after continued manifeſtly trembling.

EXPERIMENT III.

But having ſuffered him to reſt a reaſonable ſpace of time, preſuming, that aſſuefaction had accuſtomed him to greater hardſhips; we conveyed him again into the external receiver, and having brought the air to the former degree of expanſion, we were able to keep him there for a full quarter of an hour; though the external receiver did not at all conſiderably leak; as appeared, both by the mercurial gage, and by the continuing diſtenſion of the bladder. And it is worth noting, that, till near the latter end of the quarter of an hour, not only the animal did ſcarce at all appear diſtreſſed, remaining ſtill very quiet; but, which is more, whereas when he was put in, the trembling formerly mentioned were yet upon him, and continued ſo for ſome time; yet afterwards, in ſpite of the expanſion of the air he was then in, they left him early enough. And when the internal receiver was taken out, he did not only recover from his fainting fit ſooner than before; but eſcaped thoſe ſubſequent tremblings we have mentioned.

EXPERIMENT IV.

ENCOURAGED by this ſucceſs, after we allowed him ſome time to recollect his ſtrength, we reconveyed him and the odd veſſel, wherein he was included, into the former receiver, and pumped out the air, till the mercury in the gage was not only drawn down as low as formerly; but near half an inch lower, that there the air might be yet further expanded, than hitherto it had been. And though this did at firſt ſeem to diſcompoſe our little beaſt; yet after a-while he grew very quiet, and continued ſo for a full quarter of an hour: when being deſirous to try what operation a farther rarefaction of the air would have upon him, we cauſed three exſuctions more to be made by the pump, before we diſcovered him to be in manifeſt danger (at which time the bladder appeared much fuller than before) but then we were obliged to let the air into the outward receiver; whereupon the mouſe was more ſpeedily revived than one would have ſuſpected.

AND theſe trials of the power of aſſuefaction ſeemed the more conſiderable, becauſe the air, in which the mouſe had all this while lived, had been clogged and infected with the excrementitious effluviums of his body; for it was the ſame all along, we having purpoſely forborn to take off the bladder, whoſe regular intumeſcencies and ſhrinkings ſufficiently manifeſted, that the veſſel, whereof it was a part, did not leak.

POSTSCRIPT.

“THOUGH the ſucceſs of the recited experiments is very promiſing; yet a ſubſequent trial or two, whoſe particularities are ſlipped out of my memory, oblige me in point of candour, to declare, that, for further ſatisfaction, the trials of the power of accuſtomance, in reference to air unfit for reſpiration, ought to be both reiterated, and to be made in differing ſorts of animals.”

TITLE XV.

Some experiments ſhewing, that air, become unfit for reſpiration, may retain its wonted preſſure.

EXPERIMENT I.

WE took a mouſe of an ordinary ſize, having (not without ſome difficulty) conveyed him into an oval glaſs, fitted with a ſomewhat long and conſiderably broad neck, which we had provided, that it might be wide enough to admit a mouſe in ſpight of his ſtruggling. We conveyed in after him a mercurial gage, in which we had diligently obſerved, and marked the ſtation of the mercury, and which was ſo faſtened to a wire, reaching to the bottom of the oval glaſs, that the gage, remaining in the neck, was not in danger to be broken by the motions of the mouſe in the oval part: the upper part of the long neck of the glaſs was, notwithſtanding the wideneſs of it, hermetically ſealed by the help of a lamp and a pair of bellows, that we might be ſure, that the impriſoned animal ſhould breath no other air, than that which filled the receiver at the time when it was nipped up. This done, the mouſe was watched from time to time; and though by reaſon of the largeneſs of the veſſel, in compariſon of ſo ſmall an animal, he ſeemed to me rather drooping, than very near death at the end of the ſecond hour; yet coming to look upon him about half an hour after, he was judged by the ſpectators quite dead, notwithſtanding our ſhaking of the veſſel to rouze him up. This made me caſt my eyes upon the gage, wherein I could not perceive any ſenſible change of the mercury's ſtation. But being unwilling to give over the mouſe, without trying what freſh air would do to recover him, I cauſed the ſealed part of the glaſs to be broken off, and, notwithſtanding that his continuing to appear dead increaſed the confidence of thoſe that thought him ſo, I obtained after a while ſome faint tokens of life; though I am not ſure, that they would have continued in a veſſel, where the air was ſo clogged and infected, if it had not been, that freſh air was frequently blown in by a pair of bellows, whoſe noſe was inſerted into the neck of the glaſs. This freſh air ſeemed evidently, though but ſlowly, to revive the gaſping animal, whom I would not, nor could not, conveniently take out of the glaſs, till he had gained ſtrength enough to make uſe of its legs; after which, without breaking of the glaſs (which I was loath to looſe, having then no other of the kind) we took him out, and found him quickly able to go up and down. After which ſervice, and another trial we had with him, which belongs not to this place, we ſet him at liberty to ſhift for himſelf.

EXPERIMENT II.

SUCH an experiment as the former we made with like ſucceſs upon a ſmall bird, included with a gage in a receiver, holding about a quart of water. The bird in about half an hour appeared to be ſick and drooping, and the faintneſs and difficulty of breathing increaſed for about two hours and a half after that, at which time the animal died, the gage being not ſenſibly altered, unleſs perhaps the mercury appeared to be impelled up a little thort higher than it was when put in; which yet might well enough proceed from ſome accidental cauſe.

EXPERIMENT III.

To ſatisfy ſome curious perſons, that it is not want of coldneſs, but ſomething elſe in the included air, that makes it deſtroy the birds, that are pent up in it, and by the hot exhalations, that ſteam from their bodies, may be ſuppoſed to overwarm it, we made the following experiment.

In a glaſs-vial, capacious enough to hold about three quarts of water, we not only included, but for greater accuracy, hermetically ſealed up a ſmall bird, and found, that in a few minutes he began to be ſick and pant; which ſymptoms I ſuffered to continue and increaſe againſt the mind of a learned by-ſtander, (who thought the animal would not hold out ſo long,) till they had laſted juſt half an hour: at which time, having provided a veſſel of water with ſal armoniack, newly put into it, to refrigerate it, (according to the way I elſewhere publiſhed;) and the liquor thus made exceeding cold, ſomewhat to the wonder of thoſe that felt it; the vial, with the ſick bird, was immerſed in it, and kept there in that condition for ſix minutes; and yet it did not appear, in the judgment of the by-ſtanders, that the great refrigeration, that muſt be this way way procured to the impriſoned air, did ſenſibly revive, or refreſh the drooping animal, who manifeſtly continued to pant exceedingly as before, and, as ſome affirmed, more; ſo that this remedy proving ineffectual, the vial was removed out of the water, and the bird ſometime after did, as I foretold, make many ſtrains to vomit (though ſhe brought up little) followed by evacuations downward, before ſhe quite expired, which ſhe did within a minute or two of a juſt hour, after the beginning of her impriſonment.

If I had been able (which I was not) to procure more birds, I would willingly have proſecuted this experiment by ſeveral other, not unhopeful, trials which for want of ſubjects I was fain to leave only deſigned.

TITLE XVI.

Of the uſe of the air, to elevate the ſteams of bodies.

IN the digreſſion about reſpiration, annexed to the 41ſt of our Phyſico-mechanical Experiments formerly publiſhed, it is propoſed as one of the conſiderable uſes of the air in reſpiration, that, being drawn into the lungs, it ſerves to carry off with it, when it is breathed out again, the recrementitious ſteams, that are ſeparated from the maſs of blood in its paſſage through the lungs; from which fuliginous excrements if the blood were not continually freed by the help of the air, after nature had been accuſtomed to that way of diſcharging them, their ſtay in the body might have very great and deſtructive operations on it.

For the illuſtration of this uſe of the air, I ſhall now ſubjoin the following experiment.

We made by diſtillation a blood-red liquor, which chiefly conſiſted of ſuch ſaline and ſpirituous particles, as may be obtained from the maſs of blood in human bodies: this liquor is of ſuch a nature, that if a glaſs vial, about half filled with it, be kept well ſtopped, the red liquor will reſt as quietly as any ordinary one, without ſending up any ſmoke or viſible exhalation; but if the vial be unſtopped ſo, that the external air be permitted to come in, and touch the ſurface of the liquor, within a quarter of a minute or leſs, there will, upon this contact, be elevated a copious white ſmoke, which will not only fill the upper part of the glaſs, but plentifully paſs out into the open air, till the vial be again ſtopped.

My purpoſe in this tract to forbear ſidings in controverſies keeps me from taking notice of the ſpeculations ſuggeſted by ſome of the phænomena of this liquor; which yet I thought I might lawfully mention, as far as I have done it, becauſe it but adventures upon giving one of the uſes rather of the air, than immediately of reſpiration it ſelf; and is brought but to illuſtrate what I have not found denied by any, though conſidered by very few; namely, the office of the air to carry off in expiration the fuliginous ſteams of the lungs. For in our experiment we manifeſtly ſee, that the very contact of the air may give the corpuſcles of moiſt bodies a peculiar volatility, or facility to emerge in the form of ſteams. I know, there are ſome corroſive ſpirits, as in nitre and ſalt, ſimple, or compounded of them, that, when they are very ſtrong, emit for a while manifeſt fumes; but the difference of thoſe liquors, and their inferiority to our red ſpirit, in the capacity of ſmoking liquors, might eaſily enough be manifeſted, if it were judged proper in this place, where it may ſuffice, to take notice of theſe two things. The one is, that when the vial has lain ſtopped and quiet a competent time, the upper half of it will appear deſtitute of fumes, of which the air, it ſeems, will imbibe, and conſtantly retain but a certain moderate quantity; which may give ſome light towards the reaſon, why the ſame air, which will be quite clogged with ſteams, will not long ſerve for reſpiration, which requires frequent ſupplies of freſh air. The other is, that if the unſtopped vial were placed in our vacuum, it would not emit any viſible ſteams at all, nor ſo much as to appear in the upper part of the glaſs itſelf that held the liquor; whereas, when the air was by degrees reſtored at the ſtopcock, without moving the receiver itſelf, to avoid injuring its cloſeneſs, the returning air would preſently raiſe the fumes, firſt into the vacant part of the vial, whence they would aſcend into the capacity of the receiver; and likewiſe, when the air, that was requiſite to ſupport them, was pumped out, they alſo accompanied it, as their unpleaſant ſmell evinced, and the red ſpirit, though it remained unſtopped, emitted no more fumes till the new air was let in.

One may compare with this liquor another ſmoking one, mentioned in the 29th of the firſt publiſhed pneumatical experiments, where an experiment is related of it, that has ſomething in common with this, and may ſo far ſerve to confirm what is now delivered, as this alſo has ſome things additional to that: beſides that, that liquor being made with ingredients corroſive, and of a bad name among chemiſts themſelves, the fumes, that proceed from it, may fright many from daring to meddle with it whereas; this our red ſpirit has been found potently medicinal for ſome diſtempers of the lungs, by a doctor of phyſick, whom I deſired to try it. The other phænomena of this liquor I ſhall not ſtay to deſcribe, as not belonging to this place; and the liquor itſelf, with very little variation, I have in the Hiſtory of Colours communicated.

TITLE XVII.

Of the long continuance of a ſlow-worm and a leach alive, in the vacuum made by our engine.

IN the often cited digreſſion about reſpiration, there is mention made of the great vivaciouſneſs of houſe-ſnails, as they call them, and how little operation the withdrawing of the air had upon them in compariſon of what it is wont to have on other animals. I ſhall now add by way of confirmation, that I made trial upon ordinary white ſnails, without ſhells, whereof two of differing ſizes (the biggeſt about an inch and a half, and the other about an inch in length) were included in a ſmall portable receiver, which being carefully exhauſted, and ſecured againſt the return of the air, was attentively conſidered by me, preſently after it was removed from the engine; whereby it was eaſy to diſcern, that both the ſnails thruſt out and retracted their horns (as they are commonly called) at pleaſure, though their bodies had in the ſofter places pretty ſtore of newly generated bubbles ſticking to them: but though they did not loſe their motion near ſo ſoon, as other animals were in our vacuum wont to do; yet coming to look on them after ſome hours, they appeared moveleſs and very tumid, and at the end of twelve hours, the inward parts of their bodies ſeemed to be almoſt vaniſhed, and they ſeemed to be but a couple of ſmall full-blown bladders; and on the letting in of the air they immediately ſo ſhrunk, as if the bladders having been pricked, the receding air had left behind it nothing but ſkins; nor did either of the ſnails afterwards, though kept many hours, give any ſigns of life.

Upon a ſuppoſition, that the cold and clammy conſtitution of ſnails might be a main cauſe of their being able to endure the abſence of the air ſo well, I thought it worth trial, whether effs and leaches might not yet be more able to continue in our vacuum than a ſnail; and accordingly ſome experiments were made purſuant to that curioſity; the moſt fully regiſtered whereof are theſe, that follow.

Experiment I.

We included in a receiver, whoſe globular part was about the bigneſs of a large orange, one of that ſort of animals, that they vulgarly call effs: having withdrawn, but not ſollicitouſly, the air, and ſecured the veſſel againſt the unpermitted return of it, we kept him there about eight and forty hours, during all which time he continued alive, but appeared ſomewhat ſwelled in his belly; his under-chap moving the very firſt night, but not the day and night following. By opening the receiver at length under water, we perceived, that about half the air had been drawn out. As ſoon as the water was impelled into the glaſs, the animal, that was before dull and torpid, ſeemed, by very nimble and extravagant motions to be ſtrangely revived.

Experiment II.

We took a leach, that was of a moderate bigneſs, or ſomewhat ſhort of it; and having included it together with ſome water in a portable receiver, that was gueſſed to be capable of holding about 10 or 12 ounces of that liquor, the air was pumped out after the uſual manner, and the receiver being removed to a lightſome place, we obſerved, as we expected, that the leach keeping himſelf under water, there emerged from divers parts of her body ſtore of bubbles, ſome of them in a diſperſed way, but others in rows or files, if I may ſo ſpeak, that ſeemed to come from determinate points. Though this production of bubbles laſted a pretty while, yet the leach did not ſeem to be very much diſcompoſed by her preſent condition. This done, we diſpoſed of the receiver, which was well ſecured from the ingreſs of the outward air, into a quiet place, where we daily viſited it once at leaſt, or oftner, as there was occaſion; and found the leach ſomewhat faſtened by her tail to that part of the glaſs, that was under water, and ſometimes wandering about that part, which was quite above water; and ſtill, when we endeavoured to excite her, ſhe quickly manifeſted herſelf to be alive: and indeed (which will be thought ſtrange) appeared ſo lively after the full expiration of five natural days, that expecting ſomething might have happened to the receiver, and thereupon reſolving to try how ſtanch it had continued, I opened it under water, by which means the outward air impelled in ſo much of that liquor, that I was ſatisfied, the receiver was immediately before as well exhauſted, as others are wont to be in our pneumatical experiments.

TITLE XVIII.

Of what happened to ſome creeping inſects in our vacuum.

NOTWITHSTANDING the great variety of reptiles, that nature does almoſt every where produce; yet the inconvenient time,

time and place, wherein the following trials were made, ſupplied me with ſo few, that about thoſe animals I find among my adverſaria, no more than the enſuing notes.

EXPERIMENT I.

WE took five or ſix caterpillars of the ſame ſort; but I could not tell, to what ultimate ſpecies the writers about inſects referred them. Theſe being put into a ſeparable receiver of a moderate ſize, had the air drawn away from them, and carefully kept from returning. But notwithſtanding this deprivation of air, I found them about an hour after moving to and fro in the receiver; and even above two hours after that, I could, by ſhaking the veſſel, excite in them ſome motions, that I did not ſuſpect to be convulſive: but looking upon them again ſome time before I was to go to bed (which may be was about 10 hours after they were firſt included) they ſeemed to be quite dead; and though the air were forthwith reſtored to them, they continued to appeared ſo, until I went to bed; yet, for reaſons elſewhere expreſſed, I thought fit to try, whether time might not at length recover them, and leaving them all night in the receiver, I found the next day, that three, if not four of them, were perfectly alive.

EXPERIMENT II.

WE took from an hedge a branch, that had a large cobweb of caterpillars in it; and having divided it into two parts, we put them into like receivers, and in one of them ſhut up the caterpillars together with the air, which from the other was exhauſted. The event was, that in that, which had the air, the little and difficultly viſible inſects, after a ſmall time, appeared to move up and down as before, and ſo continued to do for a day or two; after which, other occaſions made the experiment to be neglected; whereas that glaſs, whence the air had been drawn out, and continued kept out, ſhewed after a very little while no motion, that we could perceive. But to try, whether caterpillars may continue ſo far alive in our vacuum all the winter, as the next ſpring or ſummer, to proceed in the tranſmigration to a butterfly, is a trial, that we have but begun, and therefore muſt not pretend to ſay any thing about its event.

TITLE XIX.

Of the phænomena ſuggeſted by winged inſects in our vacuum.

WHEN our Phyſico-mechanical Experiments were diſpatched to the preſs, the inconvenient ſeaſon of the year, and the difficulty of making the receivers, I then employed, to keep out the air for any long time, hindered me from then publiſhing above a trial or two of what will happen to winged inſects in our vacuum. But afterwards being provided of more commodious veſſels, I thought fit at ſeveral times, to repair that omiſſion by various attempts, whereof the chief enſue.

EXPERIMENT I.

Nov. 12, about 8 a clock at night. THERE were taken four middle ſized fleſh-flies, which having their heads cut off, were incloſed in a portable receiver, furniſhed with a pretty large pipe, and a bubble at the end. As ſoon as the receivers was exhauſted, thoſe flies loſt their motion, which was not briſk before; an hour or two after, I approached them to the fire, which, reſtored not their motion to them (but as to one of them, I ſuſpect it had a languid motion for a while:) wherefore I let in the air upon them, after which, in a very ſhort time (though not immediately) they began one after another to move their legs, and one or two of them to walk; and having kept them all night in a warm place, when I ſent one the next morning to try, if they would manifeſt any motion, he told me, that for a while they did, though when I afterwards roſe myſelf, I could not perceive any motion in them.

EXPERIMENT II.

Sept. 11. ABOUT noon we cloſed up divers ordinary flies, and a bee or waſp; all which, when the air was fully withdrawn, lay as dead, ſave that for a few minutes ſome of them had convulſive motions in their legs. They continued in this ſtate 48 hours, after which, the air was let in upon them, and that not producing any ſigns of life in them, they were laid in the meridian ſun, but not any of them ſeemed in any degree to recover.

EXPERIMENT III.

Dec. 11. WE put a great fleſh-fly into a very ſmall portable receiver, where, at firſt, it appeared to be very briſk and lively, but as ſoon as the air was drawn out, fell on his back, and ſeemed to have convulſive motions in her feet and proboſcis; from whence ſhe preſently recovered upon the letting in of the air, which being drawn out again, ſhe lay as dead: but a while after, (within a quarter, or half an hour) I perceived, that upon ſhaking the receiver, ſhe ſtirred up and down, but faintly. This was done pretty late yeſternight, ſince whence I had not occaſion to look on the glaſs, till this night after ſupper, when I found the fly not (whilſt I ſtayed to endeavour it) to be recovered either by warmth, or letting in the air. A while after this note was written, this fly recovered; and being next morning ſealed up again in that glaſs, and kept 48 hours, though over the chimney, died for good and all.

EXPERIMENT IV.

WE took a large graſs-hopper, whoſe body, beſides the horns and limbs, was about an inch in length, and of a great thickneſs in proportion to that length; this we conveyed into a portable receiver of an oval form, and capable of holding, by our gueſs, about a pint of water and more; and having afterwards pumped out the air, till by the gage it appeared to have been pretty well drawn out, we took care, no air ſhould re-enter to diſturb the experiment. The ſucceſs whereof was this: firſt, though, before the exhauſtion of the air was begun, the graſs-hopper was ſtirring, and lively; and continued ſo for a while after the beginning of the operation; yet when the air began to be conſiderably rarified, he appeared to be very ill at eaſe, and ſeemed to ſweat out of the abdomen many little drops of liquor, which being united, trickled down the glaſs like a little ſtream, which made at the bottom a ſmall pool of clear liquor, amounting to near a quarter of a ſpoonful, and by that time the receiver was ready to be taken off, the graſs-hopper was fallen upon his back and lay as dead. Secondly, though having a little after laid the glaſs in a ſouth window, on which the ſun then ſhone, I perceived ſome ſlow motions in the thorax, as if he ſtrained to fetch breath; yet I was not ſure they were not convulſive motions, and whatever they were, they laſted but a while, and then the animal appeared to be quite dead, and to continue ſo for three hours from the removal of the receiver. Thirdly, that time being expired, the glaſs was opened, and the air let in upon him; notwithſtanding which there appeared no ſign at all of life; but imagining there might be ſome time requiſite to recover him out of ſo deep a ſwoon, I let the glaſs reſt in a convenient poſture, that the water, that came from him might not endanger him, for a quarter or half an hour; and though I then perceived no ſigns of life, yet being deſirous to purſue the trial yet further, I cauſed him to be carried into a ſun-ſhiny place, where the beams of a declining ſun preſently began to make him ſtir his limbs, and, in a ſhort time brought him perfectly to life again.

EXPERIMENT V.

April 15. We took one of thoſe ſhining beetles they call roſe-flies, and having included it in a very ſmall round receiver, which we exhauſted; and though he that attended the engine, affirmed, it ſtruggled much whilſt the air was withdrawing, yet preſently after I could perceive but little motion, and part of that ſeemed almoſt convulſive; and afterward going abroad, and not returning to look on the glaſs till about ſix hours after, the fly ſeemed quite dead, and diſcovered not any motion upon that of the glaſs. And within about an hour after, though I let the air ruſh in, yet no ſign of life enſued, neither immediately, nor for a pretty while after. So that ſuſpecting the fly to be really dead, and yet not reſolutely concluding it, though I would then wait no longer, yet three or four hours after (viz. about 10 of the clock at night) I returned to the receiver, and found the beetle lively enough. Whereupon I cauſed the glaſs to be again exhauſted, and ſecured from the ingreſs of the air, during which time the animal ſeemed to be much diſquieted by what was done to it, but did not loſe its motion before I went to bed, which was ſoon after.

EXPERIMENT VI.

ABOUT butterflies, I remember, I made ſeveral trials, moſt of which chanced to be loſt; but thus much I very well remember, that having obſerved them not only to live, but to move longer than was expected, I choſe to include divers of them in receivers ſomewhat large, eſpecially, that I might ſee, whether in ſo thin a medium ſome or other of them, by the help of their large wings, would be able to fly. But though, whilſt the air continued in the glaſſes, they flew actively, as well as freely, up and down; and though after the exhauſtion of the air, they continued to live, and were not moveleſs; nay, though at the bottom of the receiver they would even move their wings, and a little flutter, yet I could not perceive any of them to fly, by which I mean, perform any progreſſive motion, ſupported by the medium only. And by frequently inverting the receiver (which I took care ſhould be pretty long, to let them fall from one extream to the other,) they would fall like dead animals without diſplaying their wings, though juſt as they came to touch the bottom, ſome of them would ſometimes ſeem to make ſome uſe of them, but not enough to ſuſtain themſelves, or to keep their falls from being rude enough.

TITLE XX.

Of the neceſſity of air to the motion of ſuch ſmall creatures, as ants, and even mites themſelves.

“ IN the experiments hitherto mentioned, “ the animals, on which the trials have been “ made, were divers of them of a moderate “ bulk; and others of them, though ſmall, yet “ not of the leaſt ſizes, that nature afforded us. “ Wherefore I thought fit to annex the following “ experiments, wherein I deſigned to “ examine, whether even thoſe minute ſorts of “ animals, whoſe bulk is thought the moſt “ contemptible, have not, as well as the greater, “ need of the air, if not to make them live, yet “ at leaſt to enable them to move.”

A pretty number of ants were included in a ſmall portable receiver exhauſted yeſterday about noon: between ſix and ſeven in the afternoon, they ſeemed to be all quite dead, and the rather, becauſe, though they were very lively juſt before they were ſealed up, running briſkly up and down the bubble they were in; yet they grew almoſt moveleſs as ſoon as the air was exhauſted; and a little while after appeared more ſo: though I then ſuſpected more than I ſince did, that they were much inconvenienced by ſome ſmall glutinous ſubſtance, that ſeemed to have got into the ſmall receiver from the vapours of the cement. When I looked on them at the time lately mentioned, I opened the glaſs, whereupon the air ruſhed in; but no ſign of life appeared for a great while in any of the ants: but looking upon them this morning about 9 a clock, I found many of them alive, and moving to and fro.

“ IT is ſaid by naturaliſts upon the authority “ of *Ariſtotle*, that the animal, the Greeks call “ ἄκαρι, is the minuteſt of living creatures. But “ thoſe of this ſort being very hard, if at all, “ to be met with here, I thought fit to make “ ſome experiments upon the leaſt of the terreſtrial animals I could procure, and try, whe-

“ther or no mites themſelves, which are reputed but living points, and not to be taken notice of by the naked eye to be living, but by motions, which even an attentive one can ſcarce diſcover, ſtand in need of the air; eſpecially, becauſe, in caſe they do, it may ſuggeſt to us ſome odd reflections upon the ſtrange ſubtilty and minuteneſs of the aerial particles, which muſt be capable of flowing in, and paſſing out, at the inviſible, and almoſt in-imaginable ſmall pores, and other cavities of the parts of an animal, whoſe entire body is reputed but a phyſical point.”

WE conveyed then a pretty number of mites, together with the mouldy cheeſe they were bred in to nouriſh them, into three or four portable receivers (which were all of them very ſmall,) not much differing in ſize. From all of theſe, ſave one, we withdrew the air; and then, making uſe of our peculiar contrivance to hinder its return, we took them one after another from the engine, and laid them by, for further obſervation. That one, which I took notice that we had reſerved, and, in which, to obſerve the difference, we thought fit to leave the air, was ſealed at a lamp-furnace, after the uſual manner of nipping up glaſſes there. This done, there remained nothing but to obſerve the event of our trials, which afforded us the enſuing phænomena.

1. THOSE mites, that were incloſed in the ſmall glaſs, that never came near the engine, continued alive, and able to walk up and down for a full week, after they had been put in; and poſſibly would have continued much longer, if the glaſs had not been accidentally broken.

2. AS ſoon as ever one of the receivers was removed from the engine, I looked with great attention upon it; and though juſt before the withdrawing the air, the mites were ſeen to move up and down in it; yet, within a few minutes after, the receiver was applied to the engine, I could diſcern in them no life at all, nor was any perceived by ſome younger eyes than mine, whereunto I expoſed them. Nay, by the help of a double convex-glaſs (that was ſo ſet in a frame, as to ſerve me as a microſcope on ſuch occaſions) I was not able to ſee any of them ſtir up and down. Nor was any motion taken notice of in the other ſmall receiver of like bigneſs and ſhape with mine, by them that had exhauſted it of air. And my occaſions not permitting me to attend the obſervation any longer in the place where it was made, I took the receiver, I had ſo attentively conſidered myſelf, along with me in the coach; and having occaſion to make ſome ſtay about an hour after, I looked upon it attentively again, but could not perceive any of the mites to ſtir; and the like unſucceſsful obſervation I made, when I had a conveniency two or three hours after that. And the place I did it in, being one, where I thought myſelf, as it were at home, I firſt let in the air, to try if the mites were not quite dead; and though neither upon its ruſhing in, nor during my ſtay there, I could perceive any of them to ſtir; yet I left the receiver unſtopped, as it was in the window, upon a ſuſpicion, that the air might not be able to produce its operation upon them in a ſhort time.

3. AND therefore paſſing by the ſame place about two or three days after, I called in to look upon my receiver, and found a number of my little animals revived, as an attentive eye might eaſily perceive by the motion of certain little white ſpecks, when it was helped to obſerve it by little marks, I made on the outſide of the glaſs (which was purpoſely choſen thin and clear) near this or that mite, with a diamond; by the approach to, or receſs from, which marks, the progreſſive motion became (perhaps within a minute) plainly diſcoverable; eſpecially, if we uſed the following expedient, (which I found the beſt of thoſe I tried,) namely, that when the eye perceived little white ſpecks, that looked like mites, the receiver ſhould be ſo turned and returned, that the bellies and feet of thoſe little creatures were uppermoſt; notwithſtanding which, they would not eaſily drop down, but continue their motion; which ſpecks being made upon the concave ſurface of the thin glaſs itſelf (to which you may approach your eye as much as you pleaſe) are thereby rendered much more eaſily viſible. But, this being only intimated upon the by, I proceed to take notice, that in the newly mentioned receiver, the mites did, by ſtirring up and down, continue to appear alive for two or three days after, if not longer. I ſhould not, I confeſs, have thought it ridiculous to ſuſpect, that the mites, which at firſt loſt their motion, did at laſt really die; and that thoſe, I after ſaw ſtirring up and down, were others newly generated in the included mouldy cheeſe: but I was not apt to think this ſuſpicion probable; not only becauſe of the extreme difficulty of making any living creature to be generated in *Vacuo Boyliano*, but becauſe it did not ſeem agreeable to what I elſewhere noted, about the way and time of the propagation of mites, whoſe eggs I have divers times obſerved with pleaſure, that at a ſeaſon of the year, that was not favourable (for theſe things happened in a cold *March*,) newly generated mites ſhould in two or three days grow up to their juſt bigneſs, which ſeveral of thoſe we obſerved, ſeemed to have attained.

4. BUT, becauſe it doth not by the third phænomena appear, whether or no, in caſe our mites had been kept in a moveleſs ſtate for a much conſiderabler time, than three or four hours, they would have been recoverable by the admiſſion of the air; I ſhall add, to ſatisfy that doubt, that one of the portable receivers above-mentioned, being exhauſted, and carefully ſecured from the regreſs of the air, was kept from monday morning to thurſday morning: after all which time, our attentive eyes being unable to diſcover any ſigns of life among the included mites, the air was let in upon them, and after no long time, had ſuch an operation upon them, that both I, and others, could plainly ſee them creep up and down in the glaſſes again.

SOME

CONSIDERATIONS

TOUCHING THE

USEFULNESS

OF EXPERIMENTAL

NATURAL PHILOSOPHY.

Propoſed in a familiar Diſcourſe to a Friend, by way of
Invitation to the ſtudy of it.

The Second Tome, containing the latter Section of the Second Part.

The PUBLISHER to the READER.

WHEREAS the preface of the noble author to this ſecond tome of the uſefulneſs of experimental philoſophy, was written with deſign it ſhould come forth a year or two before the laſt, it is fit, that ſomething be now added about the preſent publication.

FIRST, if inquiry be made, why the eſſays, that now come abroad, are not accompanied with thoſe others, that according to the ſorts of the titles, ſhould precede ſome of them; he repreſents, that it was not thought fit, that thoſe, that are now publiſhed, having no neceſſary dependance on the reſt, and being ſufficiently intelligible without them, ſhould ſtay for diſcourſes, that are not at preſent ready, and perhaps will not ſuddenly be ſo; partly, in regard they conſiſt of no ſmall number of looſe papers, which by reaſon of ſome, yet inſuperable, obſtacles (of which want of health is none of the leaſt) he cannot conveniently ſeek out, range, and compleat; and partly, becauſe he cannot, in the place where he is now detained, be maſter of divers uncommon minerals, and ſome chymical productions, whoſe deſcriptions through haſte he omitted, becauſe he had them at hand in the place, where thoſe eſſays were written, and preſumed, he could at leiſure fill up thoſe vacancies he left for ſuch deſcriptions.

SECONDLY, as to the eſſays themſelves, which, for the reaſons juſt now mentioned, come not abroad with the reſt, though the excellent author hath of late years conſtantly refuſed to promiſe any thing to the publick, yet, that the reader may the better judge of the ſcope and deſign of the whole treatiſe, he will not deny him an intimation of what ſubjects thoſe eſſays relate unto, by telling him, that one of them treateth of the uſefulneſs of chymiſtry (not to phyſick, but) to the empire of man over the inferior works of nature: another, of the advantages, that a naturaliſt's country may derive from his curioſity: another, of the mutual aſſiſtance, that the ſpeculative and practical part of phyſiology may afford each other: after which comes a diſcourſe, containing inducements to hope for much greater things from experimental philoſophy, than men have hitherto obtained.

LASTLY, as to what the author taketh notice of, about the coincidents of ſome experiments, that may be mentioned as well by others as by him; it is very poſſible, that the ſame things may, by the ſame, or other ways, come to the knowledge of different perſons. Beſides, that I have heard him mention with ſome complaint, that, when divers years ſince, he writ ſeveral diſcourſes (whereof ſome belonged to the uſefulneſs of experimental philoſophy,) for the uſe of a private friend, not for the preſs, he was not ſo ſhy, as had been requiſite, of ſhewing divers experiments, and of imparting others in diſcourſe, to inquiſitive men, whether Engliſh or Foreigners, that came to viſit him; divers of which things he afterwards found in print, ſometimes indeed with, but for the moſt part without, mention of his name. So, that ſometimes his unwillingneſs to diſoblige ſuch writers, and to contend about ſuch matters, made him either wholly omit ſome of the particulars he afterwards intended to publiſh, or even to croſs out ſeveral paſſages, that he had already written, where he would, without much inconvenience (for, that did not always happen) either quite leave them out, or ſubſtitute others (though leſs proper) in their ſtead. He added alſo, that ſometimes obſerving his notions and experiments to be aſcribed to other writers, and ſomewhat wondring at it, he found indeed ſuch writers to have mentioned ſuch things, but in editions, that came abroad after the publication of our author's writings; from whence ſuch things might, with the greater likelihood be preſumed to have been borrowed, both becauſe ſome of the writers had converſed with him, and he could not find them in the firſt edition of ſuch books. But theſe unfair proceedings being the faults but of a few, he ſaid, he was far from imputing them to the generality of thoſe, that have mentioned, (which divers of thoſe have very civilly done,) his experiments, or writings in theirs.

The particulars being thus taken notice of, the curious reader ought not to be any longer detained from converſing with the author himſelf in this inſtructive treatiſe. Farewel.

The PREAMBLE.

I HAVE, in the preface, and body of the former, and already publiſhed part of this treatiſe, taken notice of ſo many of the things, that concern the whole work in general, that I preſume it will not here be neceſſary to detain the reader with any other particulars, than thoſe, that will be offered by way of anſwers to ſome queſtions, that are like to be aſked about the publication of this preſent tome.

And in the firſt place, if it be demanded, why this latter part did not more cloſely follow the former, I have this to anſwer; that the papers it conſiſted of, chanced to be ſo unfortunately diſpoſed of, during the late publick confuſions, that for a great while I was not the maſter of them, and in the mean while was, ſometimes upon one occaſion, and ſometimes upon another, engaged to venture abroad the Hiſtory of Colours, the Hiſtory of Cold (with the preliminary and additional tracts) Hydroſtatical Paradoxes, and the Origin of Forms and Qualities; the publication of which treatiſes, beſides that of ſome anonymous papers, as it took up much of the time I had to ſpare for the preſs; ſo it may, I ſuppoſe, keep it from being thought ſtrange, that I did not trouble myſelf and others with this book alſo. And indeed, this having been (as the ſcope and divers paſſages of it ſufficiently intimate) one of the firſt I wrote to the gentleman I call *Pyrophilus*, I had occaſion, whilſt it was out of the way, to make uſe of ſo many of the experiments and obſervations, that belonged to it, that fearing I had thereby too much robbed, and disfigured it, to leave it any way fit for publick view, I had the greater temptation to neglect the looking after it.

But if it be further demanded, why then, ſince it was not ready to come out more early, I did not condemn it not to come out at all? I have two things to return by way of anſwer.

The firſt is, that ſome eminent virtuoſi, to whom I owe a peculiar reſpect, were pleaſed to challenge the edition of this tome, as if I had made myſelf a debtor to the publick for the ſecond part of this work, by having ſuffered what I wrote to a private friend to be divulged in the firſt. Eſpecially ſince the publick had given that ſo very favourable an entertainment; as, beſides other things, the early reprinting of it manifeſted.

The other part of my anſwer, and that, which made the former conſideration prevalent, is, that I was overcome, either by the reaſons, or by the authority, of thoſe ingenious perſons, that were pleaſed to think, that this work would not prove unſerviceable to mankind, to whoſe good, both as a man, and Chriſtian, I have been long ambitious to contribute, as well upon the account of the great author and divine redeemer of men, as of that common nature, whereof all men partake. What the utilities of this work were conceived to be, the reader will find diſcloſed at the end of this preface. To which I will therefore refer him for an account of them; and now only take notice, that as to one of the ſcruples I had againſt the publication, namely, that I had plundered this preſent treatiſe of divers particulars, wherewith I had accommodated ſome of my other writing; I could not well reject this anſwer, that in ſo many years as had paſſed ſince the writings of this book, I had not been ſo negligent a commercer with the works of nature and art, as not to be able to make ſome amends for what I had taken away, and eaſily ſubſtitute other experiments and obſervations, to ſupply the vacancies, left by thoſe I had tranſferred to other diſcourſes.

And as to another of my ſcruples, about venturing abroad this tome, namely, that it muſt come forth ſo late, if it ſhould come forth at all, it was anſwered, that it could ſcarce come forth more ſeaſonably to recommend the whole deſign of the Royal Society, whoſe generous aims being to promote the knowledge of nature, and make it uſeful to human life. This treatiſe may procure them ſome number of aſſiſtants, in a work, whoſe vaſtneſs and difficulty will need very many, if men's curioſity and induſtry can by this treatiſe (or any to the like purpoſe) be well excited by a conviction of the real and wide diſparity betwixt true natural philoſophy, and that of the peripatetick ſchools; and that in cultivating the former, they will not meet with a field, that will afford them nothing, but (the wonted production of the latter) the thorns and thiſtles of acute indeed, but uſeleſs, and oftentimes troubleſome, ſubtilties; but, that they may expect a ſoil, that may by a due culture be brought to afford them both curious flowers to gratify curioſity, and delight their ſenſes, and excellent fruits, and other ſubſtantial productions, to anſwer the neceſſities, and furniſh the accommodations of human life.

And I will not deny, that I have had the fortune to be looked upon, as not the unfitteſt perſon in the world to offer ſomething in this kind; for thoſe, that are meer ſcholars, though never ſo learned and critical, are not wont to be acquainted enough with nature and trades, to be able to ſuggeſt thoſe inſtances, that are the moſt proper to manifeſt that, which men are to be convinced of. The meer chymiſts, beſides that their curioſity is wont to be too much confined to let them be fitteſt for ſuch a work, have the ill fortune to be diſtruſted by the generality of men, not credulous; which is a great unhappineſs in this caſe, becauſe, that though their experiments were never ſo true (as divers of them are) yet ſkill in their art being requiſite to make them, men's diffidence of the propoſers, joined with the difficulty of examining the things, will not allow them, either to believe what is propoſed, or to try it. And as for the new philoſophers (as they call them) though, if they were to write but for philoſophical readers, I know ſeveral of them, that would queſtionleſs do it rarely well; yet the generality of thoſe readers, to whom we would give good impreſſions of the ſtudy of nature, being ſuch as will probably be more wrought upon by the variety of examples, and eaſy experiments, than by the deepeſt notions, and the neateſt hypotheſes, ſuch a treatiſe for the kind, as that which follows, containing many practices of artifices and other particulars, that are either of eaſy trial, or immediate uſe, may perhaps by that variety gratify, and perſwade a greater number of differing ſorts of readers, than a far more learned and elaborate piece, that might be welcomer to more intelligent and philoſophical peruſers.

If it be aſked by ſome, that know me, whence it comes, that the ſecond part of the uſefulneſs of experimental philoſophy being written (as very credible perſons, that ſaw it can witneſs) about the year 1658, there may be met with in the following treatiſe ſome experiments of my own, that they know were ſince made, and ſome (though few) citations out of books publiſhed ſince that time? If, I ſay, this be aſked, the anſwer is intimated a little above; for having transferred to other tracts many paſſages, that belonged to thoſe I now publiſh, I was obliged to repair the injury I had done them, by ſupplying them with ſuch particulars, as offered themſelves to my memory, when I haſtily reviewed this tome, without ſcrupulouſly minding the times, when the particulars inſerted did firſt occur. And if this advertiſement be applied to ſome other of my writings, that either the importunity of friends, or ſome unwelcome accidents, engaged me to publiſh out of their due time, and not in their intended order; it may keep men from thinking, that when I firſt wrote them, I had read over, or at leaſt ſeen, (which indeed I neither did nor could) every book of a recenter date, of which upon occaſion I mention a paſſage or two, and thoſe perhaps, as they are cited by other authors, we being here in *England* but ſlenderly, and very ſlowly, furniſhed with modern foreign books.

All theſe inſerted paſſages the reader ſhould find included in parentheſis, as the printers call theſe marks (), by which he will yet be able to diſtinguiſh ſeveral of them, though I now find, that ſome others, by the negligence of the tranſcribers, or of the preſs, or of both, have been omitted; which advertiſement I fear may have need to be extended to ſome other printed tracts of mine, wherein parentheſis are to be met with.

Bating theſe few additional paſſages, the enſuing book comes forth, without taking notice of what changes or diſcoveries have happened in the common-wealth of letters, ſince the time it was written in. On which account, if ſome few of thoſe many particulars delivered there ſhould chance to be co-incident with what ſome other man hath written, I would neither on the one ſide be thought a plaguary myſelf, nor on the other ſide deny any man, to whom it may be due, the honour of the earlieſt publication; though, to ſhun needleſs controverſies, I am ſomewhat ſhy of naming this or that perſon, as the firſt propoſer or inventor of an experiment, which, eſpecially if the perſons or things be not conſiderable, is often difficult enough to diſcover: witneſs the conteſts, that have been, and yet continue, about the firſt inventors of common weather-glaſſes, the aſcenſion of water in ſlender pipes, the glaſs drops that fly in pieces, the meaſuring of time by a pendulum, and, which is more ſtrange, the art of printing itſelf. If it be aſked, why I did not forbear to make uſe of ſome practices of tradeſmen, and other known, and perhaps ſeemingly trivial, experiments; theſe things may be replied.

I. That ſince on divers occaſions it was requiſite, that my diſcourſe ſhould tend rather to convince, than barely to inform my reader, it was proper, that I ſhould employ at leaſt ſome inſtances, whoſe truth was generally enough known, or eaſy to be known, by making enquiry among artificers, even by ſuch as out of lazineſs, or want of ſkill, or accommodation, cannot conveniently make themſelves the trials.

II. But yet I have taken care, that theſe ſhould not be the only, nor yet the moſt numerous inſtances, I make uſe of; it being in this tome, as well as in my other phyſiological writings, my main buſineſs, to take all juſt occaſions, to contribute as much, as without indiſcretion I can, to the hiſtory of nature and arts.

III. As to the practices and obſervations of tradeſmen, the two conſiderations already alledged may both of them be extended to the giving of an account of the mention I make of them. Of the truth of divers of the experiments I alledge of theirs, one may be eaſily ſatisfied, by inquiring of artificers about it; and the particular, or more circumſtantial accounts I give of ſome of their experiments, I was induced to ſet down by my deſire to contribute toward an experimental hiſtory. For I have found by long and unwelcome experience, that very few tradeſmen will, and can give a man a clear and full account of their own practices; partly out of envy, partly out of want of ſkill to deliver a relation intelligibly enough, and partly (to which I may add chiefly) becauſe they omit generally, to expreſs either at all, or at leaſt, clearly ſome important circumſtance, which becauſe long uſe hath made very familiar to them, they preſume alſo to be known to others; and yet the omiſſion of ſuch circumſtances doth often render the accounts they give of ſuch practices, ſo dark and ſo defective, that, if their experiments be any thing intricate or difficult (for if they be ſimple and eaſy, they are not ſo liable to produce miſtakes) I ſeldom think my ſelf ſure of their truth, and that I ſufficiently comprehend them, till I have either tried them at home, or cauſed the artificers to make them in my preſence.

THEY that have given themſelves the trouble of endeavouring to make the experiments of tradeſmen, to be met with in the writings of *Cardan*, *Weckar*, and *Baptiſta Porta*, for inſtance; and have thereby diſcovered (what is not uſually obvious upon a tranſient reading) how lamely and darkly, not to add unintelligibly, ſeveral things are written, will probably afford me their aſſent, having found upon trial the inſtructions of ſuch learned and ingenious men to be often obſcure and inſufficient for practice.

BUT here I muſt give the reader notice, that as mechanical arts for the moſt part advance from time to time towards perfection; ſo the practices of artificers may vary in differing times, as well as in differing places, as I have often had occaſion to obſerve. And therefore I would neither have him condemn other writers or relators, for delivering accounts of the experiments of craftſmen differing from thoſe I have given; nor condemn me, for having contented my ſelf to ſet down ſuch practices faithfully, as I learned them from the beſt artificers (eſpecially thoſe of *London*) I had opportunity to converſe with.

BUT here perhaps it will be demanded by way of objection, whether I do not injure tradeſmen, by diſcovering ſo plainly thoſe things, which our laws call the myſteries of their arts. To a queſtion, that may perhaps by ſome be clamorouſly preſſed, not only upon me, but much more upon ſome ingenious men of our nation, whoſe pens have been more bold than mine in diſcloſing craftſmens ſecrets, it will be requiſite to return ſeveral things by way of anſwer; but that ſuch readers, as are not troubled with the ſcruple, may not be ſo with the apology, they will find this printed in another character, ſo that, if they pleaſe, they may paſs it over unread.

In the preſent edition, theſe paragraphs are included in crotchets.

[FIRST then, it may be repreſented, that I never divulge all the ſecrets and practices neceſſary to the exerciſe of any one trade, contenting myſelf to deliver here and there, upon occaſion, ſome few particular experiments, that make for my preſent purpoſe: ſo that, for much more than I allow my ſelf to do, I can plead the example, not only of other writers, that have publiſhed books to teach the whole myſtery of this, or that trade, as the prieſt *Antonio Neri* hath diligently done in his Italian *Arte Vetraria*, and ſome Engliſh, as well as foreign, virtuoſi have done on other ſubjects; but alſo ſome of the artificers themſelves, as the famous gold-ſmith and jeweller *Benvenuto Cellini* in his much eſteemed Italian tracts of the lapidaries and goldſmiths trades. Thus alſo the famous mineraliſt *Georgius Agricola* publiſhed in Latin a whole volume of the more practical part of mineralogy, wherein he largely and particularly deſcribes experiments, tools, and other things, that belong to the callings of mine men. To which I might add divers other treatiſes, ſome of them French, others Italian, (which, though I could not procure them, I have ſeen among curious collections of books) that have been publiſhed about ſeveral arts by the artificers themſelves. And it is notorious, that in Engliſh, as well as in divers foreign languages, we have books of the arts of gunnery, diſtillation, painting, gardening, &c. divulged by perſons, that profeſſed thoſe callings.

SECONDLY, it is not the cuſtom of tradeſmen to buy books, eſpecially ſuch as are not intended for ſuch readers, and treat (for the moſt part) of things, either beyond their reach, or wherein they ſeem not likely to be concerned; and as for gentlemen and ſcholars, though ſome of them may, to ſatisfy their curioſity, make a few trials, yet their doing ſo will ſcarce in the leaſt be prejudicial to tradeſmen. Since, to omit other arguments, it will not be worth while for a virtuoſi to be at the charge and trouble of buying tools, and procuring other neceſſary accommodations to ſell a few productions of his ſkill, though he ſhould not ſcruple to deſcend to ſuch a practice. For if he make but a ſmall number of experiments, their effects will coſt him more than the like may be bought for of thoſe, that make them in great quantities, and whom their trade obligeth to be ſollicitous to buy their inſtruments and materials at the beſt hand, and ſell them to the beſt profit. Beſides that moſt of the works of artificers are chiefly recommended to the more curious ſort of buyers by a certain politeneſs, and other ornaments, compriſed by many under the name of finiſhing; which require either an inſtructed and dexterous hand, or at leaſt ſome little peculiar directions, which I did not always think my ſelf obliged to mention, in a treatiſe deſigned to aſſiſt my friend to become a philoſopher, not a tradeſman, and publiſhed to help the reader to gain knowledge not to get money.

THIRDLY, to publiſh an experiment or two, or in ſome caſes, a much greater number belonging to a trade is not ſufficient to rob a tradeſman of his profeſſion. For beſides that moſt trades conſiſt of ſeveral parts, and are each of them made up of divers practices, (that commonly are more than a few) thoſe numerous mechanical arts, that are called handicrafts, require (as their very name argueth) a manual dexterity, not to be learned from books, but to be obtained by imitation and uſe. And to theſe conſiderations I ſhall add this more important one, that mechanical profeſſions are wont to be, as it were, made up of two parts, which, for diſtinction ſake, I take leave to call the art and the craft; by the former whereof I mean the ſkill of making ſuch or ſuch things, which are the genuine productions of the art, (as when a taylor maketh a ſuit, or a cloak,) and by the latter I mean the reſult of thoſe informations and experiments, by which the artificer learns to make the utmoſt profit, that he can, of the productions of his art. And this oeconomical prudence is a thing very diſtinct from the art itſelf, and yet is often the moſt beneficial thing to the artificer, informing him how to chuſe his materials, and eſtimate their goodneſs and worth; in what places, and at what times, the beſt and cheapeſt are to be had; where, and when, and to what perſons the things may be moſt profitably vended. In ſhort, the craft is that, which teacheth him how both to buy his materials and tools, and to ſell what he makes with them, to the moſt advantage.

FOURTHLY, it may often prove more advantageous than prejudicial to tradeſmen themſelves, that many of their practices ſhould be known to experimental philoſophers. This I ſuppoſe, that I have ſufficiently proved in ſome, and eſpecially in * one of the following eſſays.

YET I ſhall now repreſent, that though ſome little inconvenience may happen to ſome tradeſmen by the diſcloſing ſome of their experiments to practical naturaliſts, yet, that may be more than compenſated, partly, by what may be contributed to the perfecting of ſuch experiments themſelves, and, partly by the diffuſed knowledge and ſagacity of philoſophers, and by thoſe new inventions, which may probably be expected from ſuch perſons, eſpecially if they be furniſhed with variety of hints from the practices already in uſe. For theſe inventions of ingenious heads do, when once grown into requeſt, ſet many mechanical hands a work, and ſupply tradeſmen with new means of getting a livelihood, or even enriching themſelves. As to the diſcipline ſubordinated to the pure mathematicks, this is very evident; for thoſe ſpeculative ſciences have (though not immediately) produced their trades, that make quadrants, ſectors, aſtrolabes, globes, maps, lutes, vials, organs, and other geometrical, aſtronomical, geographical, and muſical inſtruments; and not to inſtance thoſe many trades, that ſubſiſt by making ſuch things as mechanicians, proceeding upon geometrical propoſitions, have been the authors of; we know, that whether the excellent *Galileo* was, or was not, the firſt finder out of teleſcopes, yet he improved them ſo much, and by his diſcoveries in the heavens did ſo recommend their uſefulneſs to the curious, that many artificers in divers parts of *Europe* have thought fit to take up the trade of making proſpective glaſſes. And ſince his death, ſeveral others have had profitable work laid out for them, by the newer directions of ſome Engliſh gentlemen, deeply ſkilled in dioptricks, and happy at mechanical contrivances; inſomuch, that now we have ſeveral ſhops, that furniſh not only our own virtuoſi, but thoſe of foreign countries, with excellent microſcopes and teleſcopes, of which latter ſort I lately bought one (but I confeſs the only one, that the maker of it, or any man, that I hear of, hath perfected of that bigneſs) which is of threeſcore foot in length, and which the ingenious artiſt, that made it, Mr. *Reeves*, prized conſtantly at no leſs than an hundred pounds Engliſh money. I know not, whether or no I ſhould add, that poſſibly ſome particular experiments of mine have not been hitherto unprofitable to ſeveral tradeſmen. But this I may ſafely affirm, that a great deal of money hath been gained by tradeſmen, both in *England* and elſewhere, upon the account of the ſcarlet dye, invented in our time by *Cornelius Drebble*, who was not bred a dyer, nor other tradeſman. And, that we daily ſee the ſhops of clockmakers and watchmakers more and more furniſhed with thoſe uſeful inſtruments, pendulum clocks, as they are now called, which, but very few years ago, were brought into requeſt by that moſt ingenious gentleman, who diſcovered the new planet about Saturn.]

I have handled the ſubject of the foregoing arguments much more particularly, than I would have done, had not my pen been drawn on by a hope, that the things I have repreſented, may furniſh apologies to many inquiſitive men, who may be thereby enboldened to carry philoſophical materials from the ſhops to the ſchools, and divulge the experiments of artificers, both to the improvement of trades themſelves, and to the great inriching of the hiſtory of arts and nature.

IF it be further demanded, whether I have furniſhed theſe eſſays with the chiefeſt things I could have afforded them, I muſt confeſs, that I have not; for though I had, lying by me, ſeveral experiments and obſervations, leſs inconſiderable than many of thoſe I have made uſe of, which would have been pertinent enough to the ſubjects here treated of; yet I purpoſely forbore to imploy them in theſe tracts, becauſe I would not defraud thoſe others, to which they were more proper, and ſome of them neceſſary. For I freely declare, that my deſign in this preſent tome was not to furniſh it as well as I could, but to preſerve, as in a repoſitory, ſeveral ſcattered experiments and remarks, which I could beſt ſpare from the other treatiſes I had deſigned, which might otherwiſe probably be loſt: but yet I ſhall not deny, that I did not careleſly draw up ſome of the following tracts, but, that I endeavoured to write them in ſuch methods, that they might contain ſeveral diſtinct heads, and thoſe as comprehenſive as I could eaſily make them, that both the young and hopeful gentleman, I call *Pyrophilus*, and I myſelf, might conveniently refer ſuch other practices and experiments (eſpecially thoſe of tradeſmen) as ſhould hereafter occur to us, and appear to belong to thoſe heads. And I did the leſs deſpair of his giving a kind reception to theſe diſcourſes, becauſe I could expect ſo little aſſiſtance in my undertaking, having never met with any book, great or ſmall, written upon the ſubject I was to treat of.

IF hereupon it be objected, that by my own confeſſion, divers of the particulars admitted into this book, are but ſlight, and ſome of them already known; I ſhall repreſent, that as ſome of the experiments ſpoken of are but ſlight, ſo there are others, that poſſibly diſcerning readers will not think to be altogether ſuch; and that it was fit (for reaſons already mentioned in this very preface) that I ſhould not forbear to employ, as proofs to convince others, things either known, or eaſy to be made ſo, eſpecially, ſince I commonly uſe them to ſome purpoſe, or other, whereto they have not been applied; and my deſign in the publication of theſe trifles being chiefly to invite the generality of readers, though of different inclinations, qualities, &c. to addict themſelves to the ſtudy of experimental philoſophy. The variety and eaſineſs I have aimed at in the experiments I have ſet down, may, for ought I know, be more proper, than if I had confined myſelf to the mention of a few choice and elaborate experiments, which ſome readers would think impertinent to their ſtudies, and others judge too difficult for them to put in practice. It appeared not unfit, that a book, whoſe title was like to procure it very different ſorts of readers, ſhould be for the moſt part written in a popular way; divers perſons, eſpecially thoſe of a higher quality, by a trifle, that hath the luck to gratify their curioſity, may be more ſucceſsfully invited to reliſh and eſteem experimental learning, than by a deep notion, or a weighty experiment. And there are others, that will eaſier be brought to value and try experiments, by meeting with ſome few, though but ſlight ones, that happened to ſuit with their humour or calling, or to accommodate them on ſome particular occaſions, than they would by many others, much more luciferous, or otherwiſe important. And though it were to be wiſhed, that men's kindneſs to practical philoſophy were grounded on the beſt motives; yet this treatiſe will not altogether miſs the aim of its publication, if even upon the fore-mentioned ſlighter accounts, it engages readers to make, as well as reliſh, experiments; for the pleaſantneſs, variety, uſefulneſs, and other indearing qualities of ſuch an imployment will probably invite moſt of them to a further progreſs, whereby many uſeful phænomena and obſervations are like to accrue to what is already known of the hiſtory of nature and arts. And if this ſhall come to paſs, it will keep him from complaining of labour loſt, who in venturing upon ſuch a work, as now comes forth, was knowingly to poſtpone the appetite of fame to the deſire of doing ſome ſervice to mankind; to which end he takes one of the directeſt ways to be the contributing ſomewhat to the advancement of experimental philoſophy.

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* The Eſſay here meant is that, which treats of the utility of the naturaliſts inſight into trades.

It remains, that I add ſomething more, which poſſibly may not a little befriend both theſe laſt mentioned anſwers, and ſeveral others contained in this preface; for, when all the former demands occurred to my thoughts, as likely to made, ſome by one ſort of readers, and ſome by another, thoſe virtuoſi, that were ſollicitous for the publication of theſe papers, were not backward to urge the utilities, which they fancied would thence accrue to the publick. And I cannot very well deny, that, as meanly as I think of a treatiſe, to whoſe tome I did not, till the ſecond edition, (when I could conceal it no longer) let my name be prefixed; yet ſuch a work as this for kind, well performed, may be a very uſeful one. And even of this following book, ſuch as it is, it was ſuggeſted, that the uſes would not prove deſpicable, in regard, that beſide thoſe, that are common to it with the formerly publiſhed tome, ſuch as the improvement of the minds of men, and (eſpecially) the aſſiſting them to underſtand the works of God, and thereby engage them to admire, praiſe and thank him for them: beſides theſe (I ſay) there may be other uſes of the following tome, which, to avoid increaſing a prolixity, that I fear is already too great, I ſhall rather name than diſcourſe of, contenting myſelf briefly to intimate, that it was conceived, the peculiar uſes of this preſent tome might be ſuch as theſe.

I. It may afford materials for the hiſtory of nature, which that it may the more plentifully do, I have purpoſely, on ſeveral occaſions, added a greater number of inſtances, than were abſolutely neceſſary, for the making out of what I intended to declare or prove.

II. It may afford ſome inſtructions, advices, and hints to promote the practical or operative part of natural philoſophy in divers particulars, wherein men have been either not able, or not ſollicitous to aſſiſt the curious.

III. It may enable gentlemen and ſcholars to converſe with tradeſmen, and benefit themſelves (and perhaps the tradeſmen too) by that converſation; or, at leaſt, it will qualify them to aſk queſtions of men, that converſe with things; and ſometimes to exchange experiments with them.

IV. It may ſerve to beget a confederacy, and an union between parts of learning, whoſe poſſeſſors have hitherto kept their reſpective ſkills ſtrangers to one another; and by that means may bring great variety of obſervations and experiments of differing kinds into the notice of one man, or of the ſame perſons; which how advantageous it may prove towards the increaſe of knowledge, our illuſtrious Verulam has ſomewhere taught us.

V. It may contribute to the reſcuing natural philoſophy from that unhappy imputation of barrenneſs, which it has ſo long lain under, and which has been, and ſtill is, ſo prejudicial to it. And to effect this reſcue, it will in ſome meaſure enable thoſe, that deſire it, to employ thoſe practical arguments, that are proper to convince many, that are not to be convinced by any other ſort of proofs.

VI. And which is the main of all, it may ſerve by poſitive conſiderations, and directions, to rouſe up the generality of thoſe, that are any thing inquiſitive, and both loudly excite, and ſomewhat aſſiſt, the curioſity of mankind; from which alone may be expected a greater progreſs in uſeful learning, and conſequently greater advantages to men, than in the preſent ſtate of human affairs will be eaſily imagined.

OF THE
USEFULNESS
OF
EXPERIMENTAL PHILOSOPHY.

The SECOND PART. The SECOND SECTION.

Of its USEFULNESS to the Empire of MAN over inferior Creatures.

ESSAY I.

Containing ſome general conſiderations about the means, whereby Experimental Philoſophy may become uſeful to human life.

HITHERTO, my dear *Pyrophilus*, I have attempted to ſatisfy you of the uſefulneſs of experimental natural philoſophy to phyſick: it follows, that I proceed to endeavour to ſhew you, that it may be alſo very ſerviceable to huſbandry, in all its ſubordinate parts, and to thoſe other profeſſions, that ſerve to provide men with food and rayment, or do otherwiſe miniſter to the neceſſities or accommodations of life; as the trades of brewing, baking, fiſhing, fowling, building, and the reſt not needful here to be enumerated. For though the human body, in reſpect of the rational ſoul, (which is the inventreſs and ſeat of ſciences) be one of the corporeal things, over which the empire of knowledge is to be eſtabliſhed; yet taking man as a creature made up of body and ſoul, the advancement of his empire ſeems to conſiſt more properly in the inlargement of his power over the other creatures: phyſick ſeeming rather to defend him againſt revolts and inſurrections at home, than to increaſe his power, and extend the limits of his empire abroad.

BUT, *Pyrophilus*, I hope, you do not expect, that I ſhould now inſiſt on each, or ſo much as on any of the above-mentioned trades, by whoſe intervention it is, that man exerciſes his dominion over external bodies. For ſuch a work would require little leſs than an age, and much more than a volume; and beſides (that it is vaſtly diſproportionate, both to my ſlender ſtock of mechanical ſkill, and to the little leiſure I have to conclude this ſection in) I could not acquaint you with all that I could pertinently enough deliver about theſe matters, without too much defrauding ſome other treatiſes, that I deſign you: and therefore, I hope you will be content, if, in the remaining part of this tract, I do not only preſent you a not deſpicable number of conſiderations proper to manifeſt that, and to intimate, how experimental philoſophy may be of great uſe to the promoting of mechanical arts and trades, but illuſtrate and confirm all, or moſt of thoſe conſiderations by particular inſtances, derived from obſervations and experience.

THIS I ſhall, God aſſiſting, endeavour to do in the following eſſays. But before I deſcend to particulars, it will be expedient in this place to premiſe ſome general conſiderations relating to the influence of experimental philoſophy upon trades, and two or three advertiſements, that concern the enſuing diſcourſes.

SECTION I.

FIRST then, to make it probably, that a true inſight into natural philoſophy may be capable of affording ſome reformation, or other kind of improvement to trades, I ſhall deſire to conſider, that being, for the generality of them, converſant about ſome few particular productions of nature, ſuch men as are thoroughly ſkilled in her general laws, and acquainted with a vaſt number of her productions, and verſed in the ways of applying nature and art jointly to ſeveral purpoſes, according to the ſeveral exigencies of things; ſuch ſagacious perſons (I ſay) will, in all likelihood, be able, ſome way or other, to meliorate the inventions of illiterate tradeſmen. As the huſbandman's ſkill, for inſtance, conſiſting chiefly in the obſervations of the nature of a few plants and animals, their relation to ſuch and ſuch ſoils and kinds of culture, and the operations of ſtars and meteors upon them, which are ſubjects, that properly enough fall within the cognizance of the naturaliſt, it cannot ſeem improbable, that he, that has ſeriouſly and induſtriouſly inquired into the nature of generation, nutrition, and accretion, both in plants and animals, and knows how to vary an uſeful experiment, when once found out, ſo as to remedy the inconveniencies, or ſupply the deficiencies, or improve the advantagiouſneſs, or tranſlate and apply the uſe of it, and, in ſum, he that can knowingly and dexterouſly manage, what his own or other men's obſervations have afforded him, will be able to cultivate the ordinary huſbandman's ſkill with as much improvement, as that confuſed ſkill enables the huſbandman to cultivate his ground.

SECTION II.

TO carry on the foregoing conſiderations a little farther, I will add, that it may as well conduce much to the manifeſting, how much trades are ſubordinate to natural philoſophy, as to the improvement of trades themſelves, that it be attentively conſidered, what things each particular trade is, as it were, made up of. As for example, the chief things in the refiner's trade are, to know the ways of making, and the operations of aqua fortis upon ſilver, gold, and copper; to know how, to purge that menſtruum, that it may diſſolve no gold, nor precipitate any of the ſilver it diſſolves; to know what proportion there ought to be diſſolved in it; to know with what quantity of water to weaken the ſolution, and how long copper-plates need lie in it, to precipitate all the ſilver out of it; to know how lead is to be colliquated with them, and what proportion of it is neceſſary and ſufficient to carry off with it (when it is blown off upon the teſt) the baſer metals; to know how to make cupples of ſeveral ſorts and ſizes, and upon them to draw off the lead or antimony from the ſilver or gold, and diſcern when the metal is ſufficiently refined; to know what proportion of gold and ſilver is requiſite for the making of water-gold as they call it, (becauſe it is ſeparated from ſilver by aqua fortis, which diſſolves this metal, and leaves the other in a fine powder;) theſe things, to which many others are ſubſervient, belong to the refiner's trade, which, though underſtood by few, ſeems to be a very narrow and ſimple trade, in compariſon of a hundred others, whoſe operations are far more numerous and complicated. Now if all trades were judiciouſly reſolved (if I may ſo ſpeak) into the ſeveral parts they conſiſt of, it would, I queſtion not, manifeſtly appear, that the moſt, if not all of them, are in many particulars but corollaries duduced from ſome particular phyſical obſervations, or but applications of them to the uſes of human life.

AND if this be ſo, you will not, I preſume think it unlikely, that by a farther diſcovery of the nature of thoſe particular bodies wherewith the trade is converſant, and a ſolid knowledge of thoſe laws of nature, and thoſe operations of bodies upon one another, which it employs; ſome, if not moſt, of thoſe parts, whereof the trade may be conceived to be made up, may be reformed or bettered; which is enough to make the philoſopher an improver of the trade, which he may become upon ſuch unobvious accounts, that perhaps it may not unreaſonably be hoped, that even the chymiſt's charcoal may be made, by a good naturaliſt, equivalent to an excellent compoſt for land. For if it be true, as well as it is probable, not only that the food of thoſe animals (as oxen, ſheep, &c.) which the huſbandman deals with, ſprings out of the ground; but that the plants, which afford them this food, are themſelves nouriſhed by a certain vegetative ſalt they find in the ground; and, that this ſalt being by frequent ſeminations exhauſted, the ſoil grows barren, until either by the air, or the ſteams of the ſubterraneous parts, or the ſpontaneous maturation of the ſaline rudiments contained in the ground, or by adventitious manure, or by all or divers of theſe together, it be re-impregnated, with a new vital ſaltneſs: if theſe things be true, I ſay, then thoſe chymical experiments, that conduce to diſcover to us, what kind of ſalt that is, and to what other ſalts it is allied or oppoſite, as it is to ſeveral acid ones, may probably afford very uſeful directions to the huſbandman, towards the meliorating of his land, both for corn, trees, graſs, and conſequently cattle. And having had the curioſity * to diſtil ſome earths, ſome dungs, and ſome ſeeds, and obſerve the ſalts abounding in the liquors yielding by them, (of which we have elſewhere occaſion to ſpeak) we found cauſe to wiſh, that experiments of that nature, in relation to the improvement of huſbandry, might be induſtriouſly proſecuted by naturaliſts. He that has obſerved thoſe many particulars in in huſbandry, which might invite that great naturaliſt Sir *F. Bacon* † (who yet mentions very few of them) to pronounce, that nitre is, as it were, the life of vegetables; he that obſerves how conducive that fertilizing dung of pigeons is, both to make earth fruitful to the huſbandman, and to impregnate it with nitrous ſalt for the ſalt-petre-man; and he that knows, that moſt fat earths, ſo defended from the rain and ſun, that the one may not draw up, nor the other waſh down the embryonated ſaltneſs of them, will after a time abound in nitrous ſalt, if they are not permitted to ſpend any in producing of vegetables; ſuch a one, I ſay, will perchance be apt to think, that enquiries into the nature of ſalt-petre may be of great concernment to huſbandry. And to give you, *Pyrophilus*, ſome inducements to expect, that chemiſtry may be very uſeful in ſuch kind of enquiries, I ſhall here mention to you a couple of my experiments relating to nitre.

THE firſt is that, whereby I endeavoured to give an inquiſitive perſon hopes, that materials, which ſeemed unlikely, might, by due changes, and without much art, be turned into ſalt-petre. The experiment was this. I cauſed ſome earth to be digged up juſt underneath the clay-floor of a pigeon-houſe; ſuch earths being believed to abound the moſt with nitre, that needs only to have its particles brought together and united to compoſe ſalt-petre; a pretty quantity of this earth being put into a retort, and diſtilled with a good fire *ex arena*, afforded me, though little or no oil, yet a pretty quantity of a reddiſh liquor, which, inſtead of being, as others would have expected, of an acid nature like ſpirit of nitre, was fit for my purpoſe, by ſtrongly participating of the nature of volatile ſalts; as appeared, not only in that I could, without rectifying it, turn ſyrup of violets with it immediately green, and precipitate a ſolution of ſublimate into a milky ſubſtance; but becauſe there came over, with the ſpirit into the lower part of the receiver, a ſalt in a dry form, which not only was in taſte not unlike the other volatile ſalts, but was ſo far from being of an acid nature, that with an acid menſtruum it readily fell to hiſs, and made an ebullition. So that it ſeems (which in an enquiry about nitre is very conſiderable,) that a ſalt, very repugnant to acids, may, by the operation of the earth and air, be ſo altered, as afterwards by a ſlight management to afford ſalt-petre, whoſe ſpirit is highly acid. But of this experiment I may hereafter make farther mention.

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* *Verulam* hiſt. v. & mort. p. 237. Certiſſimum eſt quamcunque terram, licet puram, neque nitroſis admixtam, ita accumulatam & lectam, ut immunis ſit ſolis, neque emittat aliquid vegetabile, colligere etiam ſatis copioſè nitrum.

† Nat hiſt. cent. 5. cap. 444.

The other, (which we elſewhere have occaſion more particularly to take notice of with reflections on it) is briefly this. We took pot-aſhes, which you know contain but the ſalt of burnt vegetables; and on thoſe, firſt diſſolved in a little fair water, we dropped aqua-fortis (whoſe ſaline part conſiſts indeed of little elſe than the ſpirits of nitre,) till all ebullition and hiſſing betwixt it and the reſolved pot-aſhes were perfectly ceaſed; and having filtrated this liquor, and ſet it in an open veſſel in a gentle heat to evaporate, it did within two or three days after, (and ſometimes, for we made it more than once, even in a few hours) being removed to a cold place, afford us very pure cryſtals of ſalt-petre, as both their ſhape, and flaſhing (on live coals) into a blue halituous flame, informed us. And ſince I have had occaſion to mention the uſe of ſalt-petre in huſbandry, I ſhall not forbear to add, that the knowledge, which the naturaliſt, as a diſcerning chemiſt, may give the huſbandman of the natures and diſtinctions of ſaline bodies, may be of no mean uſe to him, by aſſiſting him to diſcern and obſerve the conſiderableſt differences of the various ſaltneſſes to be found in ſoils, and what ſort of ſaltneſs each particular ſeed or plant moſt affects. For by this means, not only many grounds might be made uſeful, which are thought barren, only by reaſon of our not knowing for what plants the ſaltneſs predominant in them may be proper; but the ſame ground may yield much frequenter crops than commonly it doth, when it is ſucceſſively ſowed only with one ſort of ſeed, by the due alteration of plants delighting in the ſeveral ſorts of ſalts to be met with in that ground; which oftentimes, by being impoveriſhed, or rather freed from one ſort of ſalt, doth but the more plentifully feed thoſe plants, that delight in another: which, in ſome places we have obſerved, that huſbandmen ſeem to have taken notice of already, by ſowing (in fields too remote from their dwellings to have compoſt brought to them) turnips, to fit the ground for wheat, and ſerve for a manure; though in this method ſome other circumſtances may poſſibly concur with the nature of turnip-ſeed, to the preparation of the ground for wheat. And I am prone to think, that there is ſcarce any ground or ſoil, except perhaps mere ſand, that might not, even without much culture, be made fertile, or at leaſt kept from being altogether barren, if we were on the one hand ſkilled in the ways of diſcerning the nature of the ground; and on the other hand acquainted with, and provided of, all the variety of ſeeds and plants, that nature has, though not all in one country, afforded us. For there are divers ſoils, which here in *England*, or in other regions, are, as uſeleſs, left quite uncultivated; which ſeeds or plants, that abound in other countries, and would probably be made to grow in theſe, would make ſerviceable to the huſbandman. Many ſteep and abrupt portions of ground (ſome of them very large) expoſed to the ſouthern ſun are left altogether waſte, not only in *England*, but in divers hot climates, where the planting of grapes for wine is not yet in uſe; though ſuch pieces of land in *France* and *Italy*, and, as I have obſerved, even in the *Rhetian Alps*, nouriſh excellent vineyards.

I know an ancient and landed gentleman, who communicated to me upon his own knowledge an experienced way of making wheat grow and proſper well on mere clay, where there was no grain at all did thrive; which though I have not hitherto had opportunity to try, yet upon the credit of a perſon ſo ſober and qualified, I ſcruple not to mention it here, becauſe the art conſiſting mainly in the imbibition of the ſeed for a determinate time in a certain expreſſed oil, that is not dear; it may make it probable, that without altering the whole ſoil by manures, a ſlight, but convenient change made in the ſeed it ſelf may ſerve to make them fit for one another. And (to add, that upon the by) to ſhew, that the particular diſpoſitions of ſome ſorts of ſeeds may enable them to make the ground they are ſowed in, much more productive, than it would otherwiſe be, I ſhall relate to you, that being not long ſince in the company of a learned and curious traveller, I ſaw, among ſome rarities of a quite other nature, an ear or two of corn, not much unlike our common wheat; at which being ſomewhat ſurprized, I aſked him, what peculiarity had procured that grain admiſſion among ſuch rarities? to which he replied, that in the warmer region, where he begged it of a virtuoſo, one of thoſe grains would afford ſo vaſt a multitude, as he was almoſt aſhamed to name, and I am more than almoſt afraid to repeat: but before I went out of the houſe, an Engliſh gentleman, that had a more than uſual curioſity for ſuch kind of trials, aſſured me, that having obtained ſome grains of that corn, and carefully ſowed it in ſome land of his own, not far from the place we were in, he had out of a ſingle grain ſeveral hundreds; though not near ſo many of them, as the other traveller, who yet was a very ſober and judicious man, related to have been produced in a better climate and ſoil. Of this ſtrangely prolifick wheat, the gentleman readily granted me a promiſe of a ſufficient quantity to make a trial; whereof, when I ſhall have received it from a ſervant of mine in the country, you may command the ſucceſs. And this brought into my mind what

I read in the learned Jeſuit * *Acoſta*, who affirms, that in divers parts of *America*, where it is known, that our European wheat proſpers not, the Indian (or, as many Engliſh have ſtiled it, Virginian) wheat, they call *Maiz*, does ſo wonderfully thrive, that although the ſtalk bear often more than one cluſter, and the grain be big; yet in ſome cluſters he has reckoned ſeven hundred grains: to which he adds, that it is not ſtrange in thoſe countries to gather three hundred faneques, or meaſures, for one ſown. Which paſſages, eſpecially the former, ſpeak of an increaſe, that ſeems ſo little credible, that I ſhould on that account forbear to mention it, were it not, that in *Europe*, and even in *England*, I my ſelf have reckoned ſuch a multitude of grains upon one of the very numerous ears produced by the ſame ſingle grain, that I found myſelf very inclinable to abſolve *Acoſta*, and continue to look upon him as one of the beſt writers of the natural hiſtory of *America*.

We now proceed to take notice, that in ſome Eaſtern countries, a ſort of rice (a grain that makes the chief and moſt uſual food of the natives over almoſt all thoſe parts) proſpers very well upon land ſo drenched with waters, that ſeeds-men, to ſcatter the rice, do rather wade than walk. But this itſelf (which, for the main, was confirmed to me by eye-witneſſes) is leſs ſtrange, and does leſs illuſtriouſly confirm what I was propoſing, than what the inquiſitive Jeſuit *Martinius* affirms to be the practice of ſome (as well great as ſmall) countries in *China*, where, in divers places, that are all the year under water, and would by our European huſbandmen be thought capable of no other uſe, than that of ponds or lakes, the Chinenſes caſt a certain ſeed ſo well appropriated to the place, that is to receive it, that though it falls not immediately on the land but on the water, ſo that one would think they were not about to ſow a field, but bait a pond for fiſhes, yet this ſeed, being adapted to the ſoil it meets with at the bottom of the water, does ſo well proſper and ſhoot up to the top, that in its proper ſeaſon the ſurface of the water looks as freſh and verdant, as a fruitful meadow, and yields as rich a crop. But for fear of digreſſing, I ſhall, *Pyrophilus*, proceed to tell you, that perhaps alſo chemiſtry, eſpecially in conjunction with hydroſtaticks, may prove ſerviceable to the ingenious huſbandman, by aſſiſting him to diſcover the kinds and degrees of ſaltneſſes, that are in ſeveral other bodies that he much deals with. I remember I have met with things ſurprizing enough, in examining ſome ſorts of earths by diſtillation, and by ſeveral chemical inſtruments of diſcovery; but though I have likewiſe had the curioſity to diſtil dungs and grain, and fruits, and ſome other ſubjects, wherewith the huſbandman is converſant, to obſerve what kinds of ſaline and other liquors, and in what proportion, and of what ſtrength, they could afford me; yet not having any notes by me of the particular trials, I ſhall content my ſelf to have given you this hint of a new ſort of experiments in huſbandry; and ſhall only add, as to ſalts, that ſince the fertilizing power of dungs ſeems to reſide in the ſalino-ſulphureous part of them, (and the like I have by chemical trials found in lime;) a practical inſight into the differences and differing operations of ſalts (about which I elſewhere entertain you) may probably very much aſſiſt the huſbandman to examine the ſeveral dungs, and other compoſts (the knowledge of which is of great moment in his art) and to multiply, compound, and apply them ſkilfully.

And as chemiſtry, that is converſant about fire, ſo even hydroſtaticks and hydraulicks, that teach us to make engines and contrivances for the lifting up, and for the conveying of water, may in divers places be of no ſmall uſe to the huſbandman. For not to mention what is done in ſome more known parts of the Eaſt, of the like nature with what I am going to mention, *Martinius* informs us, that in one province of *China* (whoſe name I remember not) they are ſo curious to water their fields of rice, that they have upon the river excellent mills ſo made, as that great quantities of water are continually raiſed in buckets, or other convenient veſſels, faſtened to vaſt wheels driven by the ſtream; which watering-mills (to add that notable inſtance upon the by) are not, as our European mills are wont to be, fixed to one place, but built upon veſſels, with which they may remove the mills, how great ſoever, from place to place, as occaſion requires. Nor is this eaſtern way of raiſing water by wheels, ſo as that it may be conveyed by convenient channels to places many foot higher than the river, or other receptacle of the water, that is to be diſtributed, the only way, whereby the hydrauliſt and mechanician may aſſiſt the huſbandman; ſince he may conſiderably do it by the art of libellation, or conducting of water upon the ground. For the improvement, that may be made of land by water, in ſoils fit for that way of culture, may be far more conſiderable, than is yet wont to be taken notice of; as indeed this huſbandry itſelf is in many countries both elſewhere, and in *England*, as yet unpractiſed. I have had ſome lands of my own much bettered by being ſkilfully overflowed; ſo that when I obſerved the difference, the tenant, though ſhy of acknowledging the utmoſt advantage, confeſſed to me, that he thought it yielded him double the former income. And a gentleman of quality of my acquaintance, whoſe improvements I went lately to view, ſhewed me a ſcope of ground, which at his firſt coming to that wild place, four or five years ago, was boggiſh, and which yet he had turned into a good dry ſoil, by only trenching it here and there with ſhallow trenches of not a foot deep, and overflowing it, by the means of thoſe trenches, and conveniently placed dams, as evenly as he could, five, ſix, or ſeven times a year, betwixt the beginning of October, and about the middle of April, with the water of a neighbouring ſpring, which was no way enriched by land-floods, ariſing but in a very barren and uncultivated place, far from the neighbourhood of grounds capable of enriching it; and yet this ſpring drained away, if I may ſo ſpeak, that ancient hydropical diſtemper of the land, and turned it, as I found by trial, into a good compact ſoil, on which ſtore of mowers were (when I ſaw it) employed in making of hay, which this meadow yeilded plentifully enough to be worth twenty times its former value. Nor is this the ſingle conſiderable inſtance we have met with, of the improvement that may be made of divers kinds of land, only by ſkilfully overflowing them with common waters.

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* Lib. IV. cap. 16. as he is publiſhed by *Purchas*.

BUT, *Pyrophilus*, I may hereafter have ſo many occaſions to mention particulars relating to agriculture, that I ſhould preſently diſmiſs them in this eſſay, were it not, that I am, by my having named huſbandry to you, put in mind to employ it as an inſtance to confirm this obſervation, that the more comprehenſive a trade is, the more likely it is, that it will be capable of being meliorated by natural philoſophy. For ſuch trades, as are of great extent, are obliged to deal with a conſiderable number of nature's productions, and to make uſe of divers of her operations; and conſequently muſt comprehend the more particulars, wherein the manufacture or profeſſion may be reformed, and otherwiſe advantaged by a knowing and dexterous naturaliſt. Thus the huſbandman's corn makes it fit for him to have a competent ſkill in the whole art of tillage, the keeping of cattle great and ſmall, the ordering of dairies, of wood, of flax and hemp, of hops, of the kitchen-garden, of an orchard, of bees, &c. beſides that the particular productions of ſome of theſe, as honey, cyder, &c. require ſome ſkill, and are capable of much improvement; ſo that among ſo great a variety of things, wherewith the huſbandman has to deal, it can ſcarce be otherwiſe, than that there will be ſeveral things, wherein the naturaliſt's higher and more reaching knowledge and experience will be ſerviceable to him. And whereas in the preſervation both of cattle from diſeaſes, and of the fruits of the earth from putrefaction, lieth one of the moſt beneficial and difficult parts of the huſbandman's ſkill, he may therein be much aſſiſted by an expert naturaliſt; who not only, by being able to accelerate putrefaction in divers bodies, may teach the huſbandman to furniſh himſelf with great variety of compoſts and manures, to relieve and enrich his ground with whatever peculiar ſort of ſalt he obſerves to be deficient; but alſo may teach him how to preſerve many of his ſeeds, and flowers, and fruits, beyond their wonted duration: as I know ſome perſons, to whom I recommended methods of this kind, that uſe to preſerve quinces, for inſtance, a great part of the year, by a ſtrong liquor, or pickle, made of nothing bur water, and what (for the moſt part refuſe ſtuff) may be eaſily obtained from the quinces themſelves. This way preſented us fruit at almoſt the year's end; and a while ſince I could have ſhewn you (and, for ought I know, can do ſo yet) cherries well ſhaped, and ſucculent enough, of above a year old, preſerved without ſalt or ſugar, by being kept in a ſpirit of wine fitted for that uſe, and fully impregnated, before their immerſion, with the tincture of the ſkins of other cherries of the ſame kind. The vaſt benefit, that the Hollanders derive from the beſt way of ſalting or pickling of herrings, and the advantageous uſe, that is made by others, of ſo powdering beef, and ordering other fleſh, that it will laſt good to the *Indies*, and is ſometimes brought uncorrupted into theſe parts again, may perſuade us of the benefit, that may accrue to the huſbandman, by the diſcovery of the ways of keeping the productions of the earth from corruption; eſpecially if his ſkill be extended to weak wines, cyder, perry, and other liquors, which are wont to be made in great quantities, and yet apt to decay at home, and unfit to be tranſported far abroad. And the uſe of ſugar to ſtrengthen vinous liquors, and make them durable; and, without the help of ſalt or any ſharp thing, to preſerve great variety of fruits, and of the juices of herbs, may encourage us to think, that there may be very differing ways (and ſome of them ſeemingly oppoſite) to make many things outlaſt their natural periods of duration.

BUT my trials and obſervations (whether about the conſerving of fruits, flowers, and fleſh, or of other things of this ſort) belonging more properly to another diſcourſe (of the preſervation of bodies) I ſhall now mention no more of them, but paſs on to tell you, that very much prejudice, which often happens to the poor huſbandman (and ſometimes even to his utter ruin) by thoſe, either ſtubborn, or contagious diſeaſes, (ſuch as the rot in ſheep, and the glanders in horſes,) that make havock of his cattle, may in great meaſure be prevented by the inſtructions of a knowing naturaliſt, eſpecially if he be an expert phyſician too. For, as many diſeaſes, ſo many cures are analogous in men and beaſts; and the remedies prove frequently more ſucceſsful in theſe than in them, as well for divers other reaſons, as becauſe the bodies of many brutes are more able to bear the operation of ſtrong remedies; and yet the unaccuſtomedneſs of almoſt all of them to phyſick makes them more relievable, than men by any (not improper) remedies. I will not now relate, that I have in ſome countries found medicines, that have been uſefully tried againſt diſeaſes in men, cried up for their efficacy againſt their analogous ones in horſes; nor with what difference in the doſe theſe may be purged by ſeveral of the ſame catharticks, eſpecially aloes, that are employed for the purgation of human bodies. I ſhall rather inform you, that as in theſe ſalt is, you know, reputed a great reſiſter of corruption, and an enemy to worms, (with a ſort of which the livers and neighbouring veſſels of ſheep have been obſerved to be infeſted;) ſo by the bare uſe of Spaniſh ſalt, which each ſheep, being firſt made to bleed a little under the eye, was made to take down a ſmall handful, two or three times, with ſome days of interval, without being ſuffered for ſome hours to drink any thing after it: by this remedy, I ſay, given at the time of the year when there is danger, that the ſheep will begin to be blotched, many flocks have for divers years been preſerved by a rich intelligent gentleman of my acquaintance, that is a great ſheep-maſter, and has thereby (and that alſo lately) preſerved his flocks in a moiſt country, when moſt of his neighbours loſt theirs. I might here mention to you, *Pyrophilus*, the virtues of crude antimony, to cure the foulneſs of blood, and even the leproſy in ſwine; of quick-ſilver to cure the worms in horſes; of *Palmarius*'s famous remedy, which he ſolemnly affirms to be a conſtant one againſt the bitings of a mad dog in cattle, and of a more parable one for men alſo, whoſe ſucceſs I almoſt admired in a near relation of yours and mine; of the uſe of the antimonial cup for ſeveral ſickneſſes in horſes and ſheep, which (if I miſremember not) was ſucceſsfully tried by one, to whom I recommended it; and of another antimonial medicine, which (though much commended to me by a virtuoſo, that took it himſelf) a gentleman of my acquaitance, reſident in the country, who prepares it, aſſures me, that he uſes it with ſtrange ſucceſs to fatten his horſes, (made lean by occaſion of ſickneſs) with whom yet it works not, either as an emetick, or a purge. And I could here preſent you divers other receipts much prized for their having (as well as the newly mentioned remedies) frequently been found effectual againſt the ſame diſeaſes both in human bodies and in brutes, if I did not think it leſs proper to make in this place a veterinarian excurſion, than to tell you, that, if you have any curioſity for them, you may command them.

I might add, if I had leiſure, ſome reaſons, why I deſpair not, that in time the huſbandman may, by the aſſiſtance of the naturaliſt, be able to advance his profeſſion by a therapeutical part, which may extend not only to the animal productions of the ground, and to the vegetable ones; but (in a large acception of the term) to the diſtempers of the ground itſelf. For if the cauſes of the barrenneſs of ſoils in general, and of their indiſpoſition to cheriſh particular plants or animals, were by the philoſopher's ſagacity diſcovered, I ſee not why many of thoſe defects may not be removed by rational applications, and proper ways of cure; as well as we ſee inconveniences remedied in many other inanimate bodies, without excepting the cloſe and ſtubborn metalline ones themſelves.

And perhaps alſo, that by a way of management ſuggeſted by the knowledge of cauſes the barrenneſs of a ſoil may be cured, or its fertility much promoted by methods, that do nothing near ſo much require coſt as ſkill. Some ingenious huſbandmen have of late proclaimed themſelves much ſatisfied with a way of correcting two of the barreneſt ſorts of land, not by rich manures or other coſtly cultures, but by ſkilfully mixing the ſand and clay themſelves in a due proportion, according to the uſe the huſbandman deſigns to make of it. And whereas one of the beſt modern writers of agriculture reports, as he may, for a ſtrange thing, that he had ſeen ſeven or eight and thirty ears of barley, that ſprung from one grain; I remember, that an ingenious gentleman, to ſatisfy ſome curious perſons what might be done in that kind, ſowed corn upon a piece of land, very near the place of my abode, which proſpered ſo ſtrangely, that one root, that I took particular notice of, though perhaps not the fruitfulleſt in the field, produced fifty and odd ears of corn; and yet, which was the ſtrangeſt, this wonderful increaſe depended upon a philoſophical obſervation; nothing extraordinary having been done, either to the land, or ſo much as to the ſeed; as I had opportunity to know, both by the informations of obſerving men, and by the confeſſion of the gentleman himſelf, who was pleaſed to make choice of me to intruſt his ſecret with, that, in caſe he died before me, the publick might not loſe it. Upon which account he alſo confided to me another ſpecimen of his ſkill. He once preſented your excellent mother a company of ſeveral ſorts of choice apples, among which there was one ſort excellently taſted, but very ſmall; the following year he preſented her another baſket of the like fruit, but finding no ſmall ones among them, ſhe took occaſion to aſk him, what was become of the tree, that produced thoſe delicious little apples, that made part of his former preſent? to which he replied, that he had brought ſeveral of its productions among the other fruits, ſhe was looking on; and thereupon ſhewed her ſome, that came from the ſame tree, and appeared by the peculiar reliſh to be of the ſame ſort, though exceedingly differing in bulk, that neither your mother, nor I, had any ſuſpicion, that the ſame tree bore them. Upon which occaſion he readily gratified my curioſity by acquainting me with his way, which depended almoſt only upon a phyſical obſervation; all that he added being not any rich compoſt, but ſome deſpiſed leaves of a very cheap and common vegetable. But huſbandry is too large a ſubject for me to proſecute in this place, and therefore I ſhall here diſmiſs it.

SECTION III.

THE next thing I ſhall obſerve to you, *Pyrophilus*, is, that it is not only to the trades, that miniſter to the neceſſities of mankind, but to thoſe alſo, that ſerve for man's accommodation or delight, that experimental philoſophy may bring improvements; for theſe arts alſo do, for the moſt part, conſiſt in the knowledge and application of ſome of nature's productions and courſes, whoſe being referred to the accommodation or delight of men, rather than to any other purpoſe, does produce nothing, that is truly phyſical in the things ſo referred, which thereby acquire only ſuch a kind of reſpect to man, as that which the metaphyſicians call an extrinſical denomination; and we ſee that the ſame things, without varying their nature, are ſerviceable to men in very differing capacities: as wine ſerves one, that is dry, to quench his thirſt, ſerves a fainting perſon to revive his ſpirits, and the drunkard to inebriate him; the ſame ſpirit of wine, that ſerves the phyſician to make tinctures and extracts for the recovery of health, may ſerve the ladies to diſſolve benjamin into a tincted liquor, that diluted with fair water may be uſed as a coſmetick, which I have received many thanks for; and the ſame ſpirit ſkilfully employed upon ingredients, to be named to you ere long, is of excellent uſe for making divers fine varniſhes made with rectified ſpirit of wine; nay, the newly mentioned ſolution of benjamin may it ſelf be applied to all thoſe differing uſes; for of it ſelf it is a pretty and odoriferous varniſh, and I have uſed it (though not often, for want of opportunity) with very good ſucceſs againſt a ſort of tetters, which I cauſed frequently to be bathed with it. What happy applications knowledge and ſkill may make even of unpromiſing things, to the furniſhing men with delights, is methinks very evident in muſical inſtruments, as lutes, viols, &c. For who would think (if experience did not aſſure us of it) that with a few pieces of wood joined together, and the guts of cats or lambs wreathed or twiſted into ſtrings, the ſkilful muſician, by the help of mathematicks and exerciſe, ſhould be able to charm the ear with the greateſt, as well as moſt innocent delights, the ſenſe belonging to the organ is capable of, and which ſometimes does not only pleaſe, but raviſh the tranſported hearers. But though, *Pyrophilus*, as I was lately ſaying, phyſicks may not only be very improving to thoſe arts and profeſſions, that ſerve to provide man with the neceſſaries or accommodations of life, but alſo to thoſe, that ſerve chiefly to furniſh him with pleaſures and delights; as might be inſtanced in experiments of colouring, perfuming, making ſweet-meats of all ſorts, embelliſhing the face with coſmeticks, and divers others of the like voluptuous nature: and though I may elſewhere have occaſion, when I come to treat of colours, odors, taſtes, and other qualities, to acquaint you with ſome receipts and experiments of this kind; yet now I do not only want leiſure to mention them, but am deſirous, that natural philoſophy ſhould engage you to court her, rather by her gratifying and enamouring your reaſon, than by her bribing and inveigling your ſenſes.

SECTION IV.

THOUGH what has been repreſented about the uſefulneſs of experimental philoſophy to trades does chiefly belong to thoſe, wherein nature's productions are employed to human uſes, by thoſe operations, wherein nature her ſelf, rather than the artificer, ſeems to have the chief hand, as the trades of brewing, baking, gardening, tanning, &c. yet I would not exclude thoſe very trades, wherein the artificer ſeems to be the main agent, and in whoſe ultimate productions the chief thing, that is wont to be conſidered, is the adventitious ſhape or form, which the artificer, as an intelligent and voluntary agent, does, by the help of his tools, give the matter he works on, as in the trades of the ſmith, the maſon, the cutler (when diſtinct from that of the ſword-maker,) the watch-maker, and other handicrafts. For though theſe conſiſt rather in the manual dexterity of men, than the ſkilful ordering of the productions of nature, by their material operations upon one another; yet to many, if not all, even of theſe, the naturaliſt may ſome way or other be a benefactor.

FOR there are divers of theſe manual trades, that, eſpecially as they are exerciſed in cities and greater towns, conſiſt of ſeveral parts, and have need of ſeveral other trades to prepare materials for them, and diſpoſe them to receive the laſt form, which the artificer is to give them, to fit them for ſale. And we may, in many caſes, obſerve, that though this artificer, that gives the matter this laſt form, does it chiefly with his hands and his tools; yet thoſe other tradeſmen, to whom he is beholden for his materials, do ſome or other of them, to prepare and qualify them for his uſe, need ſome obſervations of the conditions of the body they deal with, or muſt imploy ſome phyſical operations, wherein they may be much aſſiſted by the knowing naturaliſt, who may alſo teach the manual operator himſelf, how to make choice of his materials, and examine the goodneſs of thoſe, that ſubordinate workmen ſhall bring him. Thus, though ſtone-cutting be a trade, that ſeems to conſiſt almoſt wholly in giving, with proper tools, to marble, freeſtone, and other materials, the ſhape, which the artificer deſigns; yet, if I had leiſure, I could eaſily ſhew you, that even in this trade, there are many particulars, wherein experimental philoſophy might be helpful to the artificer. For ways, hitherto unuſed, may be found out (as I have partly tried) to examine the nature and goodneſs of the marble, alabaſter, and other ſtones, which the mechanicks deal with. A competent knowledge of the ſap, that is to be found in ſtones imployed for building, is of ſo much importance, that the experienced maſter workmen have confeſſed to me, that the ſame ſort of ſtone, and taken out of the ſame quarry, if digged at one ſeaſon, will moulder away in a very few winters; whereas digged at another ſeaſon, it will brave the weather for very many years, not to ſay, ages: (but of my obſervations of this kind, more elſewhere.) The cements alſo, and ſtoppings (as they call them) which are of good uſe in this trade, may be eaſily bettered by the naturaliſt, that is verſed in ſuch mixtures. And I remember, I had occaſion to teach a fine cement for the rejoining of the broken limbs of ſtatues to their bodies, to an inquiſitive artificer, who, by ſuch like helps, did in other caſes, ſo well counterfeit marble with a cement, that even where there was occaſion to fill up great cavities with it, the work would paſs for entire; the additaments being not diſtinguiſhed from the natural marble. Want of curioſity alſo keeps our ſtone-cutters here in *England* unacquainted with the ways of working upon porphyry, which they will not undertake either to poliſh or to cut. Nor is *England* the only country, where the art of working upon porphyry (which appears to have been in great uſe amongſt the *Romans*) is unknown, though at *Rome* there are ſome few, that do with great gain exerciſe it. And though I know not preciſely, what it is they imploy; yet I preſume, it may be powder of emery: for with that, and water, and ſteel-ſaws, I have here in *England* cauſed a porphyre-ſtone to be cut. And the mention of porphyre puts me in mind of telling you, that by an art I have, white marble may be ſo ſtained, and that durably, with ſpots great or ſmall, and red or brown, as it pleaſeth the artificer, as I may hereafter have occaſion more fully to relate. It would be too long to diſcourſe to you here of artificial marble, and divers other things, that ſtone-cutters affirm to belong to their trade, wherein you will ſcarce doubt, but that it may be capable of improvement. Wherefore I ſhall only add, that whereas this profeſſion does much require very good ſteel-tools, and they muſt have theſe from ſmiths, and others that deal in iron, if theſe men's trade were bettered by the naturaliſt, they might be able to afford the ſtone-cutter the better tempered tools: and that even the ſmith's craft, though it ſeem to be merely a manual art, is yet capable of much melioration by the knowledge of nature, were not difficult to manifeſt, if it were proper here to inſiſt on the proofs of it; yet thus much I ſhall here take notice of, to confirm this fourth obſervation, that not only the philoſopher may, as a mineraliſt, and a mechanician, improve the ways of making iron and ſteel, before they come to the ſmith's hand, but likewiſe may deviſe better expedients, than are among us in uſe, for the ordering of iron and ſteel, when it comes to be formed into weapons and tools. The ſword-blades, and other arms, that are made at *Damaſco*, are very famous every where, and (as far as ſome trials have informed us) juſtly for their excellency in cutting even iron. And yet it ſeems to be only the ſkill of the artificers in ordering it, that gives the ſwords, and other inſtruments made at *Damaſco*, ſo great a preheminence above others. For though the goodneſs of them have been preſumed to proceed from that of the iron-mines, and ſteel, peculiar to the region of that city; yet the judicious *Bellonius*[*], having made particular enquiry at his being there, informs us otherwiſe, and tells us, that iron and ſteel, being brought thither from other parts, (the country having no mines of it) receives there from the ſkill of the workmen its temper and perfection. And I ſee not, why I may not reaſonably ſuppoſe, that in the tempering of ſteel, it is not only the goodneſs of the metal, and the determinate degree of heat, though theſe be the only things artificers are wont to look after, that give the beſt temper; but that much may depend upon the nature of the liquors, or other bodies, wherein the hot ſteal is plunged, and upon other ways of ordering it, if thoſe be ſkilfully choſen and imployed. I have had a graver ſo well tempered, (but by whom I know not) that all the known ways uſed by me and others, (who wondered, as well as I, at the unſucceſſfulneſs of our endeavours,) could not deprive it of its temper, as they would have done any gravers, that we make here; and it was afterwards affirmed to me, that it was made of ſteel tempered at *Damaſco*.

I may elſewhere tell you, *Pyrophilus*, both of a way I have tried, of hardening gravers, without quenching them in any liquor or tallow, or any other unctuous body; and that having perſwaded an ingenious artificer to try an unpractiſed way of tempering gravers, he ſoon after brought me one to ſee the goodneſs of it, which, by being plunged in a certain cheap mixture, (wherewith I may hereafter acquaint you) had been hardened and tempered at once: which though moſt artificers would think ſcarce poſſible, yet upon the authority of trial I ſhall venture to deliver, what ſome may think as ſtrange, namely, that though ignition and extinction in cold water, be the common and known way to harden ſteel gravers; yet by that way, only obſerving preciſely a nick of time, ſteel may be made ſtrangely ſoft. But of this more elſewhere. I ſhall now add, that having enquired of one of the curiouſeſt, and moſt obſerving makers of ſteel-tools, whether he did not find a difference in the employing of pump-water, or river-water, in giving them their temper, he ſatisfied me, that he did ſo; and obſerved the former to be fitter for ſome ſorts of tools, and the latter for others. There may be divers other particulars, wherein iron and ſteel may be improved by the naturaliſt. The firſt may be this; that the metal be rendered ſo ſoft, as to be, by the help of ſtrong moulds, put into ſhapes. This an eminent and credible artificer aſſured me, he had often ſeen his maſter do to iron, with conſiderable profit. Or elſe it may be made fuſible like another metal, as I remember I have (ſometimes, with a certain flux-powder, which I compoſed, if I much forget not, of tartar, ſulphur, and arſenick) made it run, even with a charcoal fire, into a maſs exceeding hard, and very poliſhable. A third way may be this; that it be ſo ordered, as to be preſerved very long from ruſt, which an ancient virtuoſo, who had purchaſed the ſecret of a rare artiſt for a great prince, and uſed to ſhew his friend's ſteel ſo prepared, aſſured me, was done chiefly by tempering it in water well impregnated with the bark of a certain tree. In a word, there may be divers other ways, whereby iron or ſteel themſelves, or their trades, that imploy them, may be meliorated; and to add, that on this occaſion, there are many and very differing accounts, upon which a trade or profeſſion may be benefited by the Experimental Philoſopher: for he may either find out variety of materials, wherewith to perform the things deſired by the tradeſman, or he may render thoſe materials, that are already in uſe, better conditioned; or he may diſcover and reform the unheeded errors and miſtakes to be met with in the trade; or he may deviſe more eaſy and compendious ways of producing the effect that is required; or he may improve ſome of the auxiliary trades, of which the trade ſpoken of has need or uſe; or may inſtruct the artificer to chooſe, and examine, and preſerve his materials and tools, better than is uſual, or can

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[*] P. Bellonius obſervat. Lib. II. cap. 93.

make the ultimate productions of his trade ſooner, or cheaper, or eaſier, or better conditioned, or applicable to more uſes, or more durable, than they are commonly made. Nor are theſe all the particulars, that might here be enumerated to the ſame purpoſe, if this fourth conſideration had not detained us too long already.

SECTION V.

THE naturaliſt may increaſe the power and goods of mankind upon the account of trades, not only by meliorating thoſe, that are already found out, but by introducing new ones, partly ſuch as are in an abſolute ſenſe newly invented, and partly ſuch as are unknown in thoſe places, into which he brings them into requeſt. For it were injurious both to nature and to man, to imagine, that the riches of the one, and the induſtry of the other, are ſo exhauſted, but, that they be brought to afford new kinds of employments to the hands of tradeſmen, if philoſophical heads were ſtudiouſly employed to make diſcoveries of them. And here I conſider, that in many caſes, a trade differs from an experiment, not ſo much in the nature of the thing, as in its having had the luck to be applied to human uſes, or by a company of artificers made their buſineſs, in order to their profit; which are things extrinſical, and accidental to the experiment itſelf. To illuſtrate this by an example, the flaſhing exploſion made by a mixture of nitre, brimſtone, and charcoal, whilſt it paſt not farther than the laboratory of the monk, to whom the invention is imputed, was but an experiment: but when once the great (though unhappy) uſe, that might be made of it, was taken notice of, and mechanical people reſolved to make it their profeſſion and buſineſs, to make improvements and applications of it; this ſingle experiment gave birth to more than one trade; as namely, thoſe of powder-makers, founders of ordnance, gunners (both for artillery and mortar-pieces,) gunſmiths; under which name are comprized ſeveral ſorts of artificers, as the makers of muſkets, ſmall piſtols, common barrels, ſcrewed barrels, and other varieties not here to be inſiſted on.

THE diſcovery of the magnetical needles property to reſpect the poles, has given occaſion to the art of making ſea-compaſſes, as they call them, which in *London* is grown to be a particular and diſtinct trade. And divers other examples may be given to the ſame purpoſe; eſpecially where mechanical tools and contrivances co-operate, with the diſcovery of nature's production. So that oftentimes a very few mathematical ſpeculations, or as few phyſical obſervations, being promoted by the contrivance of inſtruments, and the practice of handicrafts men, are turned into trades; as we ſee, that a few dioptrical theories lighting into mechanical hands, have introduced into the world the manufactures of ſpectacle-makers, and of the makers of thoſe excellent engines, teleſcopes and microſcopes.

THE obſerving, that though quick-ſilver will amalgame with gold (and thereby ſeem to be deſtroyed,) which made *Pliny* think it an enemy to metals,) yet it may be ſeparated from the gold again, without diminution of that noble metal) has brought forth the trade of guilders, whoſe art conſiſts chiefly in mixing, by the help of a competent heat, good gold with five, ſix, or ſeven times its weight of quick-ſilver, until the mixture come of ſuch a conſiſtence, that they may ſpread it as they pleaſe upon the ſilver or copper to be gilt. For having by this means overlaid it evenly with gold, they can eaſily with fire force away the mercury; and, with a liquor impregnated with nitre, verdigreaſe, ſal armoniack, and other ſaline bodies, which they call a colouriſh, reſtore its luſtre to the remaining gold, which they after make bright by poliſhing.

THE almoſt obvious and trivial obſervation made by ſome ſagacious perſon, (whoever it was) that a ſpring was a phyſical, continual, and durable power or force, and the corollary he thence deduced, “that this force, ſkilfully applied, might be equivalent to the weights, that were thought neceſſary to move the wheels of clocks:” theſe reflections, I ſay, joined with a mechanical contrivance, produced thoſe uſeful little engines, watches, that now afford a plentiful livelihood to ſo many dexterous artificers; which though cuſtom has made familiar to us, yet were unknown to the ancients, and highly prized and admired in *China* itſelf, when firſt (in the laſt century) brought thither. The diſcovery of the virtue of aqua fortis, to diſſolve ſilver and copper without working upon gold, added to the obſervation, that lead melted with either of the two noble metals, and then forced from them by fire, will carry away with it any of the baſer metals, that may have been mixed with them; theſe two particulars, I ſay, have begot in latter ages the art of the refiners we now have.

MEN's having obſerved the operations of ſome lixiviums, clays, and a few other familiar things upon the juice of the ſugar-cane, has not only occaſioned the adding of the culture of thoſe reeds to the other parts of huſbandry, left us by the antients; but has produced the ſeveral trades of ſugar-boilers, or makers of ſugar, refiners of ſugar, and confectioners: not to mention the great addition the concreted juice of the ſugar-cane brings to the apothecaries profeſſion, upon the ſcore of ſyrups, conſerves, electuaries, and other ſaccharine medicines. Nay, a very ſlight manual contrivance, or operation, if it light fortunately, may ſupply men with a trade, as in the art of printing. To which I ſhall only add, that in *China*, and ſome other Eaſtern parts, the lucky trial, that ſome made to bore very ſmall holes through *Porcellane* or *China* cups, and employ very ſlender wire inſtead of thread or ſilk, has given being to the vulgar trade of thoſe people, that go up and down in thoſe countries, as tinkers do with us, getting their livelihood by ſewing together the pieces of cracked or broken *Porcellane*

lane veſſels; as I have been informed by more than one credible perſon, that lived in the Eaſt, and had experience of the uſe of cups ſo mended, though filled with liquors as hot, as they are wont in the Eaſt to drink their coffee and tea.

The mention freſhly made of *China* brings into my mind, that whereas the knowledge of ſome gums and liquors in that country afforded thoſe uſeful, as well as moſt beautiful, varniſhes, which we call by the name of the kingdom, that ſupplies us with them, and which do both there, and in *Japan*, employ multitudes of tradeſmen; I am credibly informed, that the art of making the like varniſhed wares is now begun to be a trade at *Paris*, and I doubt not but it will before long be ſo in *London* too. For though ſome accounts, that were given me by virtuoſi of that varniſh, were ſuch, that the trials of them did very ill anſwer expectation; yet having read in *Linſchoten*'s voyages, that in *China* and *Japan* they make this excellent varniſh of gum lacca, I found by ſome trials, that I was able to imitate one of the beſt ſorts of it, by diſſolving the gum in high rectified ſpirit of wine,* and then giving it a colour, and laying it on in ſuch a manner, as I may have before long a fitter occaſion to inform you.

And without much impropriety, I might alledge the art of cultivating and gathering ſugar-canes, and of ordering their juice, as a recent inſtance of the tranſplanting of arts and manufactures. For, as I am informed by very credible relations, there are not very many years effluxed, ſince, in our memory, a foreigner accidentally bringing ſome ſugar-canes, as rarities, from *Braſil* into *Europe*, and happening to touch at the *Barbadoes*, an Engliſh planter, that was curious, obtained from him a few of them, together with ſome hints of the way of cultivating and and uſing them. Which, by the curioſity and induſtry of the Engliſh colony there, were in a ſhort time ſo well improved, that, that ſmall iſland became, and is ſtill, the cheif ſtore-houſe, that furniſhes, not only *England*, but *Europe*, with ſugars. And this inſtance I the rather mention, becauſe it is alſo a very notable one, to ſhew, how many hands the introduction of one phyſico-mechanical art may ſet on work; ſince I have had particular opportunity to learn by enquiry, that the negroes, or, as they call them, blacks, living as ſlaves upon that ſpot of ground, and employed almoſt totally about the planting of ſugar-canes, and making of ſugar, amount at leaſt to between five and twenty and thirty thouſand perſons. And, that you may ſee how lucriferous in that place this recent art of making ſugar is, not only to private men, but to the publick; I ſhall add, that by divers intelligent and ſober perſons intereſted in the *Barbadoes*, (and partly by other ways) I have been informed, that there is, one year with another, from that little iſland, which is reckoned to be ſhort of thirty miles in length, (and ſo I found it, by meaſuring it on one of the faireſt and recenteſt maps,) ſhipped off for *England*, eſpecially, ten thouſand ton of ſugar, each ton being eſtimated at two thouſand pound weight, which amounts to twenty millions of pounds of that commodity; which though it may ſeem ſcarce credible, yet one of the antient magiſtrates of that iſland, lately aſſured me, that ſome years it affords a much greater quantity.

I ſhall not fortify what I have hitherto diſcourſed with particulars, that will elſewhere more properly fall in; it being ſufficient for my preſent purpoſe, that the inſtances already mentioned may render it probable, that the experimental philoſopher may not only improve trades, but multiply them, till I have occaſion in the laſt eſſay of this book, to make it out more fully. Nor do I deſpair, that among other ways, whereby trades will be increaſed, one may be the retrieving ſome of thoſe, that were anciently practiſed, and ſince loſt; of which we have a catalogue in the learned *Pancirollus*. For as it is the ſkilful diver's work, not only to gather pearls and coral, that grew at the bottom of the ſea, and ſtill lay concealed there; but alſo to recover ſhipwrecked goods, that lay buried in the ſeas, that ſwallowed them up: ſo it is the work of the experimental philoſopher, not only to dive into the deep receſſes of nature, and thence fetch up her hidden riches; but to recover to the uſe of man thoſe loſt inventions, that have been ſwallowed up by the injuries of time, and lain buried in oblivion. This I do not ſay altogether groundleſly, though for ſome reaſons I here decline mentioning the things, that induced me to ſay it.

SECTION VI.

TO what has been hitherto ſaid I ſhall venture to add, not only, that the ſagacious philoſopher may better moſt of the trades, that are already in uſe, and add to the number of mechanical employments; but that I am apt to think it might, without much hyperbole, be affirmed, that there is not any one profeſſion or condition of men (perhaps ſcarce any ſingle perſon of mankind) that may not be ſome way or other advantaged or accommodated, if all the truths diſcoverable by natural philoſophy, and the applications, that might be made of them, were known to the perſons concerned in them. So that beſides thoſe diſcoveries, that are compiled or formed into trades, there are, and may be, found a multitude of looſe particulars, whereby the naturaliſt may much gratify and aſſiſt men, according to the exigency of particular occaſions. The nature of the thing will ſcarce permit me to illuſtrate ſo unlikely an aſſertion, without employing inſtances in themſelves trifling, if not deſpicable; of which I will therefore give you but a few, becauſe, if they were not pertinent to my preſent purpoſe, they would be fitter to divert, than inform you.

I had, not long ſince, the honour to be known to a very great court-lady, who was much troubled, that having frequent occaſion to write letters, ſhe could ſcarce handle a pen

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* See the Appendix to Eſſay V.

without blacking her fingers with ink. I ſmilingly undertook to make her write without ink, which I my ſelf was formerly wont to do, by firſt preparing my paper with a powder made of copperas, ſlightly calcined upon a fire-ſhovel, till it grow friable, and galls, and gum-arabick finely pulverized, and exquiſitely incorporated with the vitriol in a certain proportion; which though a few trials will better teach than rules, (becauſe, according to the goodneſs and calcination of the vitriol, the proportion of the other ingredients muſt ſometimes be varied,) yet to aſſiſt you in your firſt gueſſes, I ſhall tell you, that, for the moſt part, I uſed my ſelf three parts of calcined vitriol, two parts of galls, and one part of gum-arabick, and mixed them not before I was ready to employ them; for this powder being with a hare's foot, or any other convenient thing, carefully rubbed into the paper, and the looſer duſt ſtruck off, doth, without diſcolouring it, ſo fill its pores with an inky mixture, that as ſoon as it is written upon with a clean pen, dipped in water, beer, or ſuch other liquors, the aqueous part of the liquor diſſolving the vitriolate ſalt, and the adhering particles of the galls, makes a legible blackneſs immediately diſcover itſelf on the paper. This mention of writing brings into my mind, that ſeveral times having had occaſion to make a word or two, that was but lately written, look as if it had been written long before, I performed it, by lightly moiſtening the words I would have to look old, with oil of tartar per deliquium, allayed with more or leſs fair water, according as I deſired the ink ſhould appear leſs or more decayed: which experiments may be often uſeful in manuſcripts, to keep the recent interlineations, or other additions, from betraying themſelves by their freſhneſs not to have been written at the ſame time with the reſt of the manuſcript.

AND the deſign I had in making uſe of the lately mentioned powder of galls and copperas puts me in mind of another way of writing without ink, (and too without danger of blacking one's fingers or linnen,) which I remember I have practiſed, ſometimes with one powder, and ſometimes with another. For conſidering, that common ſilver being rubbed upon bodies, whoſe ſurfaces are a little rough, and even upon coloured cloth, the metal would leave a blackneſs on it, it was eaſy to conclude, that if the ſurface of the white paper were aſperated by a multitude of irregular grains of a powder as white as it, would retain a blackneſs, wherever a blunt ſilver bodkin ſhould be drawn over the grating particles: and accordingly I found, that either exquiſitely calcined hartſhorn, or clean tobacco-pipes, or (which is better than that) mutton-bones (taken between the knuckles, and) burnt to a perfect whiteneſs, being finely powdered and ſearſed, and well rubbed upon paper, would make it fit to be written upon with the point of a ſilver table-book pin, or bodkin of ſilver (which metal is not abſolutely neceſſary in this caſe,) as well as that, which is called mathematical paper, (if the being prepared with one, or other of theſe powders, do not make it the ſame.)

AND now I am upon the mention of ſuch preparations of paper, I remember, that I was once in a place, where I could get no white leaves, to ſupply a fine table-book, that I had much uſe for; nor could I hear of any tradeſman in the whole country, that knew the way of making ſo much as ordinary table-books: wherefore I bethought my ſelf of trying to make ſomething by way of ſuccedaneum, which ſucceeded at the firſt attempt. And though there may be better ways to make white table-books, yet perhaps you will find none more ſimple and eaſy; the two only ingredients we had in it, being to be had at every apothecaries ſhop. I only take ceruſs, rubbed to very fine powder, (which is done in a trice) and temper it up with fair water glutted with clear gum-arabick. With this mixture (being brought to the conſiſtence of a ſomewhat thick ſalve) I rub over the paper I prepare, putting on more or leſs, according as I would have it laſt; and having ſuffered it to dry (which it will quickly do) it may, if there be occaſion, be preſently uſed with the point of a ſilver-pin, which will make the letters appear very conſpicuous upon a mixture, that does not at all impair the whiteneſs of the paper; and what was thus written I could, with ſpittle or water, blot out three or four times ſucceſſively without ſpoiling the paper. Which queſtionleſs had been much better prepared, if divers couches of the mixture had been laid on, and ſuffered each to dry, and if afterwards the paper had been ſmoothed by being ſcraped with a knife, and poliſhed.

A very ingenious artificer, who had contrived an inſtrument uſeful to others, and profitable to himſelf, whereof an abſolute neceſſary part was a glaſs filled with fair water, and exactly ſtopped, complained to me, that though his inſtrument did exceeding well in all but froſty weather, yet then it was apt to be ſpoiled by the freezing of the included liquor, which too often broke the glaſs. Whereupon I taught him to remedy it, by ſubſtituting, inſtead of water, good ſpirit of wine, which has not in our climate been obſerved to freeze; or rather, (becauſe in his bigger glaſſes, that liquor would be chargeable) either ſea-water ſtrengthened with a little ſalt, or elſe common ſpring-water with a twentieth, or at moſt a tenth part of ſalt diſſolved in it. For though this brine look, if well made, as clear as common water, yet I have not obſerved, that the ſharpeſt of our Engliſh winters would make it freeze.

TO a perſon of quality, that was very curious of the way of writing ſecretly, I undertook to teach an eaſy way (which after I knew it, I found alſo in an old printed book) of ſending a written meſſage, without putting it into the power of the bearer to betray it; which I could eaſily have performed my ſelf, if the meſſage were to be delivered in a ſhort time, and not too far off, by writing on his back, or other convenient part of his body, with a clean pen dipped in my own urine, (there being ſome urines, with which I have found, to my wonder, that the experiment would not ſucceed.) For if he, that receives the meſſage, rubs but a little of the black ſubſtance remaining of paper after it is burnt, thoſe ſable parts adhering to thoſe other of the liquor, that lurk yet in the pores of the ſkin (whence, if the meſſenger went faſt, and very faſt, the ſweat would probably diſlodge them) do denigrate all that was written, and make it legible enough, ſometimes, as I have tried, after many hours.

I remember too, that intending one ſummer to make ſome abode at a houſe I had in the country, I ſent for from *London*, among other things, a quantity of damaſk table-linnen, with which he that ſent it me, inconſiderately packed up a great pot of a certain confection, which, for ſome purpoſes, I had cauſed to be made of the pulp of ſloes, which, by agitation of the horſe it was carried on, being brought to ferment, and run out of the broken pot, ſtained all the new damaſk from the top to the bottom. At which an old domeſtick of mine (whom you remember very well) ſeeming much troubled, becauſe he had ſent for it, to convince him, that experimental philoſophy was not altogether uſeleſs, I ſteeped the ſtained linnen, for ſome convenient hours, in new milk; and afterwards cauſing it to be throughly and diligently waſhed in the like liquor, the damaſk came forth unſtained, and almoſt as white as it. What urine, if duly and long enough employed, may do to take ſtains, even of ink, out of linnen, is but to be hinted in this place; where I might add, that with ſtrong ſpirit of ſalt, wherewith I moiſtened, as often as was needful, the ſpotted places (firſt wetted with fair water) I have out of new linnen taken ſpots of ink (eſpecially freſh ones) of very differing ſizes, without leaving (after the linnen was well waſhed out in fair water) any of thoſe yellow ſtains which many call iron-moles.

Some ingenious perſons, that deal much in lixiviums and brines, complaining the other day, that beſides that they could not ſometimes eaſily come at an egg, to try, by its ſinking or floating, the ſtrength of the ſaline liquors they would examine, there needed a good quantity of liquor to make ſuch a trial in, I allowed their complaint to be juſt, and the rather, becauſe I obſerve, for nicer eſtimates of the ſtrength of liquors, the trial by eggs is uncertain enough, in regard, that even the ſame egg will, as I have found, by being kept, grow lighter, whence ſtale eggs have uſually a great cavity (that ſeems filled only with air) at the bigger end: and I told them, to omit the more artificial, but more difficult, ways of examining ſuch liquors, I ſometimes uſed a way, whereby I could try the ſtrength of the lixiviums made with chemical ſalts, though I had not above a thimbleful of the liquor, and this with a body, that will not eaſily waſte like an egg, and therefore may be kept. For I ſubſtituted, inſtead of the egg, a ſmall piece of amber, about the bigneſs of a pea, which in a very ſtrong ſolution of lixiviate ſalt, will, as I let them ſee, ſwim on the top, but ſink in a weak one. And as you may take a piece of amber, leſs or bigger than a pea, as beſt fits your occaſions, and need not be at all ſcrupulous about the figure, (provided the amber be once well ducked in the liquor;) ſo it is ſome convenience, that two pieces of amber, whereof the one is far more reddiſh, and the other paler, will be, as far as I have tried, of ſomewhat differing ſpecifick gravities, ſo that the one will float in ſome liquors, wherein the other will ſink.

I remember I was once in a country, where I had a great mind to try ſome things with Dantzick vitriol, or ſome other blue copperas, but, by reaſon of the wars, could not poſſibly procure any, though there were in that country a place, where green vitriol was made by the help of iron: wherefore getting ſome of that liquor, which the rain had waſhed from the copperas ſtones, I did, by putting into it a convenient quantity of copper, reduced into ſmall parts, make the newly mentioned liquor, ſerve for a menſtruum to work upon the metal, and by exhaling the ſolution to a due conſiſtence, I obtained the blue venereal vitriol I deſired. And the like, I doubt not, may be done with ſuch of thoſe common green vitriols made of iron, wherein the ſaline part is not too much ſatiated with the martial.

An ingenious and well known perſon, that is a great dealer in cyder, coming to viſit me, and expreſſing a great deſire to be able to make ſome, that would be ſtronger, and thereby likelier to keep longer than the ordinary way, I extempore directed him to an unuſual courſe, for which he afterwards came to give me ſolemn thanks. The way was to take the ſtrained juice of apples, and in ten or twelve gallons thereof to ſteep for 24 hours (more or leſs) about two buſhels of the ſame kind of apples groſly bruiſed: the apples being lightly expreſſed, the infuſion was (with freſh) repeated once more, (care being to be taken, that the infuſion be not made too ſtrong and thick, which may hinder the ſeaſonable clarification of the liquor.)

It was not perhaps difficult to mend this preſcription; but I give you the account of it, as I received it from him, becauſe he aſſured me, that none of his many trials had furniſhed him with cyder ſo well bodied, and ſo much applauded. The cautions, that belong to this practice, and the various applications, that may be made of this way of making vinous liquors of fruits, without additions (ſo much as of water,) by infuſion, and the varyings of the experiment according to particular caſes, I muſt not here ſtay to mention.

It was not long ſince, that accidentally rummaging in a dark place, where I had not of a long time been, and where unknown to me ſome chemical glaſſes, negligently ſtopped, and not written on, had been put; one of them falling down made two or three great ſtains in the conſpicuouſeſt part of a new ſuit I had then on; and would have obliged me to leave it off, but that judging by the nature of the ſtain, that it was made with ſome acid ſpirit, I tried, by ſmelling to them, whether among the other bottles, one or other had not ſome urinous, or otherlike ſpirit; and lighting on a liquor, which, though I know not what it was, I gueſſed by the ſtink, to abound with volatile ſalt, I bathed the ſtained parts well with it, and in a trice, reſtored them to their former colour. And, by a like way, I have preſently remedied the diſcolorations made by ſome ſharper and fretting liquors, of died garments of other ſorts and materials, which thoſe blemiſhes would elſe have rendered altogether unfit for wearing.

ANOTHER time, diſcourſing with a ſtateſman of the ways whereby well-meaning perſons may be injured and defamed, I undertook, that out of a parchment-writing, with his hand annexed, I would take out all, that was written above his name, without ſpoiling or diſfiguring the parchment, on which I would afterward write what I pleaſed, and whereby I might make people believe, that he had acknowledged under his hand ſuch things, as never came into his thoughts. And to ſatisfy him of the poſſibility of this, I did in a few minutes take off from the parchment all, that was written on it, without defacing the parchment. Some attempt to free paper from what is written upon it, with aqua fortis, but, that by diſcolouring the paper, makes men apt to ſuſpect ſome intended deceit. And for the true way of performing ſuch an effect, and divers others of the like nature, which I have ſometimes for curioſity proſperouſly experimented, I think it much fitter to be concealed than communicated; becauſe if ſuch ſecrets ſhould fall into the hands of perſons inclined to miſ-apply them, they might very much diſturb human ſociety. And therefore it is better men ſhould want the light afforded them by ſuch experiments, than be brought into the danger of ſuch miſchiefs, as they may be made to ſuffer by the miſ-imployment of ſuch diſcoveries.

I remember, that not long ſince, a virtuoſo happening to have made a ſolution of gold, wherewith he thought to make aurum fulminans, thought he had cauſe to ſuſpect, that it had been enbaſed with copper, and therefore would not be ſo fit for his work; whereupon I conſidered with my ſelf, that a good urinous ſpirit being imployed inſtead of the uſual menſtruum (oil of tartar,) as it would precipitate gold out of aqua regis, ſo it would readily diſſolve copper, I conjectured, that by the affuſion of ſuch a liquor I might both diſcover, whether the ſolution (whoſe colour did not at all accuſe it) contained any copper, and if it did free the gold in great part from the baſer metal: and indeed I found, that after the urinous ſpirit had precipitated the gold into a fine calx, the ſupernatant liquor was highly tinged with blue, that betrayed the alloy of copper, that did not before appear.

I hope you think, *Pyrophilus*, that it is becauſe theſe inſtances are more pertinent to my deſign, than many others (that might have been ſubſtituted) in themſelves more valuable, that I have mentioned ſuch inconſiderable ones; and I ſhall not repent the naming of ſuch inſtances, if they have let you ſee, that even mean experiments are not to be deſpiſed, but, that the meaneſt may be ſometimes, not only uſeful, but more proper to convince ſtrangers to natural philoſophy of the manifold uſes of it, than experiments of a higher and abſtruſer nature. For as in a ſhipwrack, it may more advantange the diſtreſſed pilot to know the ſupporting nature of a bladder filled with wind, though otherwiſe but a deſpicable and airy thing, than to know the abſtruſeſt properties of the magnetick needle; ſo, in ſome caſes, the more obvious and ſlight experiments may be much more welcome and ſerviceable to us, than others at other times much more conſiderable. So true is that of the wiſe man, That every thing is beautiful in its ſeaſon.

FOR my part, I am very apt to hope, that natural philoſophy will prove more and more ſerviceable, both to ſingle perſons in their particular occaſions, and to trades themſelves in general; as by other ways, ſo eſpecially by making a further ſearch into, and thereby detecting new qualities, or diſcovering unheeded uſes, of the productions of nature, and of art, that are already known.

I will not here take notice of what may be further hoped for in the detection of medical virtues of things, becauſe I treat of that ſubject in a more proper place: and as for the mechanical uſes (if I may ſo call them) and applications of the works and laws of nature, though he, that gazes upon the ſeemingly great variety of productions to be met with among tradeſmen, and in the ſhops of artificers, may be tempted to think, that art has curiouſly pryed into, and imployed, almoſt all the materials, that nature could afford it; yet he, that ſhall more narrowly and ſeverely conſider them, may eaſily diſcern, that tradeſmen have really dealt with but very few of nature's productions, in compariſon of thoſe they have left unimployed; and, that for the moſt part, they have, in the things they daily converſe with, ſcarce made uſe of any other, than the more obvious qualities of them; beſides ſome few more lurking properties, which either chance, or a lucky ſagacity, rather than inquiſitiveneſs or ſkill, diſcovered to them. And indeed this great variety of productions we have mentioned, proceeds more from a manual dexterity of diverſifying a ſmall number of known things into differing ſhapes, than either from the plenty of natural or artificial productions they work upon, or any diligent or accurate ſearch made into the qualities of thoſe productions. But becauſe, to a conſidering man, it cannot but be obvious enough, that the uſes of the things they deal in, and much more thoſe of other concretes, which they are not engaged to obſerve, have not been hitherto ſufficiently inquired into; I ſhall content my ſelf to add, that if men were but ſenſible enough of their own intereſt, and in order thereunto would keep their eyes heedfully open, partly upon the properties of things, and partly upon the applications, that may be made of thoſe properties, to this, or that uſe in human life, they might not only diſcover new qualities in things, (ſome of which might occaſion new trades,) but make ſuch uſes of them, as the diſcoveries themſelves would never before-hand have ſuſpected or imagined: whereof I may, God permitting, give you elſewhere divers inſtances.

SECTION VII.

AFTER the foregoing general conſiderations (about the uſefulneſs of natural philoſophy to the empire of man over things corporeal,) which I thought fit to take notice of in this firſt eſſay, it remains, *Pyrophilus*, that I alſo add a word or two about thoſe, that are to follow.

AND firſt you muſt not expect, that I ſhould methodically enumerate, and particularly diſcourſe to you of all the grounds and motives I may have of looking for great advantages to accrue to mankind, by men's future progreſſes in the diſcovery of nature. To entertain you with conſiderations, which perchance you would judge but ſpeculative and remote conceipts, would exceed my leiſure, and perhaps be unwelcome to you; and therefore I chooſe to confine myſelf to the inſiſting on thoſe grounds of expectation, which I can render probable by examples and inſtances of what is already actually attained to, or at leaſt very likely (in no long time) to be ſo. And this advertiſement I thought neceſſary to premiſe, partly indeed, that you may not think, that I have overlooked all the particulars pertinent to my ſubject, that I ſhall leave unmentioned, but much more, that you might not ſuſpect, that there are no other inducements to hope much from experimental philoſophy, than thoſe you will find treated of in the following eſſays. And this one thing in particular I dare not forbear to give you notice of, that for the freſhly intimated reaſon, you will there find omitted one of the principal grounds of hoping great matters from improved phyſiology; namely, that by the ſagacity and freedom of the lord *Verulam*, and other lights of this age, conſidering men are pretty well enabled both to make diſcoveries, and diſcern a poſſibility of removing all the impediments, and other cauſes of barreneſs, that have hitherto kept phyſicks from being conſiderably uſeful to mankind; ſuch as many falſe and fruitleſs doctrines of the ſchools; the prejudices, by which men have been hitherto impoſed on about ſubſtantial forms, the unpaſſable bounds of nature, the eſſential difference betwixt natural and artificial things, &c. a too plauſible deſpondency; a want of belief, that phyſicks much concerned their intereſts; want of encouragement; want of natural hiſtory; want of curioſity; want of a method of enquiring; want of a method of experimenting; want of phyſical logick; want of mathematicks and mechanicks; want of aſſociated endeavours; to all which but too many other particulars might be added.

2. You will not think it ſtrange, that in the following tracts much of the uſefulneſs, for which I would recommend phyſicks, ſuppoſes future proficiency in them, if you conſider the nature of my deſign; which is not to make an elogium of natural philoſophy, imperfect as it yet is, but to ſhew, that as it may be, and probably will be, improved, it may afford conſiderable advantages to mankind. And ſince, as I long ago intimated to you, my purpoſe in this book is to invite you, and aſſiſt you to invite other ingenious men, to a farther ſtudy of nature, it is very agreeable to my deſign, to repreſent the greateſt benefits I make it promiſe you, as effects and recompences of your future attainments: and I ſhould allowably enough diſcharge my part in this treatiſe, if I ſhould not do any more (which yet I hope I ſhall do) than give you reaſonable inducements to entertain high expectations of the fruits, that may be gathered from natural philoſophy, if it be induſtriouſly and ſkilfully cultivated: and the very rendring ſuch an expectation probable, I take to be a good ſtep towards the attainment of the things expected; many of which would queſtionleſs be obtained, if men were thoroughly perſwaded, that they are moſt worthy to be endeavoured, and very poſſible to be compaſſed. And therefore I wonder not, that ſo judicious a friend to philoſophy and mankind, as Sir *Francis Bacon*, ſhould in ſeveral places repreſent men's opinions of the impoſſibility of doing great matters of the nature of thoſe things we are ſpeaking of, as one of the chief obſtacles to the advancement of real and uſeful learning: and I the rather inſiſt on the things, that may heighten your expectations, not only becauſe many prudent and learned men, who have been bred in the philoſophy of the ſchools, are apt to judge of all philoſophy by that, which for ſo many ages has been barren, as to uſeful productions, (though fruitful enough in controverſies,) but becauſe I have met with ſome moroſe authors, and others as deſpondent perſons, who, becauſe they have unſucceſsfully attempted to perform things according to the preſcriptions of ſome unfaithful writers of natural philoſophy, fall preſently to believe themſelves, and to perſwade others, that nothing conſiderable is now (at leaſt without almoſt inſuperable difficulties) to be performed by natural philoſophy itſelf, eſpecially, whilſt men amuſe themſelves about ſpeculations and trials, that ſeem not to tend directly to practice; our anceſtors having had the luck to light upon all the profitable inventions, which ſkill in phyſiology is able to ſupply mankind with. But (to take notice firſt of what was laſt ſuggeſted) I make no doubt, but that many experiments, whereby men are not preſently enabled to do what they could not before, may yet be very uſeful to men's intereſt, by diſcovering or illuſtrating the nature or cauſes of things. For though that famous diſtinction, introduced by the lord *Verulam*, whereby experiments are ſorted into luciferous and fructiferous, may be (if rightly underſtood) of commendable uſe; yet it would much miſlead thoſe, that ſhould ſo underſtand it, as if fructiferous experiments did ſo merely advantage our intereſts, as not to promote our knowledge; or, the experiments called luciferous, did ſo barely enrich our underſtandings, as to be no otherways uſeful. For though ſome experiments may be fitly enough called luciferous, and others fructiferous, becauſe the more obvious and immediate effect of the one is to diſcover to us phyſiological truths, and of the

other, to enable us to perform ſomething of uſe to the profeſſor; yet certainly there are few fructiferous experiments, which may not readily become luciferous to the attentive conſiderer of them. For by being able to produce unuſual effects, they either hint to us the cauſes of them, or at leaſt acquaint us with ſome of the properties or qualities of the things concurring to the production of ſuch effects. And on the other ſide thoſe experiments, whoſe more obvious uſe is to detect to us the nature or cauſes of things, may be, though leſs directly, and in ſomewhat a remoter way, exceedingly fructiferous. For ſince, as I have formerly obſerved, man's power over the creatures conſiſts in his knowledge of them; whatever does increaſe his knowledge, does proportionately increaſe his power. And perhaps I ſhould not much hyperbolize, if I ſhould venture to ſay, that there is ſcarce any conſiderable phyſical truth, which is not, as it were, teeming with profitable inventions, and may not by human ſkill and induſtry, be made the fruitful mother of divers things uſeful, either to mankind in general, or at leaſt to the particular diſcoverer and dexterous applier of that truth. To countenance this opinion of mine, I have already given you ſome inſtances, and reſerve more for the laſt eſſays of this treatiſe; eſpecially having obſerved it to have been a fault, which, though prejudicial enough to the intereſt of mankind, is very incident to the more ſober and ſevere ſort of philoſophers, and perhaps more to them, than to otheis, to conclude every thing to be impoſſible, or, at leaſt, unfit to be attempted, that cannot be performed by the already known qualities of things and ways of applying them; without conſidering, that as many ſimples of excellent virtues grow in wilderneſſes, and not by the highway's ſide, ſo divers admirable properties of things may be found, out of the cuſtomary progreſs, or beaten roads (if I may ſo ſpeak) of nature; and that philoſophers are oftentimes deceived, when they think they think they may have made a true and perfect analyſis of the poſſible ways, whereby ſuch and ſuch effects may be produced. For nature by her ſubtlety oftentimes tranſcends and illudes the greateſt ſubtlety of human ratiocinations. And as ſhe may have quite other ways of working, than we are aware of, ſo the knowledge of ſome peculiar and concealed property of a thing may enable them, that are acquainted with it, to perform that with eaſe, which, by the known qualities of things, is either not at all to be performed, or not without great difficulty.

THIS ſeeming paradox you may find in due place confirmed; and in the mean while, to return to thoſe learned men, who having attempted ſome things, and poſſibly performed a few in natural philoſophy, would keep the world from expecting any great matters from it, I ſhall venture to ſay of them, that as the Jewiſh ſpies, though they brought their countrymen out of the land of *Canaan*, ſome few of the goodly fruits of that ſoil, yet bringing them withal a diſcouraging account of the difficulties they were like to meet with in conquering it, did the *Iſraelites* more harm by their deſpondency, than good by their fruits; ſo divers of the authors we are ſpeaking of, though they may have preſented us, with ſome acceptable fruits of their enquiry into experimental learning, yet by bringing up an ill report concerning the ſtudy of it, and thereby deterring irreſolute perſons from addicting themſelves ſeriouſly to it, they have more prejudiced them by their deſpondency, than advantaged them by their experiments. And though I dare not, a chemiſt would not, ſcruple to perſue the ſimile, and tell you, that as only thoſe two of the ſpies, *Caleb* and *Joſhua*, who made no doubt but, that they ſhould conquer the fertile (though never ſo well fortified) land of *Canaan*, did really poſſeſs it, all their diſanimated brethren wandering and dying in the wilderneſs; ſo none but thoſe generous attempters, that dare boldly venture upon the difficulties, that ſurround the knowledge of nature, are like proſperouſly to overcome them, and poſſeſs what they contend for.

Numb. xiii. 14.

Numb. xiv. 28, 29, 30.

BUT I muſt leave this digreſſion to proceed to the laſt advertiſement I am to give you, which is, that I know you may poſſibly expect, that I ſhould ſay ſomething to you diſtinctly of the chief means, by which the naturaliſt may probably advance trades, and aſſiſt man, by the bleſſing of the author of nature, to recover part of his loſt empire over the works of nature. And I confeſs, I have more than once had thoughts of a kind of project (if I may ſo call it) for the advance of experimental philoſophy, conſiſting of ſuch heads as theſe: a proſpect of what probably may be attained to in phyſicks (both as to theory and practice.) A ſummary account of what is attained already. The imperfectneſs of our preſent attainments. What helps men now enjoy. The incompetency of our preſent helps. The hindrances and the cauſes of them. And the means and helps, that may be employed. To which other heads might in caſe of need be added.But notwithſtanding the expectations you may have, that I ſhould handle ſuch ſubjects, and the thoughts I have had about them; I purpoſely waved the treating of them by themſelves in the enſuing eſſays, partly, becauſe theſe unelaborate diſcourſes are not deſigned for a juſt treatiſe on the ſubjects handled in them, containing but ſuch looſe experiments and obſervations, as could without too much impoveriſhing other papers, be put together on this occaſion; and partly, becauſe I have in effect been careful to mention ſeveral of thoſe things, that you might expect to find ſeparately treated of; but knowing, that a far leſs diſcerning eye than your's may eaſily, if there be occaſion, diſtinguiſh them, I thought it more convenient to interweave them with the other parts of the following diſcourſe, ſince every propoſition of a probable way to improve philoſophy is alſo a ground of expecting thoſe advantages, that may be hoped for from philoſophy improved.

OF THE

USEFULNESS

OF

MATHEMATICKS

TO

NATURAL PHILOSOPHY.

OR,

That the Empire of MAN may be promoted by the Naturaliſt's ſkill in MATHEMATICKS, (as well pure, as mixed.)

IF it were not allowable for any but thoſe, that are thoroughly ſkilled in the abſtruſer myſteries of the mathematicks, to diſcourſe of thoſe diſciplines; the title of this eſſay, would, I fear, (*Pyrophilus*) make you think me guilty of preſumption, ſince you may perchance remember, that when you were converſant about thoſe ſtudies, I confeſſed to you, that the great authority of ſome famous modern naturaliſts had, for a while, diverted me from making any great progreſs in thoſe ſciences, by their reſolute denying them to be uſeful to phyſiology. But, as I do not pretend to have taken that pains, which elſe I might have done, to become a ſpeculative geometrician; ſo I conſider, that without underſtanding as much of the abſtruſer part of geometry, as *Archimedes*, or *Apollonius*, one may underſtand enough to be aſſiſted by it in the contemplation of nature; and that one needs not know the profoundeſt myſteries of it, to be able to diſcern its uſefulneſs. And therefore I ſhall venture to propound ſomething to you concerning this laſt named ſubject, eſpecially, ſince otherwiſe you may be influenced, as I once was, by the great authority of thoſe modern philoſophers, who would have the uſe of mathematicks, as diſciplines, that conſider only abſtracted quantity and figure, to be rather hurtful than advantageous to a naturaliſt, the object of whoſe ſtudies ought to be matter. But though theſe endeavour to keep men from thinking the mathematicks to be of any great uſe toward making a man a good naturaliſt, by alledging the extravagant opinions that *Kepler* himſelf, who was mathematician to three emperors, and ſome other modern aſtronomers, have broached or maintained concerning matters phyſiological; yet I confeſs, that after I began, by reflecting upon divers of my experiments, eſpecially mechanical, to diſcern how uſeful mathematicks may be made to phyſicks; I have often wiſhed, that I had imployed about the ſpeculative part of geometry, and the cultivating of the ſpecious Algebra I had been taught very young, a good part of that time and induſtry, that I ſpent about ſurveying and fortification, (of which I remember I once wrote an entire treatiſe) and other practick parts of mathematicks. And indeed, I think, that a competent knowledge in mathematicks (for a profound one is not always neceſſary) may be ſo ſerviceable to thoſe, that would become philoſophers, that I ſhall not ſcruple to mention it, as another thing, which may increaſe your expectation from phyſiology, that thoſe, who paſs for naturaliſts, have, for the moſt part, been very little, or not at all, verſed in the mathematicks, if not alſo jealous of them. And I the leſs ſcruple to write to you on this ſubject, becauſe I do not know, that others have prevented me: for though the learned *Clavius*, and ſome other expoſitors of *Euclid*, have ſaid much of the uſefulneſs of geometry to other mathematical diſciplines; and though not a little has been ſaid in the praiſe of mathematicks in general; yet it is left free for me to diſcourſe to you of (what is the ſubject of this eſſay) the utility of mathematicks, in reference to modern phyſicks, and therein not only to the notions of the corpuſcular philoſophy, but even to practical and experimental knowledge.

NOW there are ſeveral ſcores, upon which ſkill in mathematicks may be uſeful to the experimental philoſopher. For there are ſome general advantages, which mathematicks may bring to the minds of men, to whatever ſtudy they apply themſelves, and conſequently to the ſtudents of natural philoſophy; namely, that theſe diſciplines are wont to make men accurate, and very attentive to the employment they are about, keeping their thoughts from wandering, and inuring them to patience of going through with tedious and intricate demonſtrations; beſides, that they much improve reaſon, by accuſtoming the mind to deduce ſucceſſive conſequences, and judge of them without eaſily acquieſcing in any thing but demonſtration.

AND indeed the operations of ſymbolical arithmetick (or the modern Algebra) ſeem to me to afford men one of the cleareſt exerciſes of reaſon, that I ever yet met with, nothing being there to be performed without ſtrict and watchful ratiocination, and the whole method and progreſs of that appearing at once upon the paper, when the operation is finiſhed, and affording the analyſt a laſting, and, as it were, viſible ratiocination.

BUT, *Pyrophilus*, I may not inſiſt on theſe, or the like general uſes of pure mathematicks, ſince there are divers others, which more immediately reſpect natural philoſophy.

AND to ſhew this the better, give me leave to premiſe to the following particulars, a couple of obſervations.

THE firſt is, that the phænomena, which the mathematician concurs to exhibit, do really belong to the cognizance of the naturaliſt. For when matter comes once to be endowed with qualities, the conſideration how it came by them, is a queſtion rather about the agent or efficient, than the nature of the body it ſelf. So the image or picture, that a man ſees of his face in a looking-glaſs, though that be an artificial body, falls as well under the ſpeculation of the naturaliſt, as when the like picture is preſented him by calm and clear water. And the rain-bows, that are often artificially made in grottos, by diſperſing the water of fountains into drops and ſhowers, have a juſt title to his contemplation, as well as the rain-bow that is formed in the clouds. And the echoes, that are admired in ſome of thoſe grottos, purpoſely and artificially contrived to afford rare ones, do as well belong to his cognizance, as thoſe that nature makes in ruder dens, and other cavities of hills and mountains. And indeed moſt of thoſe phænomena require (for the main) the ſame ſolutions, whether the ſkill of man do or do not intervene to exhibit them.

THE ſecond conſideration, which I am often obliged to repeat, is this; that ſince man's power over the creatures depends chiefly upon his knowledge of them, whatever ſerves to increaſe conſiderably his knowledge, is likely, either directly, or in its conſequences, to add to his power: which two advertiſements being thus given you, *Pyrophilus*, I now advance to the particulars, whoſe mention they made me ſuſpend.

1. AND firſt, theſe diſciplines teach men the nature and properties of figures, both upon ſurfaces and ſolids, and the relations (for they can ſcarce be properly called proportions) betwixt the ſurface and ſolidity of the ſame body. It is true, that matter, or body, is the ſubject of the naturaliſt's ſpeculations; but if it be alſo true, that moſt, if not all the operations of the parcels of that matter (that is, of natural bodies) one upon another, depend upon thoſe modifications, which their local motion receives from their magnitude and their figure, as the chief mechanical affections of the parts of matter; it can ſcarce be denied, that the knowledge of what figures are, for inſtance, more or leſs capacious, and advantaged or diſadvantaged, for motion or for reſt, or for penetrating or reſiſting penetration, or for the being faſtened to another, &c. muſt be of conſiderable uſe in explicating many of the phænomena of nature; and it is ſufficiently known, how much of the doctrine of figures may be learned from geometricians, who treating expreſly and copiouſly of triangles, circles, ſurfaces elliptical, parabolical, hyperbolical, and other plain figures; as alſo of ſpheres, cones, cylinders, and eſpecially priſms, pyramids, cubes, and regular bodies, intimate alſo the methods of judging of the figures of other bodies, that are either compoſed of them, or may, by reaſon of ſome analogy, be referred to them.

THERE are divers properties, as well of planes and ſolid figures, and their habitudes to each other, as of ſuch lines as are deſcribed by motions, or wherein motions may be made; the knowledge whereof may be of good uſe not only to the ſpeculative naturaliſt, but the practical.

TO know the proportion, that *Archimedes* has demonſtrated to be between a ſphere and a cylinder, and either of thoſe to a cone ſo and ſo qualified; or to know, that a triangular pyramid is the third part of a priſm, having the ſame baſe and height; and in a word, to know the proportions between geometrical bodies, may ſometimes be of good uſe, in caſes, where we can procure the one, and not the other, or at leaſt not ſo well as the other. Of this an inſtance is given us by the ingenious *Marinus Ghetaldus*, (as I find him cited by a late mathematician) who tells us, that *Ghetaldus* finding it very difficult to procure an exact metalline ſphere, wherewith to examine the proportion, in point of weight, between heavy bodies of the ſame bulk, found, that yet he could get a cylinder of tin to be turned true; and having therewith made his experiments or obſervations, it was eaſy for him, knowing out of his *Archimedes*, that the proportion of a cylinder, whoſe baſis is equal to one of the great circles of a ſphere, and whoſe height is equal to the diameter of that ſphere, is to that ſphere *in ratione ſeſquialterâ*, as they ſpeak, i. e. has the ſame proportion, that three has to two; it, was, I ſay, eaſy for him, who had often had occaſion to weigh his cylinder exactly, by ſubtracting a third part of the whole weight, to find in the remainder the deſired weight of a ſphere of tin, whoſe diameter was equal to that of the baſis, or to the height of the cylinder [*]: which weight of a ſphere of a known diameter being once obtained, he deduced from them the weights of the other ſpheres he had occaſion to imploy, about the conſtruction of thoſe tables, which have been much made uſe of by divers ſucceeding mathematicians. And what applications I have made of the ſame Archimedean theorem, I may elſewhere inform you.

It being alſo taken for granted by divers modern geometricians and engineers, that the excellent *Galilæo*, and his not degenerate diſciple *Torricellius*, had demonſtrated the line, which a heavy body, projected, and even the bullet, ſhot out of a cannon, deſcribes, to be parabolical; it may be of moment in the practice of gunnery, and in reference to divers experiments to be made with other projected bodies, to be well verſed in the nature of the parabola and parabolical lines, which are alſo thought to be capable of doing wonders in burning-glaſſes, in caſe theſe metalline ſpecula can be brought to a parabolical figure; one of whoſe remarkable properties is, that all the beams, that, being parallel to the axis, fall upon the internal ſuperficies, are reflected to one point or focus; where conſequently, if the burning-glaſs be any thing large, the heat muſt be very intenſe, eſpecially in compariſon of a ſpherical burning-glaſs of the ſame bigneſs.

And as for delightful and recreative experiments, you will eaſily allow me, that there are abundance of catoptrical ones of that ſort, which depend upon the figure of ſpherical, cylindrical, and other ſorts of reflecting glaſſes.

2. I might here tell you, *Pyrophilus*, that pure mathematicks themſelves, ſetting aſide the aſſiſtance they are wont to give to mixed mathematicks, may be of uſe to human life, and to the experimental naturaliſt; of which I ſhall give you, as a ſpecimen, this notable example.

The properties of arithmetical and geometrical progreſſions in numbers ſeem to have very little to do with the practice of weighing out things in ſhops and warehouſes. And yet by the knowledge of the double progreſſion, beginning from an unite, (as arithmeticians call that, wherein the conſequent is ſtill double to the antecedent) as 1, 2, 4, 8. a great deal of cumber, and ſometimes of charge, may be ſaved. For with three weights you may weigh all the pounds, that are from one to ſeven incluſively; with four weights, all thoſe that exceed not fifteen pound; upon which obſervation is grounded the diviſion of ſome boxes or ſets of weights, uſed by our goldſmiths. And if you would, as is very uſual, put weights (when there is occaſion) in both ſcales, to help the thing to be weighed to bring the ballance to an æquilibrium, than the tripple progreſſion (i. e. where the numbers increaſe in a triple proportion, as 1. 3. 9.) has a much more notable property for our purpoſe; by conſidering which, the induſtrious *Stifelius* concluded, that by three weights, you may weigh any number of pounds from one to thirteen incluſively; with four weights, any number of pounds from one to forty incluſively; with five weights, any number of pounds not exceeding ſixſcore and one; and with but ſix weights, any number of pounds from one to three hundred and ſixty four. But the method of ordering ſo few weights to ſerve ſo many purpoſes is beſt found out by ſymbolical arithmetick, or algegra, by which I have taken pleaſure to work ſo fine a problem; which, becauſe it is applicable, not only to pounds, but to the parts of pounds, and thoſe of differing denominations, it may be of ſo great uſe to you, if ever you buſy your ſelf about ſtatical experiments, that I ſhall to the end of this eſſay annex a table, to ſhew, what weights are to be taken in every poſſible caſe, which I found ready calculated to my hand by the ingenious *Franciſcus à Schooten*, profeſſor of mathematicks at *Leyden*.

To the former inſtance, of the uſe that an experimenter may make of pure mathematicks, I might, if it could be ſufficiently delivered in a few words, add the method of computing the combinations, that may be made of any number of things propoſed, which ſome mathematicians call *Regula combinatoria*. For though I remember not to have found this method fully handled in any one author, even among the modern algebricians; yet, as it is delivered by ſome arithmeticians, it is by no means to be deſpiſed, but, as it may be managed by ſymbolical arithmetick, it will, if I miſtake not, want nothing, but the being ſkilfully applied by the naturaliſt, to be on certain occaſions very ſerviceable to him.

3. We may take notice in the next place, that mathematicks may much help the naturaliſt, both to frame hypotheſes, and judge of thoſe, that are propoſed to him, eſpecially ſuch as relate to mathematical ſubjects in conjunction with others.

What wretched theories the ignorance of mathematicks has made naturaliſts, otherwiſe very conſiderable in their way, frame and propoſe, may be evidently ſhewn in the accounts that *Epicurus*, and his paraphraſt *Lucretius*, give of the ſun, and other celeſtial bodies. And indeed what ſatisfactory account can be given of the varying lengths and viciſſitudes of days and nights, and the eclipſes of the ſun and moon, the ſtations and retrogradations obſerved in planets, and other familiar celeſtial phænomena, without ſuppoſing theſe great mundane bodies to have ſuch ſituations in reſpect to one another, and to move in ſuch lines, or at leaſt to be made to appear to move in them by the motion of the earth in ſuch a poſition, and in ſuch lines? Nay, how without the knowledge of the doctrine of the ſphere will the naturaliſt be able to make any ſober and well grounded judgment in that grand and noble problem, which is the true ſyſtem of the world? which is endeavoured to be ſolved after ſuch differing manners by the Ptolomæans and Peripateticks, by the Tychonians and by the Copernicans, both leſs and more modern.

That then the knowledge of celeſtial bodies is not well to be attained, nor conſequently

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[*] Archimed. propoſit. 32. lib. 10. de ſphæra & cylindro.

quently the theories, propoſed of them, to be intelligently judged of, without arithmetick and geometry (thoſe wings, on which the aſtronomer ſoars as high as heaven;) he muſt be very little acquainted with aſtronomy, and particularly with the various, and too often intricate theories of planets, that can doubt. And truly, when I conſider the aſtoniſhing diſtance and immenſity of the celeſtial bodies, and thoſe almoſt numberleſs fixed ſtars (each of them perhaps much vaſter than the whole earth,) which in a clear night I take pleaſure to gaze at through the better ſort of teleſcopes, both in the milky way, and in other parts of the ſky, that ſeem not ſo much as whitiſh to our eyes; I cannot but highly prize a ſcience, that acquaints us, that what we know of ſo much of the univerſe as the globe we inhabit and call the world, is but a point to it, taking up a little more room in it, than a phyſical center in the ſphere.

The uſefulneſs alſo of pure mathematicks to geography is likewiſe evident: and ſure inquiſitive men ought not to deſpiſe this and the former part of learning, without which, as I was lately ſaying, they cannot know ſo much, as whether the earth we live upon, moves or ſtand ſtill.

There are alſo divers phænomena of nature, that are neither aſtronomical, nor geographical, where the uſefulneſs of mathematicks is manifeſt enough. For as to the phænomena of that ſenſe, to which the naturaliſt is moſt beholding, ſight, what a pitiful account is given of them by thoſe Ariſtotelians, phyſicians, and other writers, without excepting many good anatomiſts, that have been ſtrangers to mathematicks, in compariſon of what has been done (not to mention *Euclid*, *Alhazen*, and *Vitelius*) by *Kepler*, *Scheiner*, *Herrigon*, and ſome other modern mathematicians.

And it is evident to thoſe, that are acquainted with dioptricks, that without ſome knowledge, not only of the properties of convex bodies, and of the laws of refraction from and towards the perpendicular, (as the maſters of opticks ſpeak) but alſo of the properties of lines, as circular, parabolical, hyperbolical, &c. and figures, as ellipſes, circles, parabolas, hyperbolas, &c. it is almoſt impoſſible, either well to explicate moſt of the phænomena of that nobleſt of our ſenſes, ſight it ſelf, or to make a well grounded judgment of others explications of them. He, that is altogether a ſtranger to this part of mathematicks, will ſcarce be able to conceive the reaſon of the admirable fabrick of the eye, and how the chriſtalline humour does by its convex figure (like a lenticular glaſs) refract and converge the beams, (or at leaſt the pencils) that proceed from the viſible object, that they may paint the more lively picture of it upon the retina at the bottom of the eye: nor will he underſtand why, by reaſon of the decuſſation of the beams within the eye, this picture muſt be made inverted, though we apprehend the objects themſelves in a right poſture; nor why ſmall objects, placed near the eye, where they are ſeen under a wide angle, appear as big, as very much greater, that are ſeen at a greater diſtance from it. And much leſs will he be able to underſtand the reaſon of thoſe many deluſive apparitions, exhibited by concave, convex, conical, and cylindrical glaſſes, the catoptricks, or doctrine of reflex viſion, belonging yet more to the mathematicks than dioptricks do.

4. And ſince that from the magnitudes of divers bodies, or of ſeveral parts of the ſame body, and ſo likewiſe from their degrees of celerity in their motion, there will ariſe a certain reſpect, which if they be but two, geometricians call a ratio, and if more than two, a proportion, (though theſe terms are oftentimes confounded, and promiſcuouſly employed by authors:) and ſince proportion is ſo frequently to be met with in the works of him, who by an eminent, though apocryphal writer, is truly ſaid to have made all things in number, weight, and meaſure; and ſince the doctrine of proportion, as ſuch, belongs to the mathematician, as the nobleſt part of thoſe ſciences he treats of; I think it may ſafely enough be affirmed, that he, that is not ſo much as indifferently ſkilled in mathematicks, can hardly be more than indifferently ſkilled in the fundamental principles of phyſiology. Nor perhaps would it be raſh to ſay, that the fifth book of *Euclid*'s elements, where the doctrine of proportions is chiefly delivered, may prove more inſtructive to the naturaliſt, than the fifth book of *Ariſtotle*'s phyſicks. And therefore I do not ſo much wonder, that *Plato* ſhould over the gate of his ſchool place an inſcription, (ȣ̓δεὶς ἀγεωμετρητος εἰσίτω) forbidding the entrance to perſons unacquainted with geometry, as unfit to judge of what was there taught.

Nay this, though you may think it ſtrange, is very true, that there are ſome conſiderable phænomena of nature, which are ſo far from being explicable by their cauſes, that men cannot ſo much as underſtand what is meant by them, without ſome knowledge of the doctrine of proportions. As, for inſtance, when the teacher of opticks tells us, that the increments of light are *in duplicatâ ratione diſtantiarum, ſecundum quas à corporibus recedunt, à quibus primum efficiuntur*; he, that knows nothing of proportions, cannot tell ſo much as what they mean by this theorem, much leſs whether or no it be true. And ſo, when the ſame propoſition is by the diligent *Merſennus*[*] applied alſo to ſounds, a common reader would not at all underſtand him, if he did not add by way of explanation, that if, for inſtance, the noiſe of a piece of ordinance be heard a league off, that noiſe will be four times ſtronger, if it be heard but at the diſtance of half a league. Nor will this example it ſelf give ſuch a reader, as we ſpeak of, a clear underſtanding of the propoſed theorem. But a conſiderabler inſtance in this kind may be afforded us by the noble diſcovery of the moderns, eſpecially *Galilæo*, who obſerve, that when a heavy body deſcends through the air, the ſpaces paſt through, from the beginning to the end of the motion, are among themſelves in a (not double, but) duplicate ratio of the moments or equal diviſions

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[*] Harmonic. lib. I. prop. 12.

of time ſpent in the fall; which requires the knowledge of what a duplicate proportion is, to be well underſtood: but it may in ſome ſort be explained, and ſo noble a phænomenon muſt not be here omitted, by ſaying, that *Galileo* affirms himſelf to have obſerved, that a braſs bullet of 100 pound will, in the ſpace of one minute of an hour, deſcend an hundred Florentine cubits, (which ſome reckon to be 180 feet of ours, and conſequently, ſaith *Merſennus*, four cubits in one ſecond, or ſixtieth part of a minute; and by adding, that the bullet falls in ſuch a ratio, that the acceleration of the motion is made according to the progreſſion of odd numbers, beginning from an unite, or one; ſo that if in the firſt moment of time the weight fall down one fathom, in the ſecond moment it muſt deſcend three fathom; in the third, five fathom; in the fourth, ſeven; in the fifth, nine: in the ſixth, eleven; and ſo onward. Whence *Merſennus* gives this rule, to know how far the weight will deſcend in a determinate time aſſigned; and by knowing how far it has deſcended, to calculate how long it was in falling. * *Regula generalis*, ſays he, *hæc eſt. Si dentur tempora, & quærantur ſpatia, quadrentur tempora, & habebuntur rationes ſpatiorum. Si dentur ſpatia, & quærantur tempora, inveſtigetur latus ſpatiorum, & dabitur ratio temporum.*

Divers other inſtances might be produced, to manifeſt the requiſiteneſs and advantageouſneſs of ſome knowledge in mathematicks to a ſpeculative naturaliſt: but I ſhall content myſelf to name one more, viz. that the grand theorem or rule of the ſtaticks, that in the ballance, or reſembling inſtruments, the proportion betwixt the equivalent weights, and their diſtances from the fulcimentum or prop, is reciprocal, (ſo that it is uſual with butchers, and other tradeſmen, to weigh in the ſtatera, commonly called the ſtiliards, 10 or 20 pound weight, for inſtance, hung near the fulciment, with one pound weight, placed on the other ſide of the beam, at 10 or 20 times diſtance from it,) and many other theorems, that ſerve to explicate the properties of the grand inſtrument of nature, motion, (eſpecially as produced or modified by weight, or equivalent force variouſly adapted, and applied) cannot well be underſtood without an inſight into geometry, and eſpecially the doctrine of proportions; and how much the knowledge of the principles and theorems of the mechanicks may aſſiſt the naturaliſt, both to explicate many of nature's phænomena, and to try experiments, and work great changes on her productions, men will then more readily confeſs, when they ſhall better diſcern how many of her works are but engines, and do operate accordingly.

5. And give me leave, *Pyrophilus*, to add in this place, that the doctrine of proportions, as it is the ſoul of the mathematicks themſelves, ſo it may be of vaſt, though perhaps yet unheeded, uſe in phyſiology too; not only as it helps the naturaliſt (as we have newly ſeen it does) to underſtand divers phænomena of nature, but as it may enable him to perform divers things, which he could not perform without it; of which though I may have occaſion to give you hereafter in other papers ſeveral examples, yet I ſhall now mention two or three for illuſtration ſake.

That the pendulum is the accurateſt inſtrument, that we yet have of meaſuring ſhort ſpaces of time, I preſume you do not doubt; and I need not tell you, that he, who would know what length a pendulum muſt be of, to meaſure by its ſwing ſome determinate ſpace of time, as, for inſtance, a half ſecond, (or half the ſixtieth part of a minute,) muſt find it out by trial and obſervation, if he be not unacquainted with the doctrine of proportions: but in caſe he is verſed in that, as well as in the phænomena of pendulums, he may from the length of one pendulum, that exactly meaſures a known part of time, without making particular trials and obſervations, deduce the length of pendulums that will ſerve to meaſure other diviſions of time. For inſtance, that diligent obſerver *Merſennus* aſſures us, that he found by frequent trials, that a ſlender ſtring with a piſtol or muſket bullet at the end of it, whoſe length comprehending the bullet was three foot and a half, (elſewhere he mentions three foot and a 27th) vibrates ſecond (minutes:) this now being taken for granted, and it being a received theorem concerning pendulums alike in all things but length, that the lengths are in duplicate proportion to the times in which their vibrations are reſpectively performed, or are as the ſquares of the vibrations they perform in the ſame time, and conſequently, the times are in ſubduplicate proportion to the lengths of the pendulums; if a man would, as I was ſaying, have a pendulum that ſhall vibrate half-ſeconds, he muſt not take, as one unacquainted with theſe things would be apt to do, a pendulum of a foot and three quarters, which is one half the length of that which vibrates a whole ſecond, for ſuch a pendulum would prove much too long for his purpoſe, nor need he by multiplied obſervations laboriouſly find out how much it is too long, (which oftentimes for want of a ſtandard he cannot do,) but ſince the proportion between a ſecond and half a ſecond is double, and the proportion betwixt the length of the ſtrings, that are to vibrate theſe two differing ſpaces of time, muſt be duplicate of the proportion of the times themſelves, it follows, that the length of the ſtrings muſt be as four to one, which is the duplicate of the proportion of two to one, and ſo the length of the ſhorter ſtring muſt be but a fourth of that of the longer.

This, if it were needful, might be confirmed by a problem of the learned *Ricciolo*'s, whereof I ſhall here give you an example, becauſe I may hereafter have occaſion to ſhew you the farther uſe of it. Let us then ſuppoſe, to avoid fractions, that a pendulum, that vibrates ſeconds, is three intire foot long, (as indeed ſome modern mathematicians tell us it is, and as it may well be according to the meaſures uſed in ſome places.) If then you multiply 3600, the ſquare of the vibrations, which are 60, that your three foot pendulum makes in a ſecond, by the length of the pendulum, which is 36 inches, and divide the product, viz. 129600, by 9 inches, the fourth part of the length of the former pendulum; and if laſtly, of the quotient (14400) you extract the ſquare root, you ſhall find it to be 120, that gives you the number of vibrations, that will be made in a ſecond by a pendulum of nine inches long, and this root being twenty, which is the double of ſixty, you may ſee, that to make a pendulum, that ſhall vibrate half-ſeconds, it muſt be but one quarter as long as that, which vibrates whole ſeconds. And if I thought you were like to think theſe rules as ſtrange, as a perſon wholly unacquainted with the nature of pendulums, and the doctrine of proportions may do; I would invite you to conſult experience, as I have purpoſely done in differing pendulums, that divide a minute into ſeconds, half ſeconds, and quarter-ſeconds; ſince though your trials ſhould not be very nicely made, they may ſuffice to perſuade you, that the above-mentioned rules are either accurately true, or at leaſt true for the main, and therefore true enough to be very uſeful in many occurrences.

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* Merſen. Harmon. Lib. II. Propoſ. 24. Corollar 1.

To the above-mentioned inſtances afforded by pendulums I ſhall here add but one more, that comprehends many thouſands; for the art of compoſing of that great variety of harmonious tunes, that makes muſick ſo delightful to us, depends upon the doctrine of proportions. And he, that being well ſkilled in that, knows how to apply it to the notes or words propoſed, according to the obſervations, which experience has afforded, of the gratefulneſs of ſuch and ſuch conſonancies, &c. may out of his own head compoſe a ſtrange variety of new and pleaſing tunes, which are ſo many exerciſes, that man makes of the power his ſkill gives him over the bodies, of which his muſical inſtruments conſiſt, and over thoſe which they affect.

6. I know not, *Pyrophilus*, whether I may not reckon amongſt the advantages, that mathematicks may afford the naturaliſt, that they will in many caſes ſuggeſt to him divers new experiments, whereby to vary thoſe, wherein the figures of bodies, the lines of motion, as alſo numbers, proportions, and the like affections, which the mathematician is wont to treat of, may come into conſideration. For it is very likely, that thoſe ſuggeſted experiments, which either would not be thought on, or could not be ſkilfully propoſed, by a perſon not verſed in mathematicks, may, either immediately, or upon the ſcore of the applications, that may be made of them, prove ſerviceable to men: of which I hope in one of the following eſſays, to give you ſome inſtances.

See Eſſay IX.

I care not to mention to you, how great a variety of trials and obſervations, about the beſt way of levelling great guns, and the differing diſtances, to which they will carry at ſuch and ſuch elevations, and the lines deſcribed by the motion of the bullet, and other particulars belonging to the art of gunnery, have been propoſed and tried, upon the hints ſuggeſted by geometry's mathematical diſciples (eſpecially) and others, becauſe many good men wiſh theſe fatal arts had been leſs underſtood. And therefore I ſhall rather put you in mind of the great variety of phænomena, which pure mathematicks have helped men to diſcover and derive from theſe familiar obſervations; that a beam of light, paſſing through differing mediums, is not continued in a ſtreight line, but broken or refracted; and, that in ſuch and ſuch conjunctures of circumſtances, the ſun or moon will ſuffer an eclipſe, that will obſcure ſuch a part of the body, and laſt from ſuch a time to ſuch a time: from which obſervations of eclipſes divers very conſiderable things have been deduced by mathematicians, not only as to aſtronomy, but alſo geography, navigation, and chronology. And he that conſiders, what the doctrine of proportions, and of concords (or, as our muſicians call them, cords,) and diſcords, has contributed to the great number of muſical inſtruments, that have been actually made, and delightfully practiſed, and that it may afford the naturaliſt divers hints applicable to other purpoſes, (which I ſhall hereafter have occaſion to intimate,) he, I ſay, that conſiders theſe things, eſpecially if he be alſo acquainted with ingenious, pleaſant, and ſome of them uſeful, experiments, that have been or may be derived from the obſervations, that when a beam of light falls upon a body, and rebounds from it, the angle of incidence is equal to that of reflection; that if the ſuperficies of the body be curve, the angle is to be eſtimated as if it fell upon a tangent to that ſuperficies; that if the beam penetrate the body, and come to it through a thinner medium, it is refracted towards the perpendicular, if through a thicker medium, from the perpendicular; he, as I was ſaying, that ſhall conſider theſe things, and withal, what a great variety of propoſitions, as well problems as theorems, have been deduced by mathematicians by the help of theſe few obſervations, and of as few propoſitions touching the place of the object ſeen by the help of ſpecular and dioptrical glaſſes, will eaſily grant, what by ſo many inſtances I have been endeavouring to prove.

7. I come now to the conſideration, wherewith I ſhall conclude this eſſay, viz. that divers diſciplines, that are reckoned amongſt the mixed mathematicks, are chiefly practical, and may aſſiſt the naturaliſt in making experiments and obſervations, which he either could not make, or could not make ſo accurately without them: as may appear, partly by the art of dialling, which teaches how to meaſure time, and tends chiefly to practice; partly by the art of perſpective, which is of great uſe to repreſent ſolids and diſtances upon a ſmall and plain ſuperficies, and is very ſerviceable to the limner's art; wherein if ſcholars and travellers were more generally converſant, the hiſtory of nature would be far better adorned with lively repreſentations of plants, animals, meteors, &c. and alſo by ſeveral parts of the art of navigation, and particu-

larly that, which they call hiſtriodromia, or the doctrine of the lines, by which pilots make their ſhips to ſail. Now if in theſe and divers other inſtances, that may be given, it muſt be acknowledged, that mixed mathematicks may be ſerviceable to the naturaliſt, and aſſiſt him to promote the empire of man; it ought not to be denied, that pure mathematicks themſelves, as vulgar arithmetick, geometry, and algebra, may be of uſe to the naturaliſt, ſince it is from thoſe ſpeculative parts of the mathematicks, that not only theſe other more practical diſciplines are derived, but a greater number of thoſe diſciplines, that are called mixed mathematicks, may, according to what I elſewhere obſerve, be hoped for. For as ſounds and pure mathematicks make up muſick, and water with the ſame ſciences make hydroſtaticks; ſo, as I elſewhere note, by a further application of the ſame parts of knowledge to other ſubjects, (and in ſome caſes even to the ſame) thoſe diſciplines, that are called mixed mathematicks, may be advanced probably as to number, as well as certainly as to uſefulneſſes and variety of experiments. Nor is it only in thoſe parts of learning, that I have now particularly named, that uſeful applications may be made of the theorems and problems of pure mathematicks, ſince upon theſe ſublime ſciences do alſo in great part depend thoſe other mathematical diſciplines, which are wont (by a ſynecdoche) to be called mechanical, and which it is now time, that I paſs on to conſider.

OF THE USEFULNESS OF MECHANICAL DISCIPLINES TO NATURAL PHILOSOPHY.

SHEWING,

That the Power of Man may be much promoted by the Naturaliſt's ſkill in MECHANICKS.

TO prevent the danger of ſtumbling (as they ſpeak) at the threſhold, I ſhall begin this diſcourſe with advertiſing you, that I do not here take the term mechanicks in that ſtricter and more proper ſenſe, wherein it is wont to be taken, when it is uſed only to ſignify the doctrine about the moving powers (as the beam, the leaver, the ſcrews, and the wedge,) and of framing engines to multiply force: but I here underſtand the word mechanicks in a larger ſenſe, for thoſe diſciplines, that conſiſt of the applications of pure mathematicks to produce or modify motion in inferior bodies: ſo that in this ſenſe they compriſe not only the vulgar ſtaticks, but divers other diſciplines, ſuch as the centrobaricks, hydraulicks, pneumaticks, hydroſtaticks, baliſticks, &c. the etymology of whoſe names may inform you about what ſubjects they are converſant.

I. Now that theſe arts (if you will allow them that name) may be of great uſe to the experimental philoſopher, and aſſiſt him to enlarge the empire of man, may be made probable by this general conſideration, that divers of thoſe things, which in the former eſſay have been evinced to make the mathematicks uſeful to the naturaliſt, may be applied *mutatis mutandis* to the mechanicks alſo. Beſides, that theſe diſciplines have ſome advantages peculiar to themſelves. But the truth of what is thus repreſented in general terms will poſſibly be better diſcern'd, and more perſuaſive, if we deſcend to ſome particulars.

I. First then, the phænomena afforded us by theſe arts ought to be looked upon as really belonging to the hiſtory of nature in its full and due extent. And therefore as they fall under the cognizance of the naturaliſt, and challenge his ſpeculation; ſo it may well be ſuppoſed, that being throughly underſtood, they cannot but much contribute to the advancement of his knowledge, and conſequently of his power, which we have often obſerved to be grounded upon his knowledge, and proportionate to it. When, for inſtance, we ſee a piece of wood, ducked under water, emerge again and float, even vulgar naturaliſts think, that it belongs to them to conſider the reaſon of this emerſion and floating, which they endeavour to render from the poſitive levity, which they fancy to be (upon the account of the air and fire) inherent in the wood, though ſome woods, that will ſwim in water, being put into oil, or high rectified ſpirit of wine, may ſink.

But I ſee not, why it ſhould not belong to philoſophers to conſider and inveſtigate the reaſon, why one part of floating wood appears above the water, whilſt the other keeps beneath it; and why the extent part is equal to the immerſed, or either greater or leſſer than it, in ſuch a determinate proportion; and why the ſame wood will ſink deeper in ſome waters than in others, (as in a river than in the ſea) as on the other ſide ſome woods will ſink lower than others in the ſame water. For if theſe things be duly examined, as they may by the help of hydroſtaticks, not only the cauſe of theſe and the like phænomena will be diſcovered; but by the applications of that diſcovery an eaſy way may be deviſed to meaſure and eſtime at the differing ſtrength of ſeveral ſalt ſprings, and alſo of divers kinds of lixiviums, and brines; to which may be added divers other practical corollaries from the ſame diſcoveries, which I ſhall hereafter have occaſion to particularize.

II. The mechanical diſciplines help me to deviſe and judge of ſuch hypotheſes, as relate to thoſe ſubjects, wherein the notions and theorems of mechanicks either ought neceſſarily to be conſidered, or may uſefully be ſo.

Of this we have inſtances, not only in thoſe engines, that are artificial, and are looked upon as purely mechanical, as the ſcrew, the crane, the ballance, &c. but in many familiar phænomena, in which the theorems of mechanicks are not wont to be taken notice of to have an intereſt. As in the carrying a pike or muſket on one's ſhoulder, in the force of ſtrokes with a longer or ſhorter ſword or other inſtrument, the taking up and the holding a pike or ſword at arms-length, and the power, that a rudder has to ſteer a ſhip; in rowing with boats, in breaking of ſticks againſt one's knee, and in a multitude of other familiar inſtances, of which the naturaliſt's ſkill in mechanicks will enable him to give a far more clear and ſolid account, than the ancient ſchoolmen, or the learnedeſt phyſicians, that are unacquainted with the nature and properties of the centre of gravity, and the ſeveral kinds of levers, the wedge, &c.

III. Nay, there are ſeveral doctrines about phyſical things, that cannot be well explicated, and ſome of them not perhaps ſo much as underſtood, without mechanicks.

That, which emboldens me to propoſe a thing, that ſeems ſo paradoxical, is, that there are many phænomena of nature, whereof though the phyſical cauſes belong to the conſideration of the naturaliſt, and may be rendered by him; yet he cannot rightly and ſkilfully give them without taking in the cauſes ſtatical, hydroſtatical, &c. (if I may ſo name them) of thoſe phænomena, i. e. ſuch inſtances as depend upon the knowledge of mechanical principles and diſciplines.

Of this we have an obvious example in that familiar obſervation, that we partly touched upon juſt now about the ſwimming and ſinking of wood in water. For if it be demanded, why wood does rather ſwim upon water than ſink to the bottom of it, a ſchool-philoſopher would anſwer, that wood abounds with air, which being an element very much lighter than water, keeps it aloft upon the ſurface of that liquor. But this anſwer will ſcarce ſatisfy a naturaliſt verſed in hydroſtaticks. For not now to queſtion what is taken for granted, that there is a poſitive levity, and that the air is endowed with that quality, experience ſhews us, that though when wood is not heavier than ſo much water, as is equal to it in bulk, it will ſwim; yet in caſe it be heavier than ſo much water, it will ſink. As we ſee in divers woods, and particularly in guaicaum, which I therefore the rather name, becauſe chemiſts obſerve, that if it be burnt, it leaves far leſs aſhes (and ſuch are ſuppoſed to contain the terreſtrial and heavy parts) behind it, than many woods, that we know will float in water. And though ſtones and iron be, upon the ſcore of their weight, believed to be bodies, that have little air in them, yet if the liquor, into which they are put, be heavier, bulk for bulk, than they, they will not ſink but float, and if forcibly depreſſed, they will emerge; as you may try, when you pleaſe, by putting ſtones or iron, or the like ponderous body upon quick-ſilver, or melted lead; ſo that we need not here conſider, whether air be, or be not predominant in a propoſed body, when we would know, whether it will, or will not ſink in an aſſigned liquor.

And though we ſhould admit the air, whether included in the pores, or looked upon as an elementary principle, to be the cauſe of its being lighter than an equal bulk of liquor, yet the air would be but the remote cauſe of its ſwimming, its immediate cauſe being, that the floating body is lighter than an equal bulk of the liquor, and therefore the ſame body, without acquiring or looſing air, may ſwim in one kind of water, and ſink in another. As in the caſe of heavy bodies, as loaden ſhips, that having proſperouſly ſailed over the ſea, are recorded to have ſunk as ſoon as they come into harbour, i. e. into a more freſh water; and an egg, that will ſink in common water, will ſwim in a ſtrong brine. Nay a body may (as I, and others have tried) be ſo poiſed in water, that if the liquor be a little warmer, than when the body was poiſed in it, the body will ſink; as it will emerge again upon the refrigeration of it.

And if this general anſwer of the lightneſs of the air will not give ſo good an account as hydroſtatical principles, why a piece of wood will float or ſink, it will much leſs give ſo ſatisfactory an account, why differing woods in the ſame water, or the ſame piece of wood in differing waters, will ſink juſt ſo far, and no farther; whereas, by hydroſtatical principles, the phænomenon is eaſy to be accounted for, according to that theorem of *Archimedes*[*], περὶ τῶν ὀχουμένων, that ſolids lighter than the liquor they are put into, will ſink in it ſo far, as that as much of the liquor as is equal in bulk to the demerſed part, be equal in weight to the whole floating body: whence theſe corollaries are derived, that a floating body has the ſame proportion in weight to as much liquor as is equal to it in bulk, as the immerſed part of the body has to the whole body. And likewiſe, that as much liquor, as is equal in bulk to the whole body, has the ſame proportion in weight to the ſaid body, as the whole body has to that part of itſelf, which is beneath the ſurface of the liquor. And as theſe corollaries determine the proportion between the immerſed and extant part of the floating body; ſo (to ſhew you, that theſe theories lead to practice) they ſuggeſt the way of making a ſmall and light inſtrument, elſewhere deſcribed, to meaſure by a floating body the differing gravities of ſeveral liquors in reference to one another, as well as to the body itſelf. And upon the ſame grounds, the learned *Stevinus* ſhews, that if you know what part of a floating body is immerſed in a liquor, whoſe ſpecifick gravity is alſo known, as it eaſily may be, you may preſently find the weight of the whole ſolid body, let it be never ſo much too great to be weighed in ballances or ſtatera's, yea, though it were a vaſt ſhip itſelf; as ſuppoſing, that that part of ſuch a veſſel, that lies under water, ſhould be 100,000 cubick foot, and that a cubick foot of water weighs 70 lb. (which though it be not the weight we have obſerved a foot of water Engliſh meaſure to amount to, yet that alters not the general rule,) by multiplying 100,000 by 70, the product will be 7,000,000 lb. for the weight of the whole ſhip, with all that is contained in it, as ballaſt, ordnance, &c. or reſts or leans upon it. If I ſhould aſk a meer ſchool-philoſopher, why ſucking-pumps will not raiſe water higher than 40 foot, (though it be commonly preſumed they will raiſe it to any height,) or why in an inverted ſiphon of glaſs, if you pour water and quickſilver in a ſufficient quantity, the ſurface of the water in one leg of the ſiphon, will not be in a level with the ſurface of the quickſilver in the other, but 13 or 14 times as high above the bottom of the ſiphon: or why, if a piece of iron, and a piece of marble or a flint, &c. be equiponderant in the air, if the ſcales be let down into the water, the metal will appear far heavier than the ſtone: if, I ſay, I ſhould aſk a meer naturaliſt both theſe or the like queſtions, I doubt I ſhould much more perplex him, than he would ſatisfy me. And it were eaſy to add a multitude of examples, whereof a good account will ſcarce be given by a naturaliſt, that is unacquainted with mechanicks, and may eaſily be aſſigned by one that is ſkilled in them. But referring the ſchoolmen to *Ariſtotle*'s mechanical queſtions, to ſhew them the neceſſity and uſefulneſs of mechanical knowledge, to give the ſolution of ſundry phænomena, that frequently occur, I will only add an example or two to make good the moſt paradoxical part of what I was ſaying; namely, that there are divers phyſico-mechanical phænomena, which are not to be, I ſay not explicated, but ſo much as well underſtood, without the knowledge of mechanical diſciplines.

THERE is a conſiderable theorem in hydroſtaticks, which is thought to have been firſt taken notice of by *Merſennus*, and in a late writer, is thus expreſſed: *Velocitates motus aquæ deſcendentis & effluentis per tubos æqualium foraminum, ſed inæqualium altitudinum, habent ſubduplicatam rationem altitudinum.* Of which the corollary is, that the tubes are in a duplicate ration to that of the velocities of the water, that ſubſides in, and runs out of them; ſo that to make one tube at a circular hole of the ſame diameter run out in the ſame time twice as much water as another, the greater ought to be not only twice, but four times as long as the ſhorter. And of the ſame proportion (my tryals about which I may elſewhere acquaint you with) divers other practical applications may be made, which muſt not be here inſiſted on.

IV. AS I formerly ſaid of the mathematicks, ſo I now ſay of the mechanicks, that they may aſſiſt the naturaliſt to multiply experiments by thoſe enquiries, that they will ſuggeſt, and thoſe inferences and applications, whereto they may lead us.

OF this we have a noble inſtance in the great variety of tryals, which enquiries, verſed in hydroſtaticks, and other mechanical diſciplines, have upon the ſcore of their being ſo qualified, been either prompted, or at leaſt aſſiſted to make, about the famous quickſilver-experiment deviſed by *Torricellius*; about which though ſo much has been done already, yet almoſt every year brings forth new phænomena.

ANOTHER example to our preſent purpoſe we may take from the great number of new propoſitions, that the diligent *Merſennus* has given us in his baliſticks, about the force and effects of bows, and the like ſpringy bodies. But a yet more noble inſtance is given us by the moſt ingenious *Galilæo*, who, as we may learn from the already mentioned French writer, that has given us an account of *Galilæo*'s new thoughts in that language, has publiſhed ſo many propoſitions (of which he ſets down 19 or 20, with the demonſtrations) about the reſiſtance of bodies to be broken, and the weights requiſite to break them, and the lengths, at which they may be broken by their own weight, that he has reduced them into the form, and given them the title of a new art.

TO all which I ſhall need to add no more, than that he, who knows and conſiders, what a variety of uſeful propoſitions have been, or may be mechanically deduced from the obſervation of *Archimedes*, that a ſolid body weighs leſs in water than in the air, by the weight of water equal in bulk to that body, will eaſily diſpenſe with me for not adding any farther inſtances on this occaſion.

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[*] Lib. I. Prop. 5.

AND the mention of this hydroſtatical propoſition of *Archimides* falls in the more properly in this place, becauſe it will warrant me to tell you, that divers mechanical theorems are not only fertile in other theorems, but in uſeful applications too, of which I may hereafter have occaſion to give you ſome examples, by acquainting you with the uſes I have made of the lately mentioned propoſition of *Archimedes*, and ſome corollaries, that partly by others, and partly by us, have been inferred from it.

V. BESIDES the utilities, that may be aſcribed to the mechanicks in common, with the more ſpeculative mathematical diſciplines, they have ſome, as I formerly intimated, that are more peculiarly their own, ſince they may be of great uſe to the naturaliſt in making of ſuch inſtruments and tools, as for many of his obſervations, trials, and other purpoſes, he may either abſolutely need, or advantageouſly imploy.

OF this we have an example in the mariner's compaſs, as it is called; which is ſo neceſſary to thoſe remote navigations, whereto natural philoſophy and mankind owes ſo much. For though *Baptiſta Porta* * does, as well as other authors, aſcribe the invention of the directive faculty of the magnetick needle to one of his country-men (*Amalphi*, in the kingdom of *Naples*,) yet he confeſſes, that for want of the knowledge of making ſuch ſea-compaſſes as we now uſe, this lucky inventor was fain to make uſe of a piece of wood or ſtraw, to keep the needle a float, and then imbue it with a magnetick vertue; which was a ſhift ſubject to great and manifeſt inconveniencies. And indeed, notwithſtanding the knowledge of the verticity of magnetical needles, if by that of the properties of the center of gravity, or ſome practices derived thence, ſome men, verſed in mechanicks, had not deviſed a way ſo to poiſe the needle, that notwithſtanding the rolling and toſſing of the ſhip, it will continue horizontal enough to direct the pilot; what would become of him in thoſe ſtorms, when he has moſt need of a faithful guide?

BY the help of the centrobarical doctrine, mechanicks have been enabled to make thoſe dipping needles, whoſe phænomena are very odd: and though, as far as I have tried, they yet ſeem uncertain enough; yet it may very poſſibly happen, that farther obſervations may reduce them to ſome theory, whence practical inferences may be deduced.

AND you will the more eaſily believe, that the mechanical applications of centrobarical notions may be of immediate uſe, if we conſider, that by virtue of them, divers writers, and others of unſuſpected credit, aſſure us, that they have made a kind of lamp ſo poiſed, that one may roll it up and down like a bowl, without overturning the veſſel that contains the oil, or extinguiſhing the flame.

FROM the knowledge, that compreſſed air has a ſpring, whereby it reſiſts farther compreſſion, and a ſlight contrivance to make uſe of this pneumatical principle, an acquaintance of mine made a ſlight engine, which afterwards I found mentioned in a printed book, by which he was a great gainer, going, when he was well ſatisfied for his pains and hazard, to the bottom of the ſea, and by the help of this engine ſtaying there ſometimes for divers hours, till he had fetched up valuable things out of ſunk ſhips, and tied cables about their guns, that they might afterwards be buoyed up.

BUT there might be given ſo many examples of inſtruments and tools, that are uſeful to the naturaliſt, and for which, yet, he ought to thank the mechanicks, that it were tedious to enumerate them, eſpecially ſince the ſhops of mathematical inſtrument-makers, and other tradeſmen, may ſupply you with enough of them, to verify what this paragraph would perſuade.

VI. I ſhall conclude the conſiderations I deſigned for this eſſay by this, that as the knowledge of the theorems of mechanicks, and the practices, which have been thence derived, may very much aſſiſt the naturaliſt to make good mechanical contrivances, according to the exigences of his ſeveral purpoſes; ſo one good mechanical contrivance may be equivalent to, and may perhaps actually produce many good experiments.

THE former part of this propoſition will not, I think, require much proof. For a man muſt be but a dull naturaliſt, that ſhall know the properties of the center of gravity, of leavers, ballances, ſcrews, wedges, and other inſtruments for increaſing force, and by frequenting the ſhops and work-houſes of mechanitians, ſhall have ſeen variety of engines and inſtruments to compaſs different things, if he do not, from the ſurvey and conſideration of all theſe, grow more able, by compounding, varying, and otherwiſe improving them, to deviſe ſuch means and expedients, as he would not elſe have thought on, to make ſome trials, that he could not make before, and to make others more accurately, or more eaſily, or ſome way or other better.

AND as to the ſecond part of our propoſition, namely, that one good mechanical contrivance may be as conſiderable as many particular experiments, by enabling the naturaliſt to produce either numerous, or noble ones, or both, it may be manifeſted by ſeveral examples.

AND I ſhall begin with ſo familiar a one, as that afforded by valves, or trap-doors. For as ſlight and obvious as the invention of them ſeems, yet not only we owe to them a great variety of pumps and bellows for oeconomical uſes, but they make very conſiderable parts of ſeveral other engines, and may, as ſome trials have informed us, be applied about ſeveral new experiments, eſpecially if they be made of braſs, and yet ſo ſmall, that like ſome of thoſe I have had made by ſkilful workmen, (who, when I firſt directed them, told me, that they could not be made,) they may be uſed, not only in ſmall glaſs-pipes, but in ſyringes themſelves.

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* Mag. Nat. Lib. VII. cap. 7.

By the help of ſmall valves, and the knowledge of the ſpring of compreſſed air, have been made thoſe wind-guns, which may be employed, not only to weigh the air, (whoſe weight we found them to evince, but not determine,) but to kill deer, and other game, without making a great noiſe, that would fright away the reſt.

If I did not, *Pyrophilus*, foreſee, that in the following eſſays of this treatiſe, I ſhall have occaſion to mention ſome other inſtances of the ſervice, that mathematical and mechanical diſciplines may do the naturaliſt, I ſhould here add divers particulars, which I had rather you ſhould, when you meet with them, refer hither; and therefore I ſhall conclude what I intended now to ſay about theſe diſciplines, by two or three ſhort inſtances, that relate to what I have already ſaid concerning them.

The firſt is, that it was not my deſign to treat of the utility of the mathematicks and mechanicks in an abſolute way: for then I muſt have ſaid much to their advantage, which I have omitted, becauſe it would have too much ſwelled theſe eſſays, and not have been pertinent enough to them. And therefore I thought it ſufficient for me to touch upon thoſe things, on whoſe account theſe diſciplines may be made uſeful to the naturaliſt, by aſſiſting him either to frame theories, or to make obſervations and experiments, ſome (at leaſt) of which, directly, or in their applications, either are already, or are like to prove, practical and uſeful. And it ſeems to me very probable, that the notions and practices of theſe diſciplines, that have been too much hitherto reſtrained by meer mathematicians and mechanicians to the ſtars, the earth, the water, and ſome few other conſpicuous parts of nature, may be very well extended, by a philoſopher, to ſundry other productions, as well of nature, as of art. As *Archimedes* deduced hydroſtaticks from the application he made of vulgar ſtaticks to bodies weighed in air and water, or in water only: and the ingenious *Torricellius*, and others, have of late applied the principles of hydroſtaticks to that ponderous body (which the chemiſts reckon among metals) mercury.

My next advertiſement is, that mentioning mechanical inſtances, not ſo much to acquaint you fully with the things themſelves, as to make the mediums to infer what I would prove, I have taken the mechanical propoſitions, that I imployed, as they are delivered by the artiſts themſelves, without warranting, that their proportions will hold true in mathematical ſtrictneſs. For though I have made trials myſelf of ſeveral things of this nature, yet having often obſerved, how difficult it is to find a mathematical preciſeneſs in phyſical and mechanical things, I think it not amiſs to intimate thus much to you, though I may elſewhere have a fitter opportunity to make it out, that ſo great an exactneſs is in many caſes not neceſſary to make the rules, that want it, uſeful in practice.

The concluding intimation I mean to give you, is, that I have not hitherto mentioned a ſervice, that mathematicks and mechanicks may often do the naturaliſt, which is not fit to be ſilently pretermitted; and it is, that by lineal ſchemes, pictures, and inſtruments, they may much aſſiſt the imagination to conceive many things, and thereby the underſtanding to judge of them, and deduce new contrivances from them.

That I do not groundleſly ſay this, you will grant, if you conſider, how difficult (not to ſay impoſſible) it were to go through with a long geometrical demonſtration, without the help of a viſible ſcheme, to aſſiſt both the fancy and the memory; and how difficult it is to give beginners an idea of the grounds of coſmography and geography, without material ſchemes and globes, your own very recent experience, as well as that of others, will, I preſume, inform you. As it alſo may, how uſeful, not to ſay how neceſſary, pictures, and in ſome caſes, models, are wont to be, when engines, houſes, ſhips, and other ſtructures are to be judged of, that they may be approved, or improved: but I ſhall rather take notice, that not only mechanical, mathematical, and anatomical things, need ſchemes and pictures, to repreſent them clearly to our conceptions; but many things, that are looked upon as more purely phyſical, may, in my opinion, be much illuſtrated the ſame way. Of which, if *Des Cartes* has, as ſome ſay, been the intruder, I think he deſerves our thanks for it. For as *Plato* ſaid, God does always geometrize; ſo in many caſes it may be as truly ſaid, that nature does play the mechanician, not only in animals, but in plants and their parts, and divers other bodies; in the explication of which curious, and oftentimes inviſible contrivances of her's, pictures, that repreſent them well to the eye, and, if it were needful, in dimenſions much greater than natural, may very much further the framing of right ideas of them in the mind.

THAT THE
GOODS of MANKIND
May be much increaſed by the
NATURALIST'S INSIGHT
INTO
TRADES.

TO make out what is propoſed in the title of this diſcourſe, I ſhall endeavour to ſhew two things. The one; that an inſight into trades may improve the naturaliſt's knowledge. And the other; that the naturaliſt, as well by the ſkill thus obtained, as by the other parts of his knowledge, may be enabled to improve trades.

SECTION I.

AND firſt, it ſeems to me to be none of the leaſt prejudices, that either the haughtineſs and negligence, which moſt men naturally prone to, or, that wherewith they are have been infected by the ſuperciliouſneſs and lazineſs, too frequent in ſchools, have done to the progreſs of natural philoſophy, and the true intereſt of mankind, that learned and ingenious men have been kept ſuch ſtrangers to the ſhops and practiſes of tradeſmen. For there are divers conſiderations, that perſuade me, that an inſpection into theſe may not a little conduce, both to the increaſe of the naturaliſt's knowledge, and to the melioration of thoſe mechanical arts.

I. AND I conſider, in the firſt place, that the phænomena afforded by trades, are (moſt of them) a part of the hiſtory of nature, and therefore may both challenge the naturaliſt's curioſity, and add to his knowledge. Nor will it ſuffice to juſtify learned men in the neglect and contempt of this part of natural hiſtory, that the men, from whom it muſt be learned, are illiterate mechanicks, and the things, that are exhibited, are works of art, and not of nature. For the firſt part of the apology is indeed childiſh, and too unworthy of a philoſopher, to be worthy of a ſolemn anſwer. And as for the later part, I deſire, that you would conſider, what we elſewhere expreſly diſcourſe againſt the unreaſonable difference, that the generality of learned men have ſeemed to fancy betwixt all natural things and factitious ones. For beſides, that many of thoſe productions, that are called artificial, do differ from thoſe, that are confeſſedly natural, not in eſſence, but in efficients; there are very many things made by tradeſmen, wherein nature appears manifeſtly to do the main parts of the work: as in malting, brewing, baking, making of raiſons, currans, and other dried fruits; as alſo hydromel, vinegar, lime, &c. and the tradeſman does but bring viſible bodies together after a groſs manner, and then leaves them to act one upon another, according to their reſpective natures; as in making of green, or courſe glaſs, the artificer puts together ſand and aſhes, and the colliquation and union is performed by the action of the fire upon each body, and by as natural a way, as the ſame fire, when it reſolves wood into aſhes, and ſmoak unites volatile ſalt, oil, earth and phlegm into ſoot; and ſcarce any man will think, that when a pear is grafted upon a white thorn, the fruit it bears is not a natural one, though it be produced by a coalition of two bodies of diſtant natures, put together by the induſtry of man, and would not have been produced without the manual and artificial operation of the gardener.

II. BUT many of the phænomena of trades are not only parts of the hiſtory of nature, but ſome of them may be reckoned among its more noble and uſeful parts. For they ſhew us nature in motion, and that too, when ſhe is (as it were) put out of her courſe, by the ſtrength or ſkill of man, which I have formerly noted to be the moſt inſtructive condition, wherein we can behold her. And as it is manifeſt, that theſe obſervations tend directly to practice, ſo, if I miſtake not, they may afford a great deal of light to divers theories, eſpecially by affording inſtances, wherein we ſee by what means things may be affected by art, and conſequently by nature, that work mechanically.

III. THE phænomena afforded by trades are therefore the fitter to be tranſlated into the hiſtory of nature by philoſophers, becauſe they, whoſe profeſſion it is to manage thoſe things, being generally but ſhop-keepers, and their ſervants being for the moſt part but apprentices and boys, they neither of them know themſelves how to deſcribe in writing their own practices, and record the accidents they meet with: ſo that either learned men muſt obſerve and regiſter theſe things, or we muſt, to the no ſmall prejudice of philoſophy, ſuffer the hiſtory of nature to want ſo conſiderable an acceſſion, as the ſhops and workhouſes of crafts-men might afford it; which acceſſion would be much the more copious, if the experiment of trades were made by a naturaliſt, who would doubtleſs ſo manage them, as to make them far more inſtructive, and better fitted for the deſign of a natural hiſtory, than the ſame experiment would be, if they were related but by an illiterate tradeſman, though never ſo honeſt.

And, *Pyrophilus*, to invite you, as you deſign a further progreſs in natural philoſophy, to diſdain, as little as I do, to converſe with tradeſmen in their work-houſes and ſhops; give me leave to tell you, that as he deſerves not the knowledge of nature, that ſcorns to converſe even with mean perſons, that have the opportunity to be very converſant with her; ſo oftentimes from thoſe, that have neither fine language nor fine cloaths to amuſe him with, the naturaliſt may obtain informations, that may be very uſeful to his deſign, and that upon ſeveral ſcores.

For firſt, tradeſmen are uſually more diligent about the particular things they handle, than other experimenters are wont to be; becauſe theſe, if they want diligence, loſe nothing, but what that very want of it keeps them from taking notice of, or at moſt, the ſatisfaction of an unneceſſary curioſity; whereas tradeſmen have anotherguiſe concern in the management of what they employ themſelves about, for their livelihood depends upon it. And as, if they be careleſs, others more diligent will get away their cuſtom; ſo, if they do any thing extraordinary well, the chiefeſt, and, for ſome time, the whole benefit will accrue to themſelves, and by improving their profeſſion they better their income.

Secondly, As it is proverbially ſaid, that neceſſity is the mother of inventions, ſo experience daily ſhews, that the want of ſubſiſtence, or of tools and accommodations, makes crafts-men very induſtrious and inventive, and puts them upon employing ſuch things to ſerve their preſent turns, as nothing but neceſſity would have made even a knowing man to have thought on. By which means, they diſcover new uſes and applications of things, and conſequently new attributes of them; which are not wont to be taken notice of by others, and ſome of which, I confeſs, I have not looked upon without wonder.

Thirdly, I have ſeveral times obſerved trades deal with things unknown to claſſical writers, and unuſed, ſave in their ſhops. And theſe are not only factitious, but divers of them natural; as manganeſe (by ſome called magneſca;) and zaſora (if at leaſt it be what many repute it) emery, tripoli, &c. and of both ſorts there are ſome, that are exceeding uſeful; as of thoſe formerly mentioned, the two firſt are to glaſs-men and potters; and the two later to a number of other tradeſmen; and as among artificial concretes, ſoaders are of neceſſary uſe to gold-ſmiths, lock-ſmiths, copper-ſmiths, braſiers, pewterers, tin-men, glaſiers, &c. amels to gold-ſmiths, glaſs-men, &c. lakes of ſeveral ſorts to painters, heralds, &c. and putty to amel founders, potters, ſtone-cutters, gold-ſmiths, glaſs-grinders, and divers other profeſſions. I ſhall add, that even of thoſe natural things, of which ſome mention is made in famous books, one may learn many things in ſhops, not to be met with there, both as to the differing kinds of things, and as to the marks of their goodneſs, and as to other particulars conducive to the knowledge of thoſe ſubjects. And I freely confeſs to you, *Pyrophilus*, that I learned more of the kinds, diſtinctions, properties, and conſequently of the nature of ſtones, by converſing with two or three maſons, and ſtone-cutters, than ever I did from *Pliny* or *Ariſtotle*, and his commentators.

Fourthly, You ſhall often find, that tradeſmen, being unacquainted with books, and with the theories and opinions of the ſchools, examine the goodneſs and other qualities of the things they deal with, by mechanical ways, which their own ſagacity or caſual experiments made them light upon. And though theſe, having little or no affinity with thoſe, that a book-man would have taught them, will appear to him extravagant; yet being ſuch, as, if they really ſerve the craftman's turn, muſt be true and uſeful, their being extravagant will but make them the more new and inſtructive, and conſequently the more fit to be admitted into the hiſtory of nature.

Fifthly, The obſervations, that tradeſmen can ſupply us with, though they are not probably at any one time ſo accurately made by them, as they would be by a learned man; yet that defect is recompenſed by their being more frequently repeated, and more aſſiduouſly made, than moſt of the experiments, wherein men of letters have furniſhed natural hiſtory: ſo that thoſe circumſtances, which are not heeded by the artificer at one time, may obtrude upon his obſervation at another, and, by reiterating the ſame proceſſes ſo often, it can ſcarce be doubted, but that divers phænomena will offer themſelves, even to an unattentive eye, that would not have been all of them taken notice of by a more heedful experimenter, that had performed the operation but once or twice. But this will be further confirmed in the next paragraph.

Sixthly, There are tradeſmen, that do often obſerve in the things, they deal about, divers circumſtances unobſerved by others, both relating to the nature of the things they manage, and to the operations performable upon them.

Of the particulars, wherein the obſervations of tradeſmen (for the utility of many of their practiſes is not queſtioned) may help us to inveſtigate the nature of bodies, I could name more than my preſent haſte allows me to mention; and I ſhall, as a ſpecimen, take a little notice, firſt, of ſome of the remarks they have to diſtinguiſh and eſtimate what they call the goodneſs and badneſs of the things they deal with; and then of ſome few of their obſervations, that depend upon the influence, that time and ſeaſon have on the things they handle, and upon the artificers operations on them.

For, to begin with the firſt, although they commonly mean by ſuch terms (of goodneſs and badneſs) no more, than the fitneſs, or unfitneſs of ſuch things to yeild a good price, and in order thereunto for the purpoſes they are to be employed about in their particular trades; yet this fitneſs or unfitneſs is wont to conſiſt in, or to ſuppoſe, qualities, that may relate to divers other things, and be applied to many other purpoſes. For ſome of the tradeſmen's criteria diſcover to us a variety and a difference of kinds in bodies of the ſame denomination; as from the potters, the tobacco-pipe-makers, and the glaſs-men, we may learn a conſiderable variety of clays; and from ſtone-cutters and maſons no leſs variety of ſtones untaken notice of by claſſick authors. So from carpenters, joiners, and turners we may learn, that ſome woods, as oak, are fit to endure both wet and dry weather; others will endure well within doors, but not expoſed to the weather; others will hold out well above ground, but not under water; and others on the contrary will laſt better under water, than in the air.

AND as the diſtinguiſhing marks we were ſpeaking of may inform us of the differences and kinds of bodies; ſo they may likewiſe on other accounts give us notice of divers of their qualities. Thus we find by the glaſs-men and ſoap-boilers, that ſome aſhes, as thoſe of kaly, bean-ſtalks, &c. do much more abound in ſalt, than other ſome; and yet ſome of thoſe ſorts of aſhes make clearer, or otherwiſe better glaſs, than the reſt do. We may likewiſe learn of the malſters the differing impreſſions, that the barley receives according to the fewel, whether ſtraw, wood, furs, &c. that makes the fire, wherewith it is dried. And I remember, I have known an ingenious malſter much advantaged by a way he had of ſo preparing malt, as if it had not been dried with wood, (uſually the cheapeſt, but not the beſt, fewel for that purpoſe) whereas indeed it was a ſecret conſiſting only in the choice and ſeaſoning of ſuch a kind of wood, that even the ſolid parts of it cleft burnt almoſt like ſtraw with a clear flame, ſo ſtrangely free from ſmoke, that I could not behold it without ſome wonder.

THE other ſort of inſtructive obſervations to be learned of tradeſmen conſiſts of thoſe, that are made about the operation, that continuance of time, or change of ſeaſon and weather, may have upon certain bodies, and ways of handling them. For naturaliſts, uſually contenting themſelves to make their experiments but once or twice, when their leiſure beſt ſerves, or their occaſions moſt require, have not the ſame opportunity to diſcern, what influence the temper, which the air then is put into, either by the ſeaſon, or the weather, or both, may have on the event of the trial; whereas tradeſmen, by long, and ſometimes unwelcome experience, are taught ſuch and ſuch things will be beſt done at ſuch ſeaſons of the year, or in ſuch kind of weather; which if they be not in ſome caſes obſerved, either the thing will not ſucceed, or the tradeſman will be damnified by his trial.

THUS we ſee, that tanners make choice of that part of the ſpring, when the bark abounds with the riſing ſap, to take it off from the trees; becauſe at all ſeaſons it will not be ſo good nor come off ſo eaſily. Thus joyners think not wainſcoat ſufficiently ſeaſoned, till it be ſo many years old. And in ſeveral countries, butchers obſerve, that though a young bullock may be very good meat, if ſpent ſoon after it is killed; yet if powdered, to be long kept, before the beaſt be four or five years old, the ſalt will too much fret it, and make it little worth. And I look upon it as one of the advantages the naturaliſt may derive from tradeſmen's obſervations, that the ſame things being ſucceſſively dealt with by the father and the ſon, the maſter and the apprentice, they ſometimes make far more long winded obſervations, than the philoſopher has opportunity to do. As for inſtance, thoſe, that make mortars of lignum vitæ, and will make them good, will keep it in the houſe twenty years, or perhaps more, to ſeaſon, as they call it, before they will employ it. And experienced maſons tell us, and as far as I have obſerved truly enough, that as there are ſome ſorts of lime and ſtone, that will decay in few years; ſo there are others, that will not attain their full hardneſs in thirty or forty, or a much longer time. Of which I may elſewhere give you ſome inſtances.

To the ſix foregoing particulars one more may be added to the ſame purpoſe with the reſt, and it is; that by frequenting the work-houſes and ſhops of crafts-men, a naturaliſt may often learn other things, beſides the truth and falſity of what they relate, concerning the hiſtory of the arts they make profeſſion of. For though a tradeſman, being for the moſt unlearned, and aiming only at making or performing thoſe particular things, which, when done, are to bring profit, uſually overlook thoſe phænomena, that make not to his purpoſe; yet nature, (who minds as little his deſign, as he does thoſe works of her's, that conduce not to it) is by ſome agents and operations, that he employs to compaſs his ends, engaged to do ſeveral things, that have a connection with thoſe the artificer proſecutes, or elſe do depend upon them: ſo that the naturaliſt may oftentimes obſerve in ſhops divers conſiderable phænomena, that the tradeſman regards not; becauſe they neither further, nor hinder him in his work, and will be looked upon by him as impertinent to the hiſtory of his profeſſion, in caſe he ſhould be put upon delivering it. And yet ſome of theſe occurring phænomena being produced by nature, when ſhe is as it were vexed by art, and roughly handled by ways unuſual, and ſometimes extravagant enough, may diſcover to a heedful and rational man divers luciferous things not to be met with in books, or probably not ſo much as dreamed of by the authors of them. Sundry examples of this I ſhall have occaſion to diſperſe in the following Eſſay, and other tracts, that are deſigned you in this ſecond volume of our preſent treatiſe.

SECTION II.

I WILL now therefore proceed to ſhew, that as the naturaliſt may, as we have ſeen, derive much knowledge from an inſpection into trades; ſo by virtue of the knowledge thus acquired, as well as by that, which he has upon other accounts, he may be as able to contribute to the improvement of trades.

THIS he may do by ſeveral ways, and eſpecially by theſe three. The firſt, by increaſing the number of trades, by the addition of new ones. The ſecond by uniting the obſervations and practices of differing trades into one body of collections. And the third, by ſuggeſting improvements in ſome kind or other of the particular trades.

THE firſt of theſe I ſhall here lightly paſs over, having elſewhere occaſion to diſcourſe of it more fully; only I ſhall here take notice, that, for the experimental philoſopher to increaſe the number of trades now in uſe among us, it will not be abſolutely neceſſary, that he ſhould invent new ones, ſince he may do it by reviving the trades formerly known to the antients, but loſt to us; ſuch as the making incombuſtible cloth of lapis amiantus, the Tyrian purple, the making of Moſaick work, and thoſe many other inventions, which you may find mentioned in *Pancirollus*, and his learned commentator *Salmuth*. Of which it were not amiſs, that a catalogue were made publick; for ſuch things, having been once actually done by men, are not impoſſible to be done again; and therefore I ſee no reaſon to deſpair, that in ſo ingenious an age as this, ſome, if not moſt, of them may be retrieved.

THE ſecond advantage, that trades may derive from an inquiſitive naturaliſt, is; that by this means the ſeveral obſervations and different practices of trades, whoſe managers want the curioſity, the ſkill, or the opportunity, to make a general inſpection into trades, which they would find the more difficult to do, becauſe crafts-men will often be more ſhy of one another, and more backward to diſcloſe the myſteries of their art to one, that may make a gain of it, and thereby leſſen theirs, than to a philoſopher, that inquires to ſatisfy his curioſity, or enable himſelf to be helpful to them. And certainly, if ſo much as the known hints, that may be given by the experiments already diſperſed among men of ſeveral profeſſions, were known to any one man, though otherways but of common abilities; as my own experience has in ſome meaſure informed me; thoſe united beams, which ſcattered are ſcarce conſiderable, would afford him light enough to better moſt of the particular trades, that are retainers to philoſophy. And perhaps, it were not amiſs, if there were ſome knowing and experimental perſons appointed by the publick to take an exact ſurvey of the trades in uſe amongſt us, and inform themſelves particularly of all the ſecrets and practices belonging to them, that thus diſcerning the errors and deficiencies of each, they may rectify the one, and ſupply the other, partly by the hints afforded by the analogous experiments of ſome other trades, and partly by their own notions and trials.

THUS a few of the more ingenious French gardeners have of late uſefully applied to the watering of young and tender plants that way of filtration, which is uſed by apothecaries with moiſtened cotton wicks or rolls, or elſe with liſts of either linen or woollen cloth, ſo ordered, that one end being immerſed in the liquor to be ſtrained, the other may hang over the brim, and out of the veſſel ſomewhat lower than the bottom, or at leaſt the ſurface, of the liquor. For if this lower end of the liſt be placed over the root of any ſeed or tender plant, it will, by conſtantly and leiſurely dropping on it, water it much more temperately and uniformly, than can be done by common watering pots. And even this way of irrigation may by a cheap and eaſy mechanical contrivance be very much improved. There is another practice among ſtone-cutters, that caſt or mold things with plaiſter of paris, to obtain finer powders, than ſearces are wont to give them, by ſtirring the powder well in water, and after it has reſted a little while, pouring off the upper part of the troubled liquor into another clean veſſel; at the bottom of which there will in time ſettle an impalpable powder. I will not here tell you what uſe I make of this in chemiſtry, to obtain much finer powders, than are uſually to be met with of the ſame denomination. And I ſhall but intimate to you, that by letting the firſt water ſtand but ſo much the longer before you pour off the upper part of it, till not only the groſſer and heavier, but the leſs fine particles be ſubſided, you may get a powder, yet much more ſubtle, than thoſe artificers, that imploy the former way, without this circumſtance, are wont to obtain. This, I ſay, it ſhall ſuffice me to have pointed at, becauſe it is more proper to take notice, that the way of obtaining ſubtle powders by the help of water is uſeful, not only to the above-mentioned craftſmen, but likewiſe to glaſs-men, potters, makers of teleſcopes, and microſcopes, thoſe that caſt metals in ſpaud, and other tradeſmen too. Beſides, that I may hereafter have occaſion to tell you, that it is of great uſe in *China* for the makers of porcelain.

BUT it is not only by acquainting artificers of different profeſſions with one another's practices, that the naturaliſt may further trades, but by making materials employed by one ſort of craftſmen ſerviceable to another. That philoſopher, who has ſurveyed a great number of trades, and compared them together, may do this with advantage, you will eaſily grant, when I ſhall have advertiſed you, that without any ſuch aſſiſtance as that of a philoſopher, in whom their diſtinct knowledge may concenter, and who has ſkill to enlarge the applications of them, we may obſerve, that ſometimes tradeſmen themſelves can make uſe of one anothers productions. Of which I ſhall give you a couple of examples, the one furniſhed me by lytharge, the other by aquafortis.

THE former of theſe, which is but lead powdered and almoſt vitrified, by being blown off (or melted into) the refiner's teſt, as it ſerves the chemiſt to take his ſugar of lead (which it has been obſerved to do better, than minium) and other ſaturnine medicines; ſo it ſerves divers comb-makers to die horns (as we have tried by the mixture of lytharge, quick-lime, and ſharp vinegar. It ſerves alſo ſome painters and others to accelerate the preparations of their fat oils, as they call them. And ſome varniſhers to make their varniſhes dry quickly. It likewiſe ſerves ſome artiſts to make counterfeit gems; and we have tried, that by melting it with about a third part of pure white ſand, or calcined cryſtals, and then putting in a ſmall quantity of mineral concretes, according to the colour intended to be introduced, one may make ſapphires, emeralds, &c. coloured like the natural ones; though this way makes theſe productions too ponderous, ſoft, and dim, and is far inferior to another we may elſewhere have occaſion to diſcloſe.

OTHER mechanical uſes of lytharge I omit, to come to the ſecond inſtance I was mentioning, which is taken from aqua fortis. For not only refiners uſe it to part ſilver from gold and copper (whence the French call it *Eau de depart*) but divers makers of curious wooden works uſe it for the diſcolouring and ſtaining of their woods. Dyers make great uſe of it about their colours, and even about ſcarlet itſelf. Other artificers employ it to colour bone or ivory, ſteeped for a convenient ſpace of time therein, having firſt made it of the colour they deſire, by diſſolving in it copper (inſtead of which I have ſometimes uſed verdigreaſe) or other bodies, fit for their preſent turn; and ſome too by diſſolving in it the fourth part of its weight of ſal armoniac, turn it into aqua regia, and in that make a ſolution of gold, wherewith may be ſtained (as we have tried and taught ſome artificers) the ivory hafts of knives, and boxes of the ſame matter, with a fine kind of purple colour, which yet will not ſuddenly diſcloſe it ſelf on them. Some book-binders alſo employ aſperſions of aqua fortis to ſtain the leather, that makes thoſe fine covers of books that, for their reſemblance to ſpeckled marble, are wont to be called marbled. It is alſo employed (as themſelves have acknowledged to me) by ſome of the diamond cutters, to free the duſt of diamonds from metalline powders, as I ſhall hereafter declare. It is likewiſe of great (and as they imagine of neceſſary) uſe to thoſe, that etch plates of copper or braſs. To which may be added, that we have cauſed canes to be ſtained into the likeneſs almoſt of tortois-ſhell by a mixture of aqua fortis, not too well rectified, which is unexpedient in this work, and oil of vitriol laid on at ſeveral times and places, upon canes held over a large chaſing-diſh of coals, that by the heat the ſtaining liquor may be the better ſucked in by the canes, which muſt afterwards have a gloſs given them, by being diligently rubbed with a little ſoft wax and a dry cloth. Nor are theſe all the uſes made of aqua fortis, as you will find hereafter by inſtances, that I reſerve for other places. But I thought fit to mention this liquor in this place, rather than any of thoſe many factitious bodies I might have taken notice of, for theſe two particular reaſons. The one, that the uſes hitherto enumerated of this menſtruum, may ſerve to confirm what I told you in the ſecond eſſay, of the great utility of menſtruums. And the other, that though aqua fortis be a liquor of exceeding common uſe, and wont to be diſtilled by men of ſeveral profeſſions, as chemiſts, refiners, gold-ſmiths, &c. yet they have had hitherto ſo little curioſity to enquire into the nature of it, or vary the ways of making it, that not only the ways, that a ſkilful naturaliſt might direct for improving it, have not been taken notice of, but no ſmall overſights may be obſerved to be generally and daily made about it. And an ingenious gentleman of my acquaintance, by making ſome trials to improve it, has been ſo far ſucceſsful in his attempts, that he makes it by great odds better, than that which the refiners are wont to employ, or as far as my trials have informed me, than any I have uſed; and affords it for not much above half the price, that is commonly given for it. Nor have his experiments this way alone promoted the refiner's trade, but have alſo diſcloſed to him a way of clearly recovering moſt of his aqua fortis, after he has uſed it in the ſeparation of metals, not only in its former ſtrength, but ſomewhat encreaſed in virtue; which you will the more eaſily think poſſible, if I tell you, that aqua fortis may be made and received in other veſſels, than thoſe that are uſual. As alſo, that without dreaming of this chemiſt's way, I have re-obtained that menſtruum exceeding ſtrong, after having employed it upon certain minerals, for from others I know not whether it may be ſo regained. And laſtly, that there are ſome bodies, beſides glaſs and earth, that are not brittle like theſe, and yet ſerve for the ſecond diſtillation of aqua fortis, though made very ſtrong at the firſt.

AND ſince I am mentioning of this liquor, I ſhall intimate (and only intimate here) that, by adding to ſalt-petre, inſtead of the uſual addittament of three times its weight of brick, or clay, or the like, about an eighth or tenth part only of its weight of another ſubſtance, we have, even in ordinary ſand furnaces, obtained, though ſlowly, a nitrous ſpirit, or aqua fortis much ſtronger at the firſt diſtillation, than that which is wont to be ſold by our refiners, for double or rectified aqua fortis.

YOU, *Pyrophilus*, and divers other virtuoſi, have much more opportunity to make an inſpection into particular trades, than my other ſtudies and occaſions will allow me, and yet I have been more than once able to ſuggeſt to eminent artificers ſuch things, concerning their own profeſſion, as they tried and thanked me for. And therefore I have often wiſhed, that ſome ingenious friends to experimental philoſophy would take the pains to enquire into the myſteries, and other practices of trades, and gives us an account, ſome of one trade, and ſome of another, though the more are handled by the ſame perſon it will be, *cæteris paribus*, the better, not only delivering hiſtorically what is practiſed, but alſo adding their own reflections, and any other thing they think fit to propoſe, towards the melioration of the profeſſions they write of.

AND to give you, for a ſpecimen of this (not perhaps the beſt that I could, but) ſuch an one, as will be ſure not to make you deſpair of out-doing it, I will add at the cloſe of this eſſay, what came into my mind, and coſt me about an hour to ſet down about the trade of thoſe that ſell varniſhed wares.

SOME Italian writers (who indeed are to be commended for it) have given us accounts of ſome particular profeſſions, as beſides others, that I have heard of, but could not procure, *Antonio Neri* has written *Dell' Arte Vetraria*, and *Benvenuto Cellini* of ſculpture, and the ſtatuaries art, and of ſome other profeſſions, worthy, with the art of glaſs-making, to be made Engliſh.

AND indeed, I would willingly invite both you and other virtuoſi of our own country, as well as of others, not to diſdain to contribute their obſervations to the hiſtory of trades. And if you pitch upon any, you may command my thoughts of the method, wherein an account of it may be the moſt conveniently given. For I look upon a good hiſtory of trades, as one of the beſt means to give experimental learning both growth and fertility, and like to prove to natural philoſophy what a rich compoſt is to trees, which it mightily helps, both to grow fair and ſtrong, and to bear much fruit.

AND this I was ſo perſuaded of, that I once deſigned, if the publick calamities of my country had not hindered, to bind ſeveral ingenious lads apprentices to ſeveral trades, that I might the better, by their means, both have ſuch obſervations made, as I ſhould direct, and receive the better hiſtorical accounts of their profeſſions, when they ſhould be maſters of them.

III. BUT it is not only by making the practices and productions of ſome trades ſerviceable to others, that the experimental philoſophy may be a benefactor to thoſe profeſſions. For he may do it by the third of the formerly mentioned ways (which in ſome caſes is coincident with the ſecond) namely, firſt by ſurveying the rules and obſervations already received, and the practices already in uſe of each particular trade he would improve, and then by taking notice of two things concerning it, viz. the deficiencies and inconveniencies, that blemiſh it, and the optatives, that may be made about it; that he may alſo in the laſt place propoſe rational (if not certain) methods or expedients to ſupply or remedy the firſt; and either accompliſh the ſecond, or make approximations to it, as far as it is feaſable, or as his ſkill reaches.

BY deficiencies and inconveniencies, I do not here mean thoſe things, which are wanting to the abſolute perfection, which a philoſoper might wiſh to find in the trade he conſiders; (for theſe belong to the optatives) but thoſe, which are wont to be complained of, and not irremediable, or that are wanting to a more eaſily obtainable degree of perfection. I ſhall not pretend to enumerate theſe in particular trades, but only obſerve in general, that the chiefeſt of them ſeem to be ſuch as theſe.

FIRST, that the artificer may be too much confined to certain materials, ſome of which may be ſcarce, or dear, or ill-conditioned, in compariſon of others, that the naturaliſt might propoſe. As I remember, that being in a place, where we could not procure good vitriol to make aqua fortis with, after the manner of our Engliſh refiners, by a ſubſtitution of burnt allom for vitriol, but in a far leſs proportion, we made ſolvents for ſilver, as good as theirs, if not much better.

AND eſpecially in ſuch caſes as theſe it is, that the naturaliſt may be very much aſſiſtant to tradeſmen. For there are many things, which he, who is acquainted with variety of bodies, and the accounts on which they work on one another, will either quickly diſcern to be performable by other materials, than thoſe, that tradeſmen confine themſelves to, or probably gueſſed to be performable by other agents more in the tradeſmen power; and by making trials of his conjectures, it is like he will within a few trials diſcover what he ſeeks. I know an ingenious perſon, that upon the general complaint made by tanners, of the ſcarcity and dearneſs of the bark of oak, found a way to prepare leather without that or any other bark, as well, if not much better, than it is wont to be done the ordinary way, at leaſt, as far as I, and divers other more ſkilful than I, could gueſs by ſome variety of it, which he ſhewed me. And this variety of materials, which may be ſuggeſted by the naturaliſt, is therefore the more conſiderable, becauſe, that though the ſuggeſted materials be dearer, than that in common uſe, yet it may be ſo much better conditioned in other regards, as to be preferable to it. And though diamond duſt be very many times dearer, than the powder of emery, yet I ſometimes cauſe work to be done for me in a ſhop, where, to cut ſome gems, and even loadſtones themſelves, the craftſmen I made uſe of, did by my encouragement, employ the precious powder of diamonds, inſtead of that of emery, becauſe the former makes ſo great a diſpatch, and obliges them ſo much the ſeldomer to change their tools they apply it with, as makes an advantagious amends for the dearneſs. And ſo, though common ſpelter-ſoder be much cheaper, than that which is made with ſilver inſtead of ſpelter, yet in divers caſes, this laſt is preferable, even by artificers themſelves. For trial informs us, that this will run with ſo moderate a heat, as often needs not endanger the melting of thin and delicate pieces of work, that are to be ſodered; and if this ſilver-ſoder be ſo well made, as ſome I can ſhew, you may with it, ſoder even upon ſoder itſelf, made the ordinary way, with braſs and ſpelter, and ſo fill up thoſe little holes or cranies, that may have been left or made in the firſt ſodering, and are not ſafely to be mended, but by a ſoder more eaſily fuſible than the firſt.

SECONDLY, that the tradeſman may be confined to certain ways of working, when perhaps it would be much more advantageous to him, if he had others propoſed him by the experimental philoſopher, who may perhaps diſcern, that what is mechanically done by the artificer, may be better done phyſically, and on the contrary. Whereas goldſmiths, firſt directed probably by ſome chemiſt, by boiling ſilver-ſpurs, hilts, &c. of curious workmanſhip in ſalt, allom, and argol, give it that whiteneſs and clearneſs, which it would ſcarcely be ſecurely brought to by bruſhing, or pumice-ſtone, or putty. And the like clearneſs, experience has informed us, that old ſullied pieces of good gold may be brought to in a trice, by the help of warm aqua fortis. And as there are divers other things (ſome of which you will find mentioned in a following eſſay) that, though wont to be done mechanically, may be done better by phyſical means; ſo of thoſe things, that ought to be done mechanically, many things, that are wont to be done by the labour of the hand, may with far more eaſe and expedition (the quantity conſidered) be performed by engines; by which, if they be ſkilfully deviſed, our obſervations make us bold to think, that many more of thoſe, that are wont to require a laborious or ſkilful application of the hands, may be effected, than either ſhop-men or book-men ſeem to have imagined. For not to mention thoſe ſeveral inſtruments, on which I have *ex tempore* played divers tunes, that I had never learned, when we ſee, that timber is ſawed by wind-mills, and files cut by ſlight inſtruments, and even ſilk ſtockings woven by an engine, beſides divers other artificial inventions left not named, becauſe they cannot intelligibly be ſo in few words, we may be tempted to aſk, what handy work it is, that mechanical contrivances may not enable men to perform by engines.

THIRDLY, there may be deficiences alſo in this, that what the artificer undertakes is either long in doing (as in the ordinary way of tanning, brick-making, ſeaſoning of wood, &c.) or takes up more pains, or requires a greater apparatus of inſtruments, or elſe is ſome other way more chargeable, or troubleſome, or laborious to be effected, than it needs be. And theſe kinds of deficiences may in very many caſes be ſupplied by the experimental philoſopher. As I know an inquiſitive perſon, that has, upon a ſolemn trial, tanned as well as the maſters of the profeſſion, in far leſs time, (and if I much forget not, in leſs by above half) than they; ſo in ſome places they have a quick way of ſeaſoning ſome kinds of wood, for the uſe of ſea-timber, by baking it in ovens, (which way I have alſo known uſed here in *England*, to ſeaſon ſome ſorts of wood for other uſes in a few hours;) ſo, whereas our grinders of dioptrical glaſſes have hitherto believed, that they muſt make uſe of Venice glaſs, which is very dear, and oftentimes very ſcarce to be come by, ſome virtuoſi, conſidering, that the great clearneſs of an object-glaſs is rather an inconvenience, than a very deſirable qualification, have newly taught ſome of the artificers to employ that coarſer and cheaper ſort of glaſs, they call green-glaſs, which is made here in *England*, inſtead of the other, which now begins to be thought by the ſkilful (with whom my obſervations diſagree not) to be inferior to it. And ſeveral dyers employ our woad, which is not far fetched and much cheaper, inſtead of the eaſtern indigo, for dying of ſome, (if not all) ſorts of blues, and thoſe other colours, which that grand tincture prepares the cloth to receive.

FOURTHLY, another ſort of deficiences or inconveniencies may be the want of durableneſs, either as to the very being of the thing produced by the artificer, or as to the beauty or the goodneſs of it.

OF the former ſort may be (not to mention the decay and ſouring of cyder, perry, &c.) the cracking of glaſs of its own accord, and particularly that, which is complained of by divers, who deal in teleſcopes, that the object glaſſes, which are wont to be made, as I was ſaying, of fine Venice glaſs, will ſometimes, eſpecially in water, flaw of themſelves, and ſo grow uſeleſs, to prevent which, ſome, that are very curious, carry them in their pockets.

OF the latter ſort, is the fading of the bowdye of water colours in limning, and the ruſt of ſhining arms, and other poliſhed ſteel. Divers of theſe incoveniences alſo the naturaliſt may obviate or remedy; as ſome of the virtuoſi above-mentioned, by teaching the glaſs-grinders to make the object glaſſes of their teleſcopes of green glaſs, have taught them a way to make them durable in ſpight of the viciſſitudes of weather. And I have had pieces of artificial cryſtal, whereof ſome, though in no long time, cracked in ſo many places, that they changed their tranſparency for whiteneſs; yet another, though much larger, did, as I conjectured it would, hold ſound during ſome winters, nor was ever broken but by accident: and I remember, I told the artificer, in whoſe furnace the cryſtal, that laſted not, had been made, that I took, as I do ſtill, the reaſon of the difference to be, that the durable cryſtal had but a due, and the other an over great proportion of fixed ſalt. The reaſons of which conjecture I ſhall have occaſion to give you in another place.

AND, as to the ſcarlet dye (whereof I lately made mention) that it may be much advanced, as to point of fixedneſs and laſtingneſs, beyond the common bowdye, I was perſuaded by an honeſt merchant of *Amſterdam*, who had got a great eſtate by colouring of cloth, and was particularly curious about the ſcarlet dye. For he preſented me with a piece of ſcarlet (of which he ſaid he could make enough at a reaſonable rate, wherein he almoſt defied me to find either any part undyed, or to ſtain it with vinegar, lixivium, and other liquors, that he named; and indeed by cutting it I found, that though it were a thick piece of cloth, the middle of it was not (as is uſual in ſcarlets) white or pale, but it was dyed quite thorough; and though of ſcarlet I ſhall elſewhere have occaſion to ſpeak farther, yet I the rather mention it in this place; becauſe it affords me a notable inſtance, that trades may be conſiderably improved by thoſe, that do not profeſs them.

For the moſt famous *Cornelius Drebel*, who was the inventor of the true ſcarlet dye, was a mechanician, and a chemiſt, not a dyer; and as an ingenious man, that married his daughter, related to me, was ſo far from having been verſed in that profeſſion, when ſome merchants put him upon the advancement of certain way of dying a fine red, or rather crimſon, that had been a while before caſually lighted on in *Holland*, and proved very gainful to the finders, that he did not know ſo much as the common way of dying the ordinary reds, though the merchants having once taught him, that, by the help of a ſagacious conjecture (to be told you in one of the following eſſays) he ſoon invented the true ſcarlet dye, which has ſince been ſo much eſteemed.

IT now remains, that I mention in a few words the optatives, that may be propoſed by the naturaliſt about the particular trades he would improve. By which name of optatives I mean all thoſe perfections, that being deſirable, are rather very difficult, than abſolutely impoſſible, to be obtained. Of which optatives, there may ſometimes belong ſeveral to one craft or profeſſion.

OF this ſort, in the black-ſmith's profeſſion, may be the making iron to be fuſible, with a gentle heat (as the flame of a candle) and yet hard enough for many ordinary uſes. In the glaſs-men's trade, and the looking-glaſs-makers may be the making of glaſs malleable or flexible. In the clock-maker's trade, the making the newly deviſed pendulum clocks, uſeful in coaches, boats, ſhips, and in other caſes where they are put into irregular motions.

IN the braſier and copper-ſmith's trade, the making of malleable ſoder. In the ſhip-wright's art, the making of boats and other veſſels to go under water. In the diver's profeſſion, ſome ſmall and manageable inſtruments, to procure conſtantly, at the bottom of the ſea, freſh air not only for reſpiration, as long as one pleaſes, but alſo for the burning of lights.

IN the aſſay-maſter's trade, the quick-melting down of ores, and cupelling of them, or at leaſt of metals, in a trice, without bellows or furnace.

IN the carver's and joiner's trade, the way of giving a ſhape to wood in molds, as we do to plaſter of Paris and burnt alabaſter.

I know, *Pyrophilus*, that ſuch optatives may be thought but a civil name for chimerical projects; but I ſhall hereafter more fully declare to you, why I think it not altogether unuſeful, that ſuch optatives ſhould be propoſed, provided, as I hinted above, that they be very difficult, and not impoſſible; that is, that they be ſuch, as are not repugnant to the nature of the things, nor the general principles of reaſon and philoſophy, and ſeem no otherwiſe to be chemically or mechanically impoſſible, than becauſe we want tools, or other inſtruments and ways to perform ſome things neceſſary to the compaſſing of the propoſed end, or to remove ſome difficulties, or remedy ſome inconveniences, that are incident to us in the proſecution of ſuch difficult deſigns.

AND let me here tell you, *Pyrophilus*, that this advantage may be derived from the deviſing of ſuch optatives to bold and ſagacious men; that if they deſpair of attaining to the perfection they are invited to aim at, they may at leaſt endeavour to reach ſome approximation to it. Thus unſuſpected eye-witneſſes have informed us, that in ſome countries, they are wont to ſhoe horſes without the help of a forge bringing their iron to ſuch a temper, that, having a company of ſhoes ready made, they can eaſily hammer them cold, ſo as to fit them to the ſize of any horſe's foot, which the heat of the climate, where this is uſed, makes the greater conveniency. Nor do I much doubt, but, that by various tempers, iron may be made very ſoft and afterward hardened; and the rather becauſe, as I elſewhere tell you, we have, without antimony or ſulphur, melted it in a crucible, ſo as to pour it out like lead, and yet afterwards it grew harder, than it was at firſt. So, that flexible looking-glaſſes may be made with the help of ſelenitis, you will elſewhere be ſhewn; as alſo to foliate with eaſe all kinds of hollow glaſſes, and ſo turn them into ſpecula. That malleable ſoder may be made, though we have not yet performed it, we do not much deſpair, and by good ſilver-ſoder ſome approximation to it has been already made.

SUBMARINE navigation, at leaſt for a ſhort ſpace, has been ſucceſsfully attempted by the excellent *Cornelius Drebel*, as *Merſennus* aſſures us; and as I have been informed, both by *Drebel*'s ſon-in-law, and by other judicious perſons, that have had the account of trials from the very men, that went in the veſſel under water for a good while together; who affirmed, that though there were many in the boat, yet they breathed very freely, and complained not of any inconvenience for want of freſh air. And here alſo give me leave to take notice, that this inventive *Drebel* was no profeſſed ſhip-wright, nor ſo much as bred a ſeaman.

As for the optative propoſed for the divers, I know one of them, who by a ſlight inſtrument, that is all under water, and has not, as others, any chimney open to the air above the ſurface of the water, has been able to ſtay divers hours at the bottom of the ſea, and remove his reſpiratory engine (if I may ſo call it) with him; and *Merſennus* aſſures us, that a much better way, and in my opinion an admirable one, (if the thing be certain) was found out and practiſed in his Country, by one *Barieus*, who was able to ſtay ſix hours under water, by the help of an almoſt incredibly ſcant proportion of air, and even to preſerve, at the bottom of the ſea, the flame of a lamp or candle, in a veſſel not much bigger, than an ordinary lanthorn.

As to the optative propoſed in the aſſay-maſter's trade, I ſhall in the next eſſay teach you a way of cupelling in ſmall quantities, without a furnace, or coals, or ordinary cupel, or other veſſel.

AND I remember, that by way of approximation, I made a certain powder, with which, without a furnace, I have, in a trice, melted lead-oar (which very often holds ſilver) into metal, and perhaps conſumed ſome of the baſer metal too.

And laſtly, as for the making of emboſſed works of wood in molds, I am credibly informed by a learned man, that it was actually performed lately at the *Hague* by the ſecretary of a foreign ambaſſador; but of the way I could not procure the leaſt hint, though ſuppoſing the truth of the relation, I ſuſpect it was done either by ſome menſtruum, that much ſoftened the wood, which may afterwards be eaſily hardened again, by which way tortoiſe-ſhell may be molded; or elſe, by reducing the wood into powder, and afterwards uniting the parts into one body with ſome very binding and thin kind of glue, whoſe ſuperfluous parts may afterwards be preſſed out. And I remember, I began (but was accidentally hindered to proceed) a trial to make an approximation to this, by the help of a rare glue, of which I had the hint, without being much beholding to him for it, from the practice of an ingenious tradeſman, which as I now prepare it, is made by ſoaking the fineſt ichthyo-colla (i. e. izing-glaſs) for twenty-four, or at leaſt for twelve hours, in ſpirit of wine (or even common brandy, for the menſtruum need not be very good, unleſs for ſome particular uſes.) When by this infuſion the liquor has opened and ſoftened the body (which will much ſwell) both the ingredients are very gently to be boiled together (and kept ſtirring, that the ichthyo-colla burn not, till all be reduced to a liquor, ſave perhaps ſome ſtrings, that are not perchance very diſſoluble) when it is boiled enough, a drop, ſuffered to cool, will ſoon turn to a very firm jelly, and whilſt it is hot, it ſhould be ſtrained thorough a piece of clean linnen into a glaſs or other veſſel, that may be kept well ſtopped; a gentle heat ſuffices to melt this glue into a tranſparent liquor with little or no colour, and yet this fine thin glue holds ſo ſtrongly, and binds ſo very faſt, that having ſometimes taken two ordinary ſquare trenchers (for the round ones are wont to be too thick) and laid the one a pretty way over the other, a little of this liquor put between them, and ſuffered to dry of it ſelf, united the trenchers ſo faſt, that when force was employed to break them, it did it elſewhere, not where they were joined together: ſo that it ſeems, the gluten, that faſtened the trenchers together, was ſtronger than that, which joined the parts of the ſame trencher to one another. The other uſes of this jelly (which by reaſon of the ſpirit of wine, will not eaſily corrupt like other jellies) belong not to this place. Only I ſhall add to our preſent purpoſe, that having taken ſome common ſaw-duſt, and after having imbibed it with melted glue, ſtrained out ſlightly what was ſuperfluous, through a piece of linnen, and ſhaped the reſt with my hand into a ball, this negligent trial (which was only made to ſee whether a more accurate might be hopeful) made the ball, after it had been leiſurely dried, ſo hard, that being thrown ſeveral times againſt the floor, it rebounded up without breaking; but as I was ſaying, an accident hindered me from proſecuting the experiment, which therefore I recommend to you.

I will not now ſtay to tell you, *Pyrophilus*, how it may aſſiſt you toward the making ſuch approximations, as we have been ſpeaking of a little above, to take each of the difficulties, you would ſurmount, into the ſeveral parts it may be conceived to conſiſt of, and make an enumeration of the poſſible ways of maſtering each of theſe, according to ſome methods, that might be propoſed; becauſe to diſcourſe of this ſubject would take up too much of the time alloted to the following eſſays, and therefore I ſhall conclude this, by obſerving to you, that as you are, I hope, ſatisfied, that experimental philoſophy may not only it ſelf be advanced by an inſpection into trades, but may advance them too; ſo the happy influence it may have on them is none of the leaſt ways, by which the naturaliſt may make it uſeful to promote the empire of man. For, that the due management of divers trades is manifeſtly of concern to the publick, may appear by thoſe many of our Engliſh ſtatute-laws yet in force, for the regulating of the trades of tanners, brick-burners, and divers other mechanical profeſſions, in which the lawgivers have not ſcorned to deſcend to ſet down very particular rules and inſtructions.

APPENDIX.

I. HAVING in the foregoing eſſay mentioned a way of making ſpherical, and other hollow looking-glaſſes, with an intimation, that it ſhould not be a ſecret to you, I ſhall no longer delay to acquaint you with it, partly, becauſe, though it may ſeem but a curioſity, yet it may not prov euſeleſs to you in making very eaſily, divers catoptrical experiments, that are otherwiſe difficult enough; and partly, becauſe trial hath informed me, that ſome ways preſcribed, of thus foliating glaſſes, were much inferior to what was pretended. And even in a recent and famous writer, I lately found a proceſs of performing this, which when I had read over, I foretold it would not ſucceed, which prediction was ſoon verified by experience; and indeed they, that know the way and difficulty of foliating much more tractable glaſſes, than hollow ones, will ſcarce wonder, that it ſhould not be found a very eaſy matter to foil, eſpecially, without heat, ſpherical, cylindrical, and other concave glaſſes on the inſide, to which the figure of the glaſs prohibits ordinary foils to be faſtened: yet a mixture, that by the ſucceſs appeared to be fit enough for ſuch a purpoſe, I chanced to ſee employed by an illiterate wandering fellow I met with in the country, the conſideration of whoſe practice did, I confeſs, ſuggeſt to me another mixture, that I afterwards ſeveral times tried, and found it to foliate not only ſpherical glaſſes (to which he confined himſelf) but other concave glaſſes, at leaſt as well as his, if not better, which he held for a great ſecret, and which indeed excelled any I have met with in print. To give you then the way I have practiſed my ſelf, take tin and lead, of each one part (by weight) melt them together, and forthwith add of a good tinned-glaſs (or biſmuth) two parts, carefully ſkim off the droſs, and afterwards, taking the crucible off the fire, before the mixture grow cold, put to it ten parts of clean quick-ſilver, and having ſtirred all well together, keep this foliating liquor in a clean new glaſs for uſe. When you would employ it, ſtrain it through a clean linnen cloth, to ſever it from droſs, and then by the hole of the ſpherical or cylindrical glaſs, put in a long and narrow funnel of paper, reaching almoſt to the bottom of the glaſs, that the falling liquor may not ſputter to the ſides. By this funnel you muſt ſoftly pour in ſome ounces of the mixture, and then dexterouſly and leiſurely inclining the glaſs every way, endeavour to make it faſten on all the inward cavity thereof: this being done for the firſt time, and the veſſel being laid aſide for ſome hours, that the foil may the better ſtick to it, it is beſt to take it in hand again, and after the former manner frequently, but ſlowly, paſs the liquor over thoſe parts of the glaſs, which by holding it againſt the light you ſhall diſcern not to have been ſufficiently foliated the firſt time; afterwards the glaſs being again laid aſide for ſome hours more, the former operation is to be reiterated once (or if it be needful twice) more, until you find the glaſs equally and ſufficiently foiled, which when you perceive it is, you may gently pour out the ſuperfluous liquor, to be reſerved for the ſame uſe in other glaſſes. Laſtly, with a cloth well ſprinkled with putty or ſcraped tripoli, or for need powdered chalk, the outſide of the glaſs muſt be carefully rubbed, to take off the foulneſs it may have contracted by being handled, and to make it look clean and poliſhed.

THIS way I have made uſe of in glaſſes of ſeveral ſizes, and figures, and preferred before that, which I remember, I once ſaw tried, and was aſcribed to a learned Italian, one *Caneparius*, as being more eaſy than it, and more ſafe in regard ours need no arſenick. I found it alſo much better than another, which is kept as a ſecret and highly eſteemed, becauſe though the the ingredients, abating the tin, be the ſame in both, yet in the way already delivered, the liquor or amalgam being uſed cold, there is no danger of breaking the glaſs to be poleated, or miſtaking the degree of heat to be given it, to both which inconveniences trial taught me, that the other way is obnoxious.

II. AND on this occaſion it will not be amiſs, to acquaint you, that I made this improvement of our way; that having made the outſide of glaſſes ſo foliated very clean, I have (by laying on very thinly ſuch a kind of varniſh, as that yellow one elſewhere deſcribed, as fit to make gilt-leathern hangings) made them appear richly gilt, and yet ſo bright and poliſhed, that they would, notwithſtanding this gilding, ſerve very well for looking glaſſes.

III. WHAT other improvements I made of this experiment, I muſt not here inſiſt on, eſpecially that I may comply with the haſte, which obliges me to omit, what I had thoughts of annexing here about varniſhes; ſo that though I have made many trials (whereof another time you may command an account) about ſeveral ſorts of them, ſome, that emulate gilding upon metals as well as leather, others, that imitate and diverſify (if not alſo excel) the China varniſh, and others deſigned for differing purpoſes, yet I can at preſent only tell you in general, that they are an uſeful, as well as ornamental, ſort of productions, and capable (if I miſtake not) of much improvement.

OF DOING BY

PHYSICAL KNOWLEDGE,

What is wont to require

MANUAL SKILL:

OR,

That the Knowledge of Peculiar Qualities, or Uſes of Phyſical Things, may enable a Man to perform thoſe Things Phyſically, that ſeem to require Tools and Dexterity of Hand, proper to Artificers.

The particulars to be mentioned in this eighth eſſay might have been ranged partly under the preceding diſcourſe, and partly, under the eleventh eſſay, (which will be the laſt of this treatiſe,) whoſe titles are comprehenſive enough to take in the inſtances, that make up this preſent diſcourſe; which yet I have rather choſe to deliver apart, not only becauſe they ſeem ſomewhat differing from the examples alledged in the two mentioned eſſays, but chiefly becauſe the uſes, that may be made of ſuch inſtances, may make them deſerve a diſtinct and peculiar mention. For it is both a notable argument of the induſtry of mankind, and may prove a great encouragement to it, that the help of philoſophy may ſupply the office of manual dexterity, ſtrength, or art; and a knowing head may do what is thought not performable, but by a ſkilful hand, or an arm aſſiſted by ſome inſtrument or engine. And of theſe inſtances (which may be juſtly looked upon as ſo many trophies of human knowledge, and ſo many incitements to human induſtry) it will be needleſs to make any diviſion; and therefore I ſhall barely ſet them down as they come into my mind, no other order being neceſſary for particulars, that are brought but as proofs, and have not a dependency upon one another.

The aſſertion, that makes the title of this diſcourſe, the king of *Spain* finds true ſo much to his advantage, that, if I miſtake not, it amounted, for a good while, to divers millions yearly. For whereas formerly in the ſilver-mines of *Potoſi* in *Peru*, (accounted the richeſt in the world) it was wont to be a very tedious laborious, and conſequently chargeable work, to ſever the ſilver particles of the ore from the ignobler parts of it, by many ſlow and coſtly, both manual and metalurgical fuſions, and other ways of ſegregation, much of that labour is now ſaved by *Pero Fernandes de Valeſco*, who, as *Acoſta* informs us, firſt made uſe of at *Potoſi* of the property of quick-ſilver to amalgamate with the nobler metals. For now, by accurately grinding the powdered and ſearſed ore with quick-ſilver (ſtrained through a cloath) and ſalt, and decocting them for five or ſix days, in pots and furnaces fitted for the purpoſe, the greedy mercury licks up the ſilver and gold (which it ſometimes meets with) without meddling with the ignobler parts of the ore; and being enriched with as much of them as it can imbibe, and diligently waſhed from the adhering ſordes, the amalgam is, by diſtillation with a ſtrong fire, freed from the mercury; which coming over revived into the receiver, leaves behind it the fixed metals, viz. gold and ſilver, which may be afterwards (if need be) eaſily reduced into bodies, and parted by the common way. And by a not unlike way ſome of our goldſmiths and refiners are wont (as themſelves inform me) to regain out of the duſt and ſweepings of their ſhops, the filings and other ſmall particles of gold and ſilver, which fall to the ground in their operations, and in proceſs of time may amount to a conſiderable value.

To make an head, exactly repreſenting the ſize, ſhape, and lineaments of the face of any living man, ſeems to require an exquiſite ſkill in the ſtatuary's art; and yet at my deſire, and in my preſence, that was lately performed by a tradeſman, after the following manner. The party, whoſe face was to be caſt off, was laid flat upon his back, having round about the edges of his forehead, his cheeks, and his chin, ſomething placed to hinder the liquid plaiſter from running over on his hair: then into each of his noſtrils was put a hollow piece of ſtiff paper, of about a quarter of a foot long, and of the figure of a ſugar-loaf, and open at both ends, that the affuſion of the plaiſter might not hinder him to take breath. And of theſe pipes, which were carefully oiled over, the acuminated extremes reſted upon his noſtrils, and the other were ſupported by one of the aſſiſtant's hands. Then his face being lightly oiled over, to hinder the plaiſter from ſticking to it, with oil-olive, and his eyes being ſhut, alabaſter newly calcined in a copper-kettle, till it was as white as before, was tempered up with fair water to the conſiſtence of batter, and by ſpoonfuls nimbly put all over his face, till the matter lay every where near an inch thick. Almoſt as ſoon as it was all laid on, it began to grow ſenſibly hot, and in about a quarter of an hour hardened into a kind of lapideous concretion; which being gently and eaſily taken off, ſhewed us, in its concave ſurface, the exact impreſſions made there by the parts of the face, and even by the ſingle hairs of the eye-brows. In this mould they caſt a head of good clay, by working it in, and on that head they open the eyes, which in the prototype and mould were ſhut, and, if need be, heighten the fore-head, and make what other amendments they think fit; and anointing this new face with oil, they after the former manner make a ſecond mould (of two parts, contiguous all along the ridge of the noſe) with calcined alabaſter, and in this ſecond mould (lightly oiled on the inſide) they caſt with the ſame matter the fore-part of an head, more like the original, than ever I ſaw made by the moſt ſkilful ſtatuary, and yet with ſo much eaſe, that the very firſt trial I made my ſelf to caſt a face thus, ſucceeded.

To take the impreſſion of a leaf, or other flattiſh part of a plant, it may ſeem requiſite that a man be a good painter; and yet I found, that the thing may be performed, only by holding a whole leaf (or ſprig of roſemary, &c.) in the ſmoke of a piece of common gum ſandarack, rozin, camphire, or ſome ſuch body, that emits a copious and fuliginous ſteam, (for which purpoſe I have made uſe of a common link, when that was moſt at hand:) for the leaf being well blacked by theſe fumes, and placed betwixt the leaves of a ſheet of white paper, if you carefully preſs the paper upon the leaf with the haft of a knife, or ſome other ſmooth thing, you may thereby print on the paper in a few moments the exact ſize and figure, but not colour, of both ſides, but eſpecially the back-ſide, of the leaf, with the very ramifications of the fibres that are diſſeminated through it. And this may be performed, though not ſo lively, by blacking the plant, whoſe picture is required, with the fumes of a candle or taper, (eſpecially if it be of wax) inſtead of thoſe of the aforementioned reſinous concretes, and afterwards proceeding as in the former experiment: which ſometimes may be of good uſe to you, when you turn botaniſt, and in your travels meet with plants, whoſe pictures you think worth having, but have not time or conveniency to draw them.

ANOTHER inſtance, of the ſame import with the foregoing ones, may be afforded us by the art of etching, whereby copper and ſilver-plates may be enriched with figures, which may ſeem to have been made by the tool of ſome excellent graver; and yet thoſe engravings do not require the preſumed manual ſkill, and are made without ſuch tools, by having a peculiar ſort of varniſh (for on the goodneſs of that, depends much of the ſucceſs of the operation) on the plates, and drawing on it the figures to be engraved. For all thoſe lines, where the plate is freed from the varniſh, by ſkilfully tempered aqua-fortis (from whoſe corroſive violence the remaining varniſh ſecures the reſt of the plate) may be ſo curiouſly wrought on by thoſe few artiſts, that are ſkilful in it, that I have very ſeldom ſeen lovelier cuts made by the help of the beſt tempered and beſt handled gravers, than I have ſeen made on plates etched, ſome by a French, and others by an Engliſh, artificer.

BUT the knowledge of the phyſical properties of things may ſometimes enable a man to perform, not only things to which mechanical tools and manual dexterity ſeem to be neceſſary, but ſome things alſo, whereto even mathematical inſtruments, and ſkill in mathematicks, are thought requiſite; of which I ſhall at preſent propoſe a couple of inſtances.

In the elſewhere mentioned French abridgement of *Galileo*'s Italian book *, I find a paſſage very pertinent to our preſent deſign, which agreeing very well with our obſervation of that kind, we ſhall propoſe it a little more clearly as follows.

SUPPOSE in a high church (the book exemplifies *Noſtredame*) the great candleſtick, that hangs from the top of the church being made to ſwing, a philoſopher, that has obſerved, that the vibrations of a pendulum, though the arches it deſcribes be unequal, are in the ſenſe formerly declared equitemporaneous; and that, when the ſtrings, at which ſuch pendulums hang, are very unequal, their lengths will have the ſame proportion, as is between the ſquares of the numbers of their ſingle vibrations performed in the ſame time: ſuppoſe, I ſay, that ſuch a perſon have a pendulum with him, whoſe ſtring (which may be of any length, ſo it be determinate) is, for example, a yard long, it will not be difficult for him, without any quadrant, or geometrical inſtrument, to find out the length of the ſtring that ſupports the candleſtick, and conſequently the height of the church. For the candleſtick and the ſhort pendulum being made to ſwing, beginning both at the ſame time, let us ſuppoſe, that when the candleſtick has made nine vibrations, the pendulum of a yard long has made 54, the ſquares of theſe two numbers will be 81 and 2916; and becauſe, as we lately ſaid, the length of the pendulums will have the ſame proportion with the ſquares of the number of their vibrations, dividing 2916 by 81, the product will be 36; which ſhews, that the ſtring, at which the lamp hangs, is 36 times as long as that of the ſhorter pendulum, and conſequently a yard, containing three feet, amounts to (36 yards or) 108 feet.

UPON the knowledge of another phyſical property of heavy bodies, I remember I have grounded a way to meaſure vaſt heights and depths without any geometrical inſtruments, and in ſuch caſes, where ſuch an inſtrument cannot be imployed, by the help of a pendulum; which, becauſe in this caſe it muſt be very ſhort, will require an attentive and expert obſerver. For it being known, that a ſtone, or a piece of lead, or the like ſolid weight, falling from a height, does ſo accelerate its deſcent, that the differing ſpaces it has tranſmitted, at any differing times aſſigned, will have betwixt the ſame proportion with the ſquares of the times, wherein the reſpective ſpaces were tranſmitted; if it be once known by diligent obſervations, how far a ſtone, or ſuch a ſolid body, (whoſe greater or leſſer bulk is not here conſiderable) does fall at the end of the firſt ſecond-minute of its motion downwards, it will be eaſy enough for a naturaliſt, verſed in the doctrine of proportions, to collect from the time, that the ſtone imploys in deſcending perpendicularly from the top of a high tower or ſteeple, how high that building is. This way of meaſuring, provided attention and accuracy be not wanting, we found agreeable enough to divers obſervations of our own and our friends; and by this way, one may meaſure the depth of a well (to the ſurface of the water) how deep ſoever, though the bottom, as it is uſual, by reaſon of the darkneſs, cannot be ſeen, which makes the depth unfit to be meaſured by quadrants, and ſuch like geometrical inſtruments. For, if at the ſame time, that you let fall a ſtone, or other weight, you alſo let go a pendulum, that vibrates quarter-ſeconds, that is, makes two excurſions, and as many returns in the ſixtieth part of a minute, and reckon its vibrations, till you hear the noiſe made by the ſtone daſhing againſt the water in the bottom of the well, you may eaſily enough collect the depth. For let it be ſuppoſed, that it be found by experience, that a falling ſtone, or other like weight, do in the firſt ſecond-minute of its deſcent, diſpatch (as the diligent *Merſennus* affirms himſelf to have often found) 12 feet, (which I underſtand of French, not having found it hold in Engliſh meaſure,) and let us alſo ſuppoſe the pendulum to have perfected ſix ſingle vibrations before the daſhing of the ſtone againſt the water was heard; if we proceed according to the rule formerly given, we ſhall find, that if the time, wherein the falling ſtone tranſmitted thoſe ſpaces, that are to direct our calculation, be one and ſix, the ſquare of thoſe two numbers being 1 and 36, the ſtone muſt have fallen at the end of the ſixth ſecond 36 times as far as at the end of the firſt. And ſince by obſervation (about whoſe accurateneſs we need not be ſolicitous here, where we deſign only the giving an explanatory example) a falling ſtone in the firſt ſecond deſcends 12 foot, we need but multiply 36 by 12, to obtain in the product 432, the perpendicular depth of the well to the ſurface of the water. And the ſame number may be collected, and perhaps you will think more eaſy, by ſuppoſing, as *Galileo's* experiments ſeem to prove, that a falling body accelerates its deſcent according to a progreſſion of odd numbers, beginning from an unit; ſo that, if in the firſt ſecond-minute, or any other determinate part of time, it falls one ſpace, whatever that be, in the next ſecond it will fall three ſpaces, and in the third five ſpaces, and ſo onwards: according to which reckoning, if the falling body be ſuppoſed to deſcend 12 foot, during the firſt ſecond, it will deſcend 36 (beſides the former 12 in the next ſecond,) in the third 60, in the fourth 84, in the fifth 108, in the ſixth 132, which ſummed up together, amount to 432. And, by the ſame way, one may meaſure the height of divers precipices how great ſoever, as far as one can reach downward in a perpendicular line. And one may alſo give ſome gueſs at the depth of ſome volcanos, which are not acceſſible to thoſe, that know but the common ways of menſuration, or which have burned the ropes, and even melted down the chains and weights, by which ſome curious perſons have attempted to fathom their depth. It is true, that in mathematical rigour, ſome abatement ought to be made, becauſe the ſtone ſtrikes the ſurface of the water, or the bottom of the precipice, ſome little while before the ſound, produced by that ſtroak, can arrive at our ears. But unleſs the height or depth to be meaſured be very extraordinary, this allowance, for the delay of the noiſe, either may be neglected without much inconvenience, or in probability, will ſcarce exceed a quarter (or at moſt half) of a ſecond; ſince, as has been elſewhere noted, it has been found by obſervation, that a ſound in the air moves above twelve or thirteen hundred foot in one ſecond. And in what I have here delivered concerning the way of meaſuring depths and heights by the falling of a heavy body, I have been much confirmed by an obſervation I chanced to meet with in an outlandiſh book, which I have not now by me to look out the place, where the mathematician, that writes it, who ſeems to have been a diligent obſerver, affirms, that he found a weight let fall from the top of a church, or ſteeple (for I remember not which, nor is it material,) ſo high as to amount to 300 foot, to reach the ground in about five ſeconds; which agrees very well to what we have been delivering. For ſuppoſing the weight to fall 12 foot the firſt ſecond, at the end of the fifth ſecond it muſt have fallen 25 times as far, (1 and 25 being the ſquares of the numbers of the ſeconds of time,) and conſequently 300 foot.

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* Nouvelles penſees de Galileo liv. I.

To ſlit (or divide tranſverſly into flakes or leaves) ſo thin a piece of metal as an old groat, which ſeems not to exceed, if it ſo much as equal, the thickneſs of a leaf of white paper, may be thought, if it be feaſable, to require ſome very ſubtle dividing inſtrument, with an edge finer than that of a razor; and yet the way of performing this by phyſical means is but an almoſt ludicrous experiment, which (if you know it not already) is eaſily thus made. Take three pins, and ſtick them in the form of a triangle, at ſuch a diſtance from each other, that the groat may reſt upon the heads of them: put upon this thin piece of metal almoſt as much flower of brimſtone, or, at leaſt, finely powdered ſulphur, as will conveniently lie on it; then kindling the ſulphur, let it burn out of itſelf; which done, take off the groat, and throwing it hard againſt the floor, the upper part, with the adhering remains of the ſulphur, will be parted from the lower: which (lower) if the coin were not very thin, will retain its former ſhape. I have obſerved in this experiment, a pretty circumſtance or two, the knowledge of which is very apt to be miſemployed, and need not here be mentioned: though I would not ſilently paſs by the experiment itſelf, becauſe as ludicrous as you may think it, it may ſuggeſt uncommon ſpeculation to a conſidering naturaliſt, and alſo intimate a way of preparing ſilver, of which I may elſewhere tell you the practical uſe.

HE that takes notice of ſo pretty a variety of colours and ſhapes, as may be diſcerned on a ſkilfully made ſheet of marble-paper, will be apt to conclude, either, that the differing colours were laid on one by one with a pencil, which would require a great deal of time and pains; or that the ſheet was marbled by being printed off from ſome plate, on which the differing ſhapes were cut or engraven, and the differing colours ſingly placed, which would require yet more labour, and a greater apparatus; whereas the whole ſheet is painted thus variouſly and delightfully at once, and in a trice, by the contact of the ſurface of a veſſel full of water, on which the colours (firſt blended a little, by a quick and eaſy motion of the artiſt's hand) are ſo ordered, as to ſwim without being confounded. This artifice hath, as I am informed, been delivered by the curious *Kircherus*. But if you have a mind to know the particulars of it more fully, you may command me to acquaint you with what I have learned from experience, by which the practice is ſuppoſed to have been of late improved.

IF it were propoſed to free weak ſpirit of wine, or aqua vitæ, from a great part of its phlegm, the generality of diſtillers would think it not to be effected, but by the help of fire and a furnace, an alimbeck, or ſome other diſtillatory veſſels; and yet, without the help of any of all theſe inſtruments, I have ſometimes taken pleaſure to dephlegm brandy, (as they call weak ſpirits of wine of the firſt diſtillation,) only by putting it into ſalt of tartar. For conſidering the faculty this alkalizate body has to attract (as men commonly ſpeak) or imbibe the aqueous particles, that ſwim in the air, and reſolve itſelf, with them into that liquor, that the chemiſts call oil of tartar per deliquium, there ſeemed ſufficient reaſon to expect, that the ſame ſalt being put very dry into phlegmatick ſpirit of wine, would embody with the phlegmatick parts, with which, if it were not overcharged, it would probably keep them ſeparate from the more ſpirituous liquor; ſince ſuch oil of tartar as I have juſt now mentioned, and dephlemged ſpirit of wine, will ſwim upon one another without mixing; and accordingly, I have ſometimes taken pleaſure, by putting a ſufficient proportion of dry ſalt of tartar into brandy, and leaving it there for ſome time (for the experiment will, to be compleated, require ſome while) to make ſome ſeparation of a great part of the phlegm, which by degrees diſſolving the ſalt, will reduce again part of it into a liquor, that will keep its ſurface diſtinct from that of its ſupernatant ſpirit, and if confounded therewith by the ſhaking of the glaſs, would ſpeedily part from it, and regain its own ſtation; and if you would have a ſeparation of the phlegm begin to appear quickly, you may compaſs what you intend, by tying up a convenient quantity of dry ſalt of tartar in a dry rag of linnen cloth, and immerſing it a little while in the brandy, and then lifting it up a little above the liquor; for the phlegmatick parts being copiouſly imbibed in the ſalt, which will be thereby reſolved into a ponderous liquor, will in drops (whoſe deſcent will be diſtinguiſhable enough, if the glaſs be held againſt the light) fall to the bottom of the ſpirit of wine. And leſt you ſhould ſuſpect, that this deſcent comes not from their weight, but from the force they acquire in falling through the air, you may keep the rag immerſed beneath the ſurface of the liquor, and yet may perceive the efflux and ſubſidence of the lixivium we have been ſpeaking of.

THERE are ſome caſes, wherein bodies, that are to be held very ſoftly, are either ſo brittle, that it would be hard to hold them faſt enough without danger of breaking them; or elſe ſo ſmall, and ſo inconveniently ſhaped, that it would be very difficult to procure inſtruments to lay hold on them, and keep them moveleſs in the inſtrument: and in ſeveral ſuch caſes the uſe of tools, to hold faſt ſuch bodies, may be advantageouſly ſupplied by artificial cements. As I remember I have known the glaſs-grinders, inſtead of more mechanical tools, employ pitch, melted and made up with aſhes, very well ſtirred and incorporated with it, into a ſtiff paſte. For this mixture, being by a fit heat brought to a convenient ſoftneſs, the glaſs to be ground or poliſhed is bedded in it, in what poſture, and as far as, the artificer pleaſeth; and by the ſame mixture the glaſs being faſtened, at the end of a ſtick or ſome proper inſtrument of wood, the glaſs, upon the cooling of the cement, remains firmly faſtened, until the artificer have done with it what he deſigned; after which, by ſoftning the cement with heat, he can readily take it off again.

AND even the diamond cutters, who, to grind thoſe ſtones into ſhapes, are wont to employ a very vehement attrition, make uſe, for holding their diamonds, eſpecially when they would poliſh them, of a cement, the like to which I remember I have ſometimes made to other purpoſes: for themſelves have confeſſed to me, that they made theirs chiefly of roſin, melted and brought to a ſtiff paſte, with fine brick-duſt, to which one of the eminenteſt of them for ſkill adds a proportion of ſealing-wax; (I told him I preferred plaiſter of Paris before brick-duſt, and he told me he did the like.)

AND indeed by variety of cements we may be aſſiſted to make divers experiments, that we could not otherwiſe make ſo well, if at all; for which reaſon I have been ſomewhat curious about making a pretty number of ſuch mixtures, whoſe compoſitions you may command of me.

THERE are divers artificers, eſpecially thoſe, that ſlit and poliſh cryſtal, agates, and other hard ſtones, and cut ſeals in gems, who have need of powders of emery, of differing degrees of fineneſs, and ſome of them extremely ſubtle: to obtain theſe, one would think it neceſſary to have variety of ſearces, and ſome of them as fine as it is poſſible. But the ſkilfulleſt artificers judge they can obtain their deſire much better by fair water, than by the beſt ſearces. For having in a mortar beaten the hard body of emery, as long as they think neceſſary, they put the powder into a pail, or other fit veſſel, full of water; and then with a ſtick, or ſome ſuch thing, they ſtir very well all, that is at the bottom, that it may be raiſed and thoroughly mingled with the liquor; then pouring it out into another veſſel, the groſſeſt and the moſt ponderous grains of the diſperſed powder will firſt fall to the bottom, and give a powder leſs groſs, than that, which remained in the firſt veſſel, which may be again beaten ſmall in the mortar. Afterwards they pour the troubled water of the ſecond veſſel into a third, and there ſuffer the duſt to ſubſide, and then decanting the liquor, if this duſt be not yet fine enough, they trouble the water again, and after a little while, pour it off either into one veſſel, or two, or more, ſucceſſively, according to the exigency of their uſes; and then ſuffering the tranſvaſated water to ſettle for ſome hours (more or fewer,) as the diſperſed duſt is more or leſs light, they decant the liquor, or ſuffer it to exhale, and take the remaining powders, of which that, which ſettles ſloweſt, will oftentimes be ſtrangely ſubtle. And by this way, if a man will have patience to pour ſucceſſively the troubled liquor into veſſels enough, and give the diſperſed powder a competent time to let fall the leſs light parts, before the upper part of the water be poured off into the veſſel it is finally to ſettle in; he may obtain ſeveral degrees of powders, leſs and leſs groſs, and ſome ſo fine, as one would admire how it was made ſo. And this (*Pyrophilus*) I the rather mention to you, becauſe it is not only from emery, but from divers other bodies, that one may obtain extremely minute, and (as they ſpeak) impalpable powders, of great uſe in ſome of the moſt curious trades, and perhaps in phyſick too. For I may elſewhere tell you, how I apply this way to magiſteries of cryſtal, and of gems, and even to *Crocus Martis*; the naming of which laſt puts me in mind to add, that a chemiſt, much prized for finer *Crocus Martis*, than others of his profeſſion, and thereby enabled to ſell it at an extraordinary rate, confeſſed to me, that it was to the artifice I had been commending, that the *Crocus* he ſold owed all its advantages.

IT has long been, and ſtill is, in many places, a matter of much trouble and expence, as well of time as money, to cut out of rocks of alabaſter and marble, great pieces, to be afterwards ſquared or cut into other ſhapes; but what by the help of divers tools and inſtruments cannot in ſome quarries be effected, without much time and toil, is in other places eaſily and readily perfomed, by making, with a fit inſtrument, a ſmall perforation into the rock, which may reach a pretty way into the body of it, and have ſuch a thickneſs of the rock over it, as is thought convenient to be blown up at one time; for at the farther end of this perforation (which tends upwards) there is placed a convenient quantity of gunpowder, and then all the reſt of the cavity being filled with ſtones and rubbiſh ſtrongly rammed in, (except a little place, that is left for a train,) the powder, by the help of that train being fired, and the impetuous flame being hindered from expanding itſelf downwards, by reaſon of the newly mentioned obſtacle, concurring with its own tending another way, diſplays its force againſt the upper parts of the rock, which, in making it ſelf a paſſage, it cracks into ſeveral parts, moſt of them not too unweildy to be manageable by the workmen.

AND by this way of blowing up rocks a little varied and improved, ſome ingenious acquaintances of ours, employed by the publick to make vaſt piles, have lately (as I received the account of themſelves) blown up or ſcattered, with a few barrels of powder, many hundred, not to ſay thouſand, tuns of common rock.

TO give ſmall glaſſes the ſhape, that is requiſite to fit them to ſerve for covers to the dial-plates of watches, and for other purpoſes, to which artificers ſometimes employ them, one would think it neceſſary, that they ſhould be ground, or otherwiſe wrought with tools, by a ſkilful hand, to give the glaſſes the concave, as well as the convex figure they ought to have. And yet I have learned by trial, that a flat plate of glaſs of a competent thickneſs, that has its two ſurfaces ſmooth and parallel to each other, being carefully laid upon a deep ring of iron, or a ſhallow and hollow cylinder of the ſame metal, and of the diameter required, ſo that the edge of the glaſs (which is to be reduced to roundneſs) may every where reſt upon that of the cylindrical piece of metal; the heat of the fire warily and ſkilfully adminiſtred will ſo ſoften this plate of glaſs, that its own weight will ſo depreſs the middle parts, that the glaſs will thereby obtain the figure required. And though ſuch glaſſes do not conſtantly fall juſt into the deſired figure, yet when they are ſkilfully ordered, they fall into it ſo often, that I am told, that ſome ingenious artificers have quitted the ordinary way of making covers for watches, for that we have been deſcribing; which, though not free from caſualties, is yet ſo much more cheap and eaſy.

WE have in ſome parts of *England* various kinds of talk, or *lapis ſpecularis*, (ſeveral of which I have been poſſeſſor of,) and of ſome of them there is ſo great plenty, that one may procure good ſtore for little or no charge: but the reducing of a great lump of this talk to fine powder, if it muſt be done the common way, by beating it in mortars, and ſearcing it often, will require much time and pains; but as I have ſeveral times tried, the ſmaller pieces may, by the help of an actual flame, be quickly reduced to a ſnow-white calx; ſo by the experiment of a ſagacious acquaintance of mine, even great lumps of it may, almoſt in a trice, be brought to fine powder, by heating them red-hot, and caſting them, while they are ſo, into cold water, whereby there will preſently be made a comminution of them into a fine, and as it were, mealy calx.

THE ground of this operation is much the ſame with that, whereby ſome chemiſts granulate maſſes of gold and ſilver, when they pour the ſtrongly melted metal from a competent height into cold water, whereupon there happens a diſſilition of the parts of the metal; many of which fall to the bottom in little fragments. But the more eaſily fuſible metals, tin and lead, may be quickly and better granulated by the mechanical way, freſhly mentioned, as to talk. I remember, I was wont (eſpecially if the ignition and extinction were repeated two or three times) to reduce cryſtal flints, almoſt in a trice, to a fitneſs to be eaſily brought to a very ſubtle powder, proper to make amauſes (or counterfeit gems) of.

THE mention I have already made in this eſſay, of what may be performed by the faculty, that burnt alabaſter, made liquid with water, has to grow hard again, puts me in mind of another inſtance, very properly referrible to the ſubject of this eſſay. For one, that beholds how curiouſly oranges and lemons, and other fruits are counterfeited in wax, would imagine, that ſo lively a repreſentation of them could not be effected, but by a hand, as ſkilful at leaſt as that of a painter; ſince by this plaſtick art, not oranges, and lemons, &c. in general, but this or that particular orange or lemon may be moſt lively repreſented; and yet you may learn this art within one hour or two, the thing being performable eaſily and quickly: for having the orange, &c. we would imitate, we bury it half way in a coffin of clay, whoſe brim, together with the extant part of the fruit being oiled over, to keep the mixture from ſticking, the tempered alabaſter (or plaiſter of paris) is nimbly laid on to a good thickneſs, and, upon its concretion, removed, whereby you obtain an half-mould for that part of the orange; then the formerly latent part of the fruit being likewiſe placed uppermoſt in the half-mould, which ſhould have ſome pretty deep notches cut in the rim of it, which, with the protuberant part of the fruit, ought to be oiled, the tempered mixture is likewiſe put upon that, and thereby an exact mold is compleated, at any convenient part of which a hole being made, to pour in a little tempered and coloured wax, when it is brought by fuſion to a due heat, (for every degree of that quality is not convenient,) ſhaking the mold nimbly and every way, the wax comes to be ſo applied to the internal ſurface, that when the mold is cold, and the parts taken aſunder, you have an orange of wax very lively repreſenting the original.

THERE are ſome circumſtances belonging to this eaſy and delightful art of molding and caſting in wax, (which is pleaſant enough to be practiſed even by ladies) that I purpoſely omit; what has been mentioned being ſufficient to ſhew you as much as is neceſſary for my preſent purpoſe. And I the rather pitched on this experiment, becauſe it may afford us another inſtance, not impertinent to the deſign of this tract. For one, that ſhould ſee how great a cavity is left within the counterfeit orange, would think, that there were ſome great and rare artifice requiſite to caſt it thus hollow, and make ſo ſmall a quantity of wax reach to the counterfeiting of ſuch a fruit; whereas the bare ſhaking of the mold, when the melted wax is in it, together with the expanſive endeavour of the included air, applies the wax to every part of the inſide of the mold, and thereby turns it into one great film, which one would think it very difficult to ſeparate, without injuring it, from the mold, to which it is applied ſo cloſe: and indeed it might be ſo, if nature did not again aſſiſt the artiſt, by making the mixture, when it cools, ſhrink a little, and thereby part eaſily from the mold it ſtuck to.

BUT one of the pretticſt and the ſtrangeſt artifices, that belong to this eſſay, is that, whereby the knowledge of a few unheeded phyſical properties of two or three bodies may enable a man to perform that, which ſeems to require, not only good tools, and great dexterity in the art of graving, but likewiſe an exquiſite ſkill in caligraphy, or the art of writing fair: for I know a graver, famous for ſkill in his profeſſion, who writes, as I have had good opportunity to obſerve, but a bad hand; and yet this man with his tool writes rarely well, and will imitate and emulate the fineſt copies of the choiceſt writing-maſters, ſo that even virtuoſi have much admired how a man, with a ſtiff iron tool upon a tough copper-plate, can write incomparably fairer, than the ſame perſon can with a good pen upon paper. But to eaſe you ſomewhat of your wonder, I ſhall add, that though this artifice be kept for a choice ſecret, and though I could not learn a conſiderable particular or two, which belong to the delicacy of it; yet (partly by putting queſtions, and partly by ſome trials of my own) I attained to the ſubſtance of this myſtery, as they call it, which ſeems to be this.

A writing-maſter, or ſome other, that writes a very fair hand, is deſired to write a copy, or what elſe is to be engraven, with a peculiar kind of ink, which differs not in ſhew from common ink, being fully as black as it. Then they take a very clean and well-ſmoothed copper-plate, which being moderately warmed, is to be ſo rubbed over with a certain white varniſh, or ſomething equivalent (to be mentioned a little beneath,) that when the plate grows cold again, it may be thinly and evenly caſt over with a kind of ſkin or film (if I may ſo call it) of varniſh; then lightly moiſtening the paper, that it may part with its ink the more readily, the written ſide is to be laid on the prepared ſide of the plate, and that, together

ther with the paper being paſſed through a rolling-preſs, enough of the ink will ſtick (but in an inverted poſture) to the varniſh, whoſe whiteneſs renders the black letters very conſpicuous; ſo that it is eaſy with a needle, fitted with a wooden handle, to draw over the very ſame lines and ſtrokes through the yielding varniſh upon the metalline plate, whence they may, after the plate is by heat, or otherwiſe, freed from the varniſh, be compleated with a graver; and laſtly, when the whole engraving is finiſhed up, may be printed off in a rolling-preſs like ordinary cuts. And even without a rolling preſs I have ſometimes taken off written characters, only by laying the moiſtened paper very ſmooth upon the varniſhed copper, and rubbing it hard thereon with a convex piece of glaſs, or ſome ſuch ſmooth and hard body, whoſe preſſure makes the ink ſtick to the varniſh, for which I have uſed the purer ſort of virgin wax, if the ink be good, and have been laid on plentifully enough by the pen. That ink, which I moſt uſed, I made only of fine Franckfort black, as the painters, that ſell it, are wont to call it; by grinding it little by little, but very diligently, with water, till it had attained the conſiſtence of a ſomewhat thick ink; in which this only circumſtance is carefully to be obſerved, that no gum be added, as is uſual in other inks, leſt that hinder its coming off.

AND here it will not be impertinent to the argument in hand to add another artifice, whereby a printed cut may be ſo far taken off, that at leaſt the out-lines and the principal ſtrokes may be ready copied for the graver's hand; by which way, beſides other uſes that may be made of it, copies of rare and choice pieces may be procured, and the periſhing or want of the originals ſupplied: if then the print to be taken off be recent enough, (as it is wont to be, if it exceed not a year, or perhaps two,) then the paper needs only be well moiſtened, as if it were to be printed off at a rolling-preſs (with the ink proper to which, it is ſuppoſed, that the cut was, as uſually cuts are, printed off:) but if the picture or ſcheme be more ancient, it muſt be laid all night to ſoak in water, and then hung in the air, till it have but ſuch a degree of moiſture as makes it fit for the rolling-preſs. The paper being thus prepared, either by bare wetting, or by ſteeping, the printed ſide is to be laid upon a copper plate, thinly caſed over, as was formerly directed, with virgin-wax; for the plate and paper being put into a rolling-preſs, the compreſſion of that will make the moiſtened ink ſtick to the pure wax, which conſequently will take the impreſſion of the cut, or at leaſt of the outlines and chief ſtrokes of it.

THERE is another thing, which ſeems above all theſe to require the expreſs and immediate operation of the hand, and it is a phyſical way, if I may ſo ſpeak, of tranſcribing a whole page of a letter, or other writing, all at once. Whether this can be performed cheaply and eaſily enough for common uſe, is hereafter to be conſidered. But that, abſtracting from theſe circumſtances, it is poſſible to be done, (by an artificial application of phyſical things) I have been perſuaded by ſome experience; of which I may in one of the following papers give you a more particular account, than I now conveniently can.

IN the former part of this eſſay, *Pyrophilus*, I have preſented you ſome inſtances, wherein phyſiological knowledge may be ſubſtituted for manual dexterity, mechanical tools, and even mathematical inſtruments: but now to ſhut up this diſcourſe, I ſhall ſubjoin a relation, that will manifeſt, that even a mathematician and an engineer may ſometimes perform that by the knowledge of a ſlight phyſical quality of obvious bodies, which, without that knowledge, all his ſkill in mathematical diſciplines, and his vaſt and artificial engines, will not have enabled him to accompliſh. For who would think, that by a comparatively few pounds of water (perhaps the moiſture of the air in wet weather might have ſufficed) a maſſy body of peradventure ſome hundred thouſand pounds in weight ſhould be raiſed; and yet, that this was performed at *Conſtantinople*, is one of the remarkableſt things I remember I met in the ingenious account of his voyage, that is given by the learned *Busbequius*, embaſſador from the king of the Romans to the Turkiſh emperor. His words are theſe. * *De obeliſco, cujus suprà memini, qui eſt in hypodromo, ſic Græci commemorant; à baſi convulſum multis ſeculis jacuiſſe humi: tempore poſteriorum imperatorum repertum architectum, qui operam ſuam in eo ſuæ baſi reſtituendo deferret; illumque, poſtquam de pretio conventum eſſet, ingentem apparatum organorum ex trochleis & funibus præſertim inſtituiſſe, quibus lapidem illum ingentem erexerit, ſublimemque eo evexerit, ut uno tantùm digito abeſſet à dorſo aſtragalorum, quibus imponi debebat, tum indicâſſe populum ſpectatorem oleum illi operam tanti apparatus periiſſe, magniſque denuo laboribus & impenſis opus inſtaurandum: at illum minimè diffiſum perito à rerum naturalium ſcientia ſubſidio juſſiſſe afferri immenſam aquæ vim, qua multis horis in machinam illam injecta, funes, quibus obeliſcus librabatur, ſenſim madefactos rigenteſq; (ut eorum eſt naturæ) ſe contraxiſſe, ſic ut obeliſcum altiùs ſublatum in aſtragalis ſtatuerunt, magna cum admiratione & plauſu vulgi.* And for confirmation of this narrative, it may be added, that the ſame thing is mentioned by good authors, as having been practiſed elſewhere; and a like ſtory is allowed, and ſomewhere made an argument of, to another purpoſe, by that great maſter of mechanicks *Galileo* himſelf.

To catch any ſtore of fiſh the ordinary way, you know it is cuſtomary, that even in rivers, either ſtore of angles, and ſome ſkill in uſing them, or nets, or ſome other artificial inſtruments be made uſe of; and if it be in the ſea, that men are to fiſh, large nets or ſome peculiar contrivances are employed as neceſſary; and one would not expect from ſuch people as the Americans, eaſier ways of fiſhing than theſe, and yet theſe illiterate barbarians, by having found out (probably by chance) the phyſical property of a wood, make that ſerve them to catch fiſh in great plenty, and with as much eaſe. For our late Engliſh navigators have obſerved, as their voyages witneſs, that in ſome parts of the *Weſt-Indies*, the natives, by impregnating the water with this wood, do ſo ſtupify the fiſh, that rolling up and down upon the ſurface of the water, as if they were foxed, they are eaſily taken up in great numbers in their hands: which relation of our ſeamen I therefore, notwithſtanding its ſtrangeneſs, ſcruple not to alledge, partly becauſe, that though we do not uſe a ſimple drug, much leſs a wood, for the ſame purpoſe, yet our foxing-ſtuff, as they call it, which is but a ſlight compoſition, produces effects not much inferior; and partly, becauſe having purpoſely enquired of a learned phyſician, that came not long ſince out of a part of *America*, where this practice is in requeſt, he aſſured me, that he ſaw the Engliſh themſelves uſe this way of fiſhing, only by tying a log of this wood, to which, for what reaſon I know not, they have given the name of dogwood, to the ſtern of their boats; ſo eaſily does the odd property of this wood enable them, that make uſe of it, to catch fiſh.

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* Aug. Busbequii, Epiſt. I. pag. 69.

To take off the hair is generally ſuppoſed to require both a razor and other implements, and the manual ſkill and operation of a barber, eſpecially if the hair be grown under the armpits, and in other places, which an inconvenient ſituation or figure makes to be of difficult acceſs; and yet by the knowledge of a property of that natural production, formerly mentioned in the ſixth eſſay under the name of *Rusma*, the hair may be, without inſtruments, taken off from any part of the body, and that not only in much ſhorter time than is required to ſhaving, but, as far as the eye is wont to diſcern, by the roots, which makes it much longer before the part be again covered with hair of the former dimenſions. The way uſed in the eaſt to effect this the forecited *Bellonius* annexes, inſtead of which I ſhall tell you what I tried with a parcel of it, brought into *England* before I met with his obſervations about it. We mixed the fine powder of it with an equal weight of ſtrong powdered quick-lime, (*Bellonius*, probably not without reaſon, preſcribes but half as much quick-lime,) and having ſuffered them to ſoak together a ſhort while in a little fair water, we thinly ſpread the ſoft paſt or ſlime, made by the water and ingredients, upon that part of the body, which we deſigned to free from hair; and having ſuffered this mixture to ſtay on about three minutes, or ſixtieth part of an hour, meaſured by a minute-watch, (our author preſcribes as long time as is requiſite to the boiling of an egg,) we wiped it off with a linen cloath dipped in warm water, and found the hair taken off by the roots, without any inconvenience to the part, that we could diſcern, though I ſeveral times ſhewed the experiment to others, and the trial of it was more than once made upon my ſelf.

IT may ſeem ſcarce poſſible, without the help of water, or any engine made with ſprings or wheels, to meaſure time, though but for a little while, as exactly as our beſt clepſydra's, clocks, or watches, are wont to do. And yet (which is now a known, and almoſt vulgar thing) ſuch an account of time may be kept by him, that has obſerved, that the vibrations or diadroms of a pendulum, are made in ſenſibly equal ſpaces of time, though the arches continually decreaſe, that are made by the ſwinging pendulum, (as you know they now call a bullet, or the like weight hanging at the end of a ſtring from a nail, or other fixed ſupporter.) For by ſo ſlight a thing, as I have been mentioning, if you watchfully obſerve and reckon the returns, that the ſwinging weight makes towards you in a minute, or other determinate ſpace of time, doubling the number of thoſe returns, and adding thereto an unite, if you left off counting, when the weight was at the further end of the arch deſcribed by its motion, you may obtain a more accurate diviſion of time, than by any of the formerly known ways of meaſuring it. For if you make your pendulum of the length of very little (perhaps a tenth of an inch) leſs than ten inches (or twelve parts of our Engliſh foot*) accounted from the nail, or other thing, whence it is ſuſpended, to the center of the piſtol-bullet, (or the like ſmall round weight;) and, removing this a pretty way from the perpendicular it naturally reſts in, ſuffer it to fall gently out of your hand, each of its two ſwinging motions (the one whereby it is carried from you, and the other whereby it returns to you) will be (eſpecially, whilſt the arches are of a moderate length) phyſically *æqui-temporaneous*; and theſe motions will very diſtinctly enough, to an attentive eye, divide a minute, or ſixtieth part of an hour, into an hundred and twenty parts, (called half ſeconds,) and will conſequently divide an hour into ſeven thouſand two hundred parts, if not perfectly equal, yet leſs unequal, as to ſenſe, than the diviſions of time made even by good watches are wont to be. And therefore this way may be of very great uſe, in making aſtronomical and other obſervations, that laſt not long, but require exact meaſures of time. And by the help of a pendulum, a ſkilful muſician of my acquaintance, teaches his unpractiſed ſcholars to keep time when they ſing in his abſence. But when we meaſure experiments by the excurſions and returns of a weight, the beſt way is to make the duration of the pendulum's whole motion (before it come to reſt) as long as the place where the experiment is made will permit, renewing now and then, if need be, the impulſe given to the weight, when the arches begin to grow too ſhort; that being increaſed, the vibrations may be the better reckoned.

THE mention I have been making of the uſes of pendulums, joined to that I lately made of æquivelocity of ſounds, brings into my mind another inſtance pertinent to this part of our diſcourſe. For it is not impoſſible, by the knowledge of the velocity of a ſound's motion in the air, and the æquivelocity (as to ſenſe) of great and ſmall ſounds, to meaſure without geometrical inſtruments, in ſome caſes, the breadth of a river, though exceeding wide, or the diſtance of the place one ſtands in, from the top of a high tower or hill on the other ſide of a river, or ſituated in ſome inacceſſible place, and this, in caſes, where the difference of ſtations uſually in geometrical menſurations is not allowed. The way is evident by what is elſewhere delivered. For it having been found by *Merſennus*'s trials, that ſounds (as well ſmall as great) do move in a ſecond (as they call the ſixtieth part of a minute) 230 fathom, or thirteen hundred and eighty foot; if I ſee my correſpondent fire a gun on the other ſide of the river, or if I ſee muſkets or other guns caſually fired on ſome tower or baſtion, though never ſo far diſtant, and never ſo inacceſſible to me, it is eaſy for me, by letting fall a ſhort pendulum, as ſoon as I ſee the flaſh of light produced by the kindled powder, and by reckoning the vibrations (made by that ſhort pendulum, which diſtinguiſhes ſeconds into halves or quarters) that ſhall happen to be made before the noiſe arrive at my ear, to know how far off the place, where the gun was diſcharged, is from that I am in. As if a correſpondent, ſtanding over againſt me on the other ſide of a river, or ſome ſoldiers being there exerciſing, I ſee the flaſh or ſmoak of a muſket, or other gun, two ſeconds ſooner than I can hear the report of it, I may conclude the river to be 2760 foot broad; and if a piece of ordnance being fired upon the tower of a beſieged place, the noiſe arrive at my ear in half a ſecond, I may collect 690 foot to be the diſtance betwixt that gun and my ſtation. And by this means may that problem be performed, that we elſewhere mention as a thing, which, when nakedly propoſed, may ſeem impoſſible. For if I ſee a ſhip at ſea be ſhooting, whether in earneſt, or for ſalutation, or for joy, it is very poſſible for me to meaſure, without geometrical inſtruments, how far it is off, though the ſhip itſelf be under ſail. For veſſels, that fire guns, uſually firing more than one, whether to offend their enemies, or to ſalute their friends, it is eaſy to take warning, by the firſt gun, to be in readineſs with a ſhort pendulum againſt another to be fired, and in this way of meaſuring (though not in any other yet known) one may take diſtances in the darkeſt night. For it matters not, whether I ſee the ſhip or place, whoſe remoteneſs from me I would know, provided, by ſome candle or taper I ſee the pendulum before the flaſh of the fired gun, which will ſufficiently diſcover itſelf by its own light. And (to add, that upon the by) I have had ſometimes thoughts, that if the velocity of eccho's, which are but reflected ſounds, be ſo well determined as that of direct ſounds, navigators might ſometimes make uſeful eſtimates in dark nights, whether they be near coaſts, or conſiderably great rocks. For though upon diſcharging a gun, they cannot conclude, how near the ſhore they are, becauſe there may be parts of it leſs remote than thoſe that ſend the eccho; yet if they follow very quick upon the diſcharge of the gun, they have reaſon to ſuſpect, that the ſhore, whoſe approach the ſeamen do ſo juſtly fear in the night, is at leaſt, as near as the vibrations of the pendulum inform them, that the ecchoing place is.

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* N. B. The author has elſewhere ſhewn, that the Engliſh foot differs very little, if at all, from the ancient Romans.

ESSAY X.

OF MEN'S GREAT IGNORANCE Of the USES of NATURAL THINGS:

OR,

That there is ſcarce any one Thing in Nature, whereof the Uſes to human Life are yet thoroughly underſtood.

THIS being an entire propoſition, and clear enough of itſelf, will not need to be explicated, but evinced.

AND evinced ſomewhat ſolemnly it will require to be, not only becauſe it is a paradox, but ſuch an one, as will meet with a peculiar indiſpoſition to be entertained; ſince men cannot allow this paradox to be a truth, without ſuch a confeſſion of their ignorance, as muſt implicitely accuſe them of lazineſs too. But however, I think, we may juſtly enough apply, with a little variation, to our preſent purpoſe, that true ſaying of *Seneca*, *Multi ad ſapientiam perveniſſent, niſi*, &c. and affirm, that many had attained to a greater knowledge and command of nature, if they had not preſumed, that what is arrived at already, is much greater, and more conſiderable than indeed it is; eſpecially, in compariſon of what is ſtill behind, and yet attainable: and therefore, I think it not fit to ſuppreſs the conſiderations I was about to mention, ſince the diſplaying them may perhaps do you and others ſervice, if they rouze up your curioſity, by ſhewing how much it has been defective, and if (which they ought to do) they encourage it alſo, by ſhewing you how much of nature undiſcovered there yet remains, to recompence, as well as exerciſe your induſtry.

BUT becauſe that of the particulars, whereby our paradox may be confirmed, there are divers, that properly belong to the next enſuing eſſay, the proofs, that we ſhall mention in this diſcourſe, though I hope they will appear ſufficient alone, will yet be much ſtrengthened, both as to number and weight, if you pleaſe to add to them thoſe inſtances to be mentioned in the next diſcourſe, that may be conveniently referred to this. In which I ſhall therefore inſiſt but upon five general conſiderations; in all which I hope you will not forget, that I have already taken it for a ſuppoſition, which I doubt not of your granting me, that the uſefulneſs of the works of nature to us depends chiefly upon the knowledge we have of their properties and other attributes; and conſequently, that the more we know of theſe, the greater uſe we are like to be able to make of thoſe phyſical things, (and on the contrary.) And therefore, that ought to be looked on as an uſe of a phyſical thing, even though not immediately practical, that helps us to make diſcoveries of things, that properly may prove ſo.

SECTION I.

AND I conſider in the firſt place, "That "there are very few of the works of na-"ture, that have been ſufficiently conſidered, "and are thoroughly known," even as to thoſe qualities, and other attributes of this and that body (or other phyſical thing) which belong properly to it, and are not thought to be ſo relative to other bodies. It is not only in the terreſtrial globe, but in almoſt every body to be met with in it, that there may be a kind of *terra incognita*, or undetected part, whoſe diſcovery is reſerved for our further induſtry.

THIS will appear the leſs improbable, if we conſider theſe two things; whereof the one is, that there are divers ways of inveſting the attributes of bodies, as chemical, optical, ſtatical, &c. which being artificial, and requiring ſkill, and induſtry, and inſtruments, there are very few men, that have had the curioſity and ability to examine them after theſe ſeveral ways: without which, nevertheleſs, divers other attributes, ſome of which probably are capable of uſeful application, are not like to diſcovered. To the proof of which, if it were needful, a multitude of paſſages in theſe preſent eſſays, as well as in our other writings, might be eaſily referred.

I ſhall therefore rather inſiſt a little on the ſecond of the two particulars lately mentioned. For it will eaſily appear not unlikely, that there ſhould be many things undiſcovered in the others works of nature, when there are ſo even in thoſe obvious and familiar objects, that men are frequently converſant with, and have occaſion to take notice of; nay, even in thoſe nobleſt of mere corporeal things, our own bodies, whoſe ſtructure does ſo much merit our curioſity, and of which it ſo highly concerns no leſs than our healths and lives, that we have an accurate knowledge. How many new diſcoveries have been made in the preſent age, beyond what the induſtry of the phyſicians and philoſophers for above two thouſand years has been able to take notice of? Witneſs the circulation of the blood, the Aſellian, Pecquetian, and Bartholinian veſſels; to which may be added, the Ductus Pancreaticus, and to which I doubt not will be added divers other diſcoveries, to recompence the induſtry of the anatomiſts of this inquiſitive age.

IN ſo familiar bodies as eggs and chickens are, which ſo many thouſand perſons do daily ſee and handle, and perhaps eat, though many ages ſince, even *Ariſtotle* was ſolicitous about the hiſtory of them, concerning which he has delivered divers not inconſiderable particulars; yet there has been little within theſe few years ſo much undiſcovered, that whilſt men were hotly diſputing, whether the chick was firſt formed of the yolk or the white, our excellent *Harvey* made it evident (which our own obſervations have confirmed to us) that it is made of neither, nor yet of the tredle, (as ſome modern obſervers have taught,) but of the cicatricula, or ſpeck, that appears on the coat of the yolk.

WHO would imagine, that in a body ſo familiar, and ſo often treated of by philoſophers, as ſnow, mankind ſhould, for ſo many ages, take no notice of a thing ſo obvious as the figure of it frequently is? and yet *Kepler* is, by a very learned writer, acknowledged to have been the firſt, that acquainted the world with the ſexangular figure (as it is wont to be called) of ſnow, in a diſcourſe by him publiſhed on that ſubject. And though I find mention made of it in *Olaus Magnus*, and have obſerved it ſo often (but not conſtantly in the ſame ſhape,) eſpecially about the beginning of the ſeaſon of ſnow, that I cannot but admire, men ſhould not have very early heeded ſo obvious a phænomenon; yet I find not the diſcovery of it had been made ſo much as an age ago.

AS many ages as vinegar has been one of the commoneſt liquors in the world, yet, that it oftentimes abounds with ſhoals of living creatures, that move, and in the microſcope look like little eels, was looked upon but few years ſince as ſo new a diſcovery, that when, as I formerly noted, I firſt propoſed it here in *England* to divers very learned men and virtuoſi, as a thing to be ſeen even without the help of a magnifying glaſs, they took it to be a deception of my eyes, till their own aſſured them of the contrary.

THAT the milky way, though conſiſting of innumerable ſtars, ſhould for two thouſand years paſs for a meteor, the inconſpicuouſneſs of thoſe ſtars keeps me from much admiring. And for the ſame reaſon I wonder not, that the men, that lived before *Galileo*, reckoned no more than ſeven planets, or ſuſpected not, that *Venus* herſelf is ſometimes horned, and has her full and wane as the moon. Though theſe inſtances may ſerve to confirm what I lately told you, that many of the attributes of bodies are not like to have been diſcovered by thoſe, that employed not artificial helps. But what may we not expect, that mankind may overlook, when the ſun himſelf, which is not only the moſt conſpicuous body in the world, but, that by whoſe light, we ſee all the others, may have vaſt and dark bodies (perhaps bigger than *Europe* or *Aſia)* frequently enough generated and deſtroyed upon him, or about him; and men, without excepting aſtronomers, never took notice of it, till of late years the excellent *Galileo*, or the induſtrious Jeſuit *Scheiner*, informed the world of them. And though I grant, that they diſcovered them by the help of teleſcopes, (inſtruments unknown to the ancients,) yet if men had been as watchful, as the nobleneſs and conſpicuouſneſs of the object would make one expect, they might have diſcovered ſome ſpots at leaſt, without thoſe helps. For I find by an Italian letter of *Galileo*'s, that ſome curious perſons of his acquaintance, after his diſcoveries had awakened them, deſcryed and diſcovered ſome of thoſe ſolar ſpots with their naked eyes unaſſiſted by his tubes.

IT may belong to this firſt ſection of our preſent eſſay to take notice, that one account, on which we may reaſonably ſuppoſe men to be ignorant of the uſes even of thoſe things, wherewith they think themſelves well acquainted, may be, that the bare difference of climates, and of places, may even in ſuch bodies, as we familiarly converſe with, beget ſuch new relations betwixt them, as may endow them with qualities, and fit them for uſes we dreamed not of.

I will not here mention the differing qualities, that bodies vulgarly referred to the ſame ſpecies of plants, animals and other bodies, in almoſt all countries, are endowed with in ſome countries; (as, that ſpiders are not venomous in *Ireland*, and Iriſh wood in general, if the received tradition be true, has an hoſtile faculty againſt venomous creatures,) becauſe the inſiſting on this ſubject would take up too much room in this place, and is reſerved for another; and therefore I'll only add a couple of inſtances, the one to manifeſt what difference of climates may do, and the other to ſhew the unexpected influence of difference of places, though perhaps in the ſame climate.

THE firſt of theſe examples is afforded us by water and ice; for thoſe, that live in thoſe warmer regions, where it never freezes, and who have divers of them derided the relations of what happens in gelid climates as ridiculous, in probabilty would never dream, that it could be a familiar uſe of a liquor they were ſo well acquainted with as water, to be broken or beaten in mortars like a dry body, and carried in carts or wheel-barrows from place to place, and kept all the year in that form, to make other water intenſly cold in the greateſt heats of ſummer. And even amongſt us, thoſe, that have not been very inquiſitive, can ſcarce imagine, that one of the uſes of water ſhould be to ſerve for high-ways, whereon armies may march for divers days together, with all their carriages and artillery, and whereon they encamp and fight battles with as much aſſurance as on the firm land; and yet thoſe, that have been in *Ruſſia*, and the neighbouring northern countries, aſſure us, that during the winter, when the rivers are frozen over, they uſually take great journies on them, and oftentimes rather than in ſummer, and chooſe that rigorous ſeaſon, which allows them to march every where without ſinking into the ground, to proſecute their wars in.

The ſecond of the forementioned inſtances we are ſupplied with by the declination of the magnetick needle from the true north and ſouth points, and the variation of that declination. For though the loadſtone were highly admired as well by philoſophers and mathematicians, as the vulgar; and though, ſince the great and happy uſe of it to navigation has been generally known, men have been upon ſeveral accounts invited to conſider it with a peculiar attention and regard, yet that in ſome places the magnetick needle does not point directly, perhaps not by a great many degrees, at the pole, as in others it does, is no ancient obſervation, ſince it is aſcribed to *Sebaſtian Cabot*; and it appears by the writings of our famous countryman *Gilbert* himſelf[*], that it muſt be ſome body, that lived ſince he wrote, that muſt have the honour of being allowed the firſt obſerver of that ſtrange and unexpected phænomenon, that oftentimes in the ſelf ſame place, the declination of the needle towards the eaſt or weſt does in proceſs of time conſiderably alter. Which diſcovery I could confirm, by comparing ſome obſervations I have had opportunity to make, with thoſe recorded by ſome modern authors.

AND as the ſame kind of bodies may have differing qualities, and conſequently uſes in differing places; ſo they may have, if examined or employed at differing times, compriſing under that name, together with the four ſeaſons of the year, thoſe peculiar ſeaſons or periods of time, to which ſome ſignal change of qualities or ſtate in particular bodies do belong.

THE mutations, upon the account of time, which I am here ſpeaking of, are not thoſe, that are obvious to every eye, ſuch as the differing qualities of fruit green and ripe, or the degeneration of wine into vinegar; but ſuch as are not vulgarly taken notice of, and require either ſkill or curioſity, or both, in the obſerver; and of theſe a few inſtances will ſuffice for a taſte.

WHEN common urine either is freſhly made, or has not long been kept, the volatile and pungent ſalt is ſo clogged with other particles, wherewith it is aſſociated, that uſually, to obtain it, one muſt evaporate or diſtil away near eight or nine parts of ten of the liquor, and then at length give a not inconſiderable heat to force up the laſt: but though the tradeſmen, that deal in urine, do commonly overlook the difference, yet if the crude liquor be kept ſix or ſeven weeks, though not near the fire, the ſaline and noble parts will have ſo extricated themſelves, that a very gentle heat will make them aſcend, and leave behind them that phlegm, that formerly would have preceded them.

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[*] *Gilbert* de magnete, lib. I. c. 1. See the ſame *Gilbert*, lib. IV. c. 3.

That the Thames water, which our navigators are wont to take with them in long voyages, after a while, if they ſail into hot climates, ſtinks very often too offenſively to be potable, that, which happens uſually to water, which is vulgarly obſerved to putrefy by long ſtanding, will eaſily perſuade us; and yet it is found, that this water, by being kept long enough in the ſame veſſels, though it be in the ſame, or even in an hotter climate, will at length looſe its ſtink, and grow potable again; as I have, upon enquiry purpoſely made, been aſſured, not only by the vulgar tradition, but by two very inquiſitive perſons upon their own knowledge; the one having particularly obſerved it, ſailing betwixt *Europe* and *Africa*, and the other in a voyage to, and from *America*. And I the rather mention this, becauſe I am very credibly informed, that there are divers other waters, that have this faculty of recovering after putrefaction, which is ſuppoſed to be peculiar to the water of the Thames.

And, if I much miſtake not, one or both of theſe very perſons named another river to me, with an affirmation of its having the ſame power of ſelf-recovery. And having held ſome curioſity to try experiments, how pump-water, or the like rough water, as they call them, that would not bear ſoap, may be helped; an induſtrious perſon I employed aſſured me, that he met with pump-waters, which after having ſtood a few days, without having any thing done to them, would bear ſoap, which before they would not do.

Coriander ſeeds being freſhly gathered have been obſerved to have ſo much acrimony, that divers of the ancient phyſicians reckon them among venemous plants; and in diſpenſatories they are uſually preſcribed to be prepared with vinegar, or ſome other corrective: whereas the more accurate obſervers take notice, that within a competent time after the ſeed is gathered, it loſes of it ſelf that exceſſive acrimony, that at firſt blemiſhed it. And the like I find obſerved, by good apothecaries, of the roots of aron, which are mitigated by keeping, (and which ſome noted phyſicians of my acquaintance do little leſs magnify to me than does *Quercetan* himſelf.)

[That vegetables, what known way ſoever they are wont to be laid up, and ordered, do not afford, unleſs firſt reduced to ſoot, any dry volatile ſalt, like that of animal ſubſtances, I elſewhere more particularly declare, and thoſe, that have had the curioſity to try it, will confirm: but yet by ſome diſcourſe I lately had with a very ingenious perſon, and ſome ſubſequent trials made after a way I deviſed to examine diſtilled liquors, I was ſatisfied, that there are divers vegetables, and thoſe very commonly growing here in *England*, which being gathered and laid together at a certain ſeaſon, and diſtilled alſo at a certain nick of time, will yeild, inſtead of the vinegar-like, and other liquors, wont to be afforded by ſuch plants diſtilled the common way, a volatile ſpirit; which in ſmell, taſte, and divers operations, as turning ſyrup of violets green, hiſſing with acid ſpirits, &c. reſembles the volatile ſpirits and ſalts of animal ſubſtances; and, which I doubt not but you will wonder at, this great change, whoſe ſecret I wiſh I durſt teach you, is effected without the help of any additament.]

And, that you may not think, that it is only in vegetable and animal ſubſtances, that are commonly of a more looſe or alterable texture, that the trying things at one time rather than another may be very conſiderable, I will add a couple of inſtances, even in mineral bodies.

It is a chemical complaint, even of the curious and experienced, that though authors teach us to make the ſalt of violently diſtilled or calcined vitriol, by forthwith taking the caput mortuum, (from which all the oil has been by the violence of fire forced out) and extracting the ſaline part by effuſions of water; yet thoſe, that make exact trials of it find, that when the dark red maſs of powder is newly taken out of the veſſels, it is ſo totally robbed of its ſaline particles, that no affuſion of water will at all obtain from it the expected ſalt. Notwithſtanding which, having purpoſely enquired of ſome, that diſtil great quantities of oil of vitriol, whether or no, when they had made an end of one diſtillation, if they lay by the caput mortuum for a pretty while in the air, they could not find it impregnated enough with new ſaline particles, to be fit to yeild more menſtruum, and be worth another diſtillation? I was anſwered in the affirmative, provided, that (as I mentioned in the ſtate of the caſe) there were a competent time interpoſed between the former and the latter diſtillations. (The reaſon of which, according to my trials and conjectures, may be aſſigned of this odd phænomenon, belongs not to this place, but you will hereafter meet with it in another.)

The ſecond inſtance I promiſed you, is afforded me by ſtones; for there are, and not far from this place, quarries of ſolid and uſeful ſtone, which is employed about ſome ſtately buildings I have ſeen, and which yet is of ſuch a nature, wherein divers other ſorts of ſtone are ſaid to reſemble it, that though, being digged at a certain ſeaſon of the year, it proves good and durable, as in thoſe ſtructures newly mentioned; yet employed at a wrong time it makes but ruinous buildings, as even the chief of thoſe perſons, whoſe profeſſion makes him more converſant with it, has himſelf acknowledged to me to have been found by ſad experience.

rience. But concerning this obſervation, you may expect to meet elſewhere with a farther account.

And though time and place be two of the principal, yet they are not the only circumſtances, whoſe variations may make ſome ſuch attributes diſcovered in natural things, as are not uſually heeded; of which I ſhall mention but a couple of inſtances, becauſe they may ſerve to ſhew you, that ſuch circumſtances, as are thought the ſlighteſt, may afford new uſes even of ſolid and laſting bodies. Skilful artificers, that grind optical glaſſes for tubes, have complained to me, that oftentimes the convex glaſſes they faſhion, will prove veiny, and conſequently, after all their labour, of little value; and yet they are not able to diſcover theſe unwelcome veins in the glaſs, by the moſt careful viewing it againſt the light, till they have ſpent a pretty deal of time about working of it; and even then they are unable to deſcry theſe blemiſhes, if they hold the glaſs at an ordinary diſtance from the eye; but they are obliged to remove it a great way (perhaps ſix or ſeven foot) farther, ſo much may an increaſe of diſtance become ſerviceable, even where one would expect the quite contrary.

But probably you will look upon poſture as a ſlighter circumſtance than diſtance itſelf; and yet Dr. *Gilbert* has obſerved, and I have found it true by many trials, that long irons, as the bars of windows, that have ſtood upright for a great while, do, by that perpendicular poſture, acquire a verticity or magnetick virtue, as having acquired magnetick poles. So that if you apply the needle of a dial, which I mention as the readieſt way of trial, to the lower part of the bar, it will draw the ſouth end of the needle; whereas the upper extreme of the bar will ſeem to drive away that end, and will draw the northern.

But here I muſt not forget to take notice, that I can ſcarce think men will be able to know all the properties and uſes, even of familiar bodies and other things, till they have mathematically conſidered them; there being ſeveral attributes belonging even to ſuch things, which a naturaliſt, though curious, will probably never find out, unleſs he be both acquainted with mathematical diſciplines, and have the curioſity to apply them to phyſical ſubjects. And though in other eſſays of this book, divers things are delivered, that do directly enough tend to manifeſt what I have now ſaid; yet it is of ſuch importance, that naturaliſts ſhould be thoroughly perſuaded of a truth, that may be ſo much more uſeful than it is yet generally admitted, that I am content to inculcate it, by ſetting down here a few inſtances of ſomewhat a differing ſort from thoſe elſewhere delivered, and more appropriated to the preſent ſubject of our diſcourſe.

You will not doubt, but that ever ſince the firſt ages of the world, the majority of men have had ſome occaſion or other to ſee bodies ſwing; and yet, till *Galileo* (for he is generally believed the diſcoverer) took notice of the vibrations with a mathematical eye, men knew not this property of ſwinging bodies, that the greater and ſmaller arches were, as to ſenſe, equitemporaneous; from which diſcovery have been derived ſeveral practiſes of good uſe, ſome of which have been already mentioned in theſe eſſays.

That water, running out at a hole made in the ſides near the bottom of the veſſel, makes a parabolical line, or one that near reſembles it, and that in ſuch effluxions of water, there is a determinate proportion aſſignable betwixt the perpendicular height of the liquor, and the diameter of the hole, whereby the velocity, and quantity of water, that would run out, may be computed, has not been, that I know of, taken notice of, till the obſervations of the above-named *Galileo*, and the diligent *Merſennus* (to which we may elſewhere add ſome of our own) have endeavoured to define thoſe matters.

As conſtantly as we have occaſion to take notice of the air, and water, and glaſs, yet the curioſity of our modern maſters of opticks has obſerved many things touching the refraction of the beams of light, made in thoſe mediums in different quantities, and to and from the perpendicular, not to ſay any thing of the equality of the angles of incidence, and of reflection made on the ſurface of ſtill water, unheeded by thoſe that are not verſed in opticks: the drops of dew, that hang in numberleſs multitudes upon the graſs and leaves, are things, that every eye has been invited to take notice of by the orient colours the ſun is wont to make them afford us; but till the excellent *Des Cartes*, contemplating them with a more critical eye, found, that in ſuch a determinate angle made at the ſpectator's eye, between the ray of light coming from a certain part of the drop, and the imaginary ſtraight line reaching from the eye to the ſun's center, the drop appeared red, and in another determinate angle exhibited yellow, blue, and other colours, and at other angles, ſhewed no colour at all; the world ignored a conſiderable property of ſpherical diaphanums irradiated by the ſun, and ſeems not to have dreamt of a neat hypotheſis, with which ſome ingenious mens minds are no leſs taken, than their eyes are with thoſe vivid colours of the rainbow, which it pretends to give a clear account of. And though we daily ſee pieces of wood and timber broken by the weight of over heavy bodies, yet till the often named, and ſtill to be commended *Galileo* applied geometry, and the doctrine of proportions to matters of this kind, the reſiſtance of ſolid bodies to be broken by weight (whether their own, or that of other bodies) ſeems not to have been ſo much as ſuſpected to be reducible to ſuch an eſtimate, as he and others have brought it to. And a virtuoſo of my acquaintance, (for whom *Merſennus* laid the way) in a muſical inſtrument, that I have with pleaſure heard him play on, can obſerve a property of metals, that chemiſts thought not of, namely, that equal wire-ſtrings made of differing metals, and having a due tenſion, will yeild ſounds differing as to ſharpneſs by determinate muſical notes, or the diviſions of them. And to theſe I might add divers

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vers other remarks of *Merſennus* and *Galileo* about the force of guns, (which were found to increaſe with their length but till ſuch a number of feet, beyond which the length did but leſſen it) and the parabolical line, in which bullets (that are thought of all other bodies to move the ſtraiteſt) are ſaid to move; and I know not how many other mathematical attributes, if I may ſo call them, of natural things, that geometricians, aſtronomers, engineers, &c. have already obſerved, might be here added, but that I think it ſufficient to ſubjoin one inſtance more, that may well ſerve to keep us from imagining, that even the moſt familiar objects in the world, and that ſeem likely to afford the leaſt diſcoveries, have been ſufficiently conſidered. For how few phænomena in nature are there, that occur to us more frequently than the falling of heavy bodies? And yet though the ancients and *Ariſtotle* himſelf took notice, that there was an acceleration of deſcent in falling bodies, yet we find not, that any ſo much as fairly attempted to determine that acquired velocity, till *Galileo*'s obſervations reduced it to the proportion mentioned in ſome of the former eſſays, wherein moſt of the following mathematicians (for I have ſcarce met with two diſſenters) have acquieſced; and whereby in the eighth eſſay we endeavoured to meaſure heights and depths without geometrical inſtruments. In a word, till geometry, mechanicks, opticks, and the like diſciplines be more generally and ſkilfully applied to phyſical things, I cannot think otherwiſe, than that many of the attributes and applications of them will remain unknown; there being doubtleſs many properties and uſes of natural things, that are not like to be obſerved by thoſe men, though otherwiſe never ſo learned, that are ſtrangers to the mathematicks.

And as I have hitherto obſerved of bodies, ſo I ſhall venture to add of qualities, and divers other natural things, that even thoſe, that are very familiar, may have attributes and uſes, which the generality of men, without excepting thoſe that are otherwiſe learned, are not wont to take any notice of.

That black bodies, for inſtance, as ſuch, are much more ſtrongly and eaſily warmed by the ſun beams than white ones, nay, though the diſparity be not ſo great, than bodies of any light colour, *cæteris paribus*, is perhaps more than even you have taken notice of: and yet I ſhall hereafter have occaſion to prove it by divers inſtances, and you may eaſily try it, either by expoſing for ſome time to the ſummer-ſun a white glove and a black, or a couple of eggs, whereof one is inked, or otherwiſe blacked all over.

Cold is one of the moſt familiar qualities men have to deal with; and though they otherwiſe are not wont to expect much from it, yet leaſt of all would they expect that it ſhould, contrary to the received definition of it, which is, *congregare tam heterogenea quàm homogenea*, that it ſhould, I ſay, perform the office of heat in ſpirit of wine, nay, and in preſenting us ardent ſpirits from beer and other liquors inferior to wine; and yet, not to mention *Paracelſus*'s proceſs of making the eſſence of wine by freezing all the flegm, we have the repeated experiments of navigators into the frigid zone, who aſſure us, that not only from wine, but from beer, by the congelation of the aqueous parts, there may be ſeparated or obtained a liquor, ſtrong, hot, and ſpirituous, almoſt like aqua-vitæ.

And even in our temperate climate ſome odd ſeparations may be made by cold; for, not to anticipate thoſe trials of mine, that belong to other papers, there may, by ſuch cold as we have here, be made a ſeparation in oil, of a liquor much finer and more ſpirituous than the reſt; for I know an eminent artificer, who kept it as a choice ſecret to reſort (as himſelf confeſſed to me he did) in hard froſts to the great jars of oil, where he often found greater or leſſer cracks or chinks in the congealed part of the oil, in which cranies was contained an unfrozen liquor, that appeared thinner and finer than common oil, and was much better than it to preſerve things from ruſting, as perhaps having left many of its ſaline parts in the concreted oil; and for that purpoſe was much prized, not only by him, but by ſome watchmakers, that were made acquainted with the virtue of it.

But it were tedious to inſiſt on all the inſtances, that may be brought of the applications, that may be made of colour, ſound, levity, ſpringineſs, fermentation, and even putrefaction; and it would be not only tedious, but almoſt endleſs to proſecute thoſe inſtances, that might be afforded by other more general and operative ſtates and faculties of bodies. For not only motion and reſt, fluidity and firmneſs, gravity, and the like, have a more univerſal influence of natural things, than even philoſophers are wont to take notice of; but thoſe leſs catholick affections of matter, that are reckoned among but particular qualities, ſuch as gravity, and heat, may have ſo diffuſed an influence, and be applicable to ſo many differing purpoſes, that I doubt, whether all the uſes of that particular degree or pitch of heat, that reigns in fire, will have all its uſes diſcovered, before the laſt great fire ſhall diſſolve the frame of nature.

Nor muſt I pretermit one conſideration more, that belongs to my preſent ſubject, which is, that probably many more qualities, or other attributes, would be taken notice of, even in thoſe natural things, that are reckoned among the moſt known, if men did not want a meaſure of curioſity, that might juſtly be expected. For I ſpeak not here of curioſity in general, (which I doubt not would make far more numerous diſcoveries, than were neceſſary to juſtify my preſent diſcourſe,) but I only ſpeak of ſuch a curioſity about the things of nature we familiarly converſe with, as we could ſcarce want, if it were not out of lazineſs, or a prejudicate opinion, that makes us take that for granted, that we ſhould find to be quite otherwiſe, if we did not chuſe rather to preſume than to try.

Thus, that falling bodies, the heavier they are, the faſter in proportion they fall, has been a received opinion in the ſchools ſince *Ariſtotle*'s time, and has kept the equivelocity, as to ſenſe at leaſt, of bodies of very differing bulks and weights falling from moderate heights, ſuch as ſurpaſs not ordinary towers and ſteeples, from being taken notice of, till of late inquiſitive men by experiments found it out.

THAT water by glaciation is reduced into a leſſer room, has been and is ſtill the opinion, not only of the vulgar, but of the generality of learned men; and yet, that water is not condenſed, but expanded by freezing, he that will congeal that liquor in veſſels ſtrong enough, may eaſily find by trial. And the floating of ice upon water, and the bubbles, that are uſually to be obſerved in it, may alone to ſuffice to make a conſidering man diſtruſt the vulgar opinion.

THAT the common air we breath and live in, is a body endowed with poſitive levity, has been for many ages, and continues to be almoſt univerſally believed; and yet if men had the curioſity to examine this ſuppoſition by one or other of thoſe ſeveral ways, by which the gravity or levity of the air may be diſcovered, they would quickly find, that it is not devoid of weight. And even ſo ſlight a way as the condenſing the air in a blown bladder, by tying a ſtring ſomething ſtrong about the middle of it, may bear witneſs to what we ſay. For though we ſhould oppoſe, as ſome have lately done, that in ſuch caſes the air is not in its natural ſtate, but condenſed; beſides, that is an objection, to which all the expedients of weighing air are no way liable, it makes rather againſt the objectors, than the concluſion, againſt which they urge it; ſince, if the particles of the air be really light, the filling the bladder the fuller of them ought to make it rather lighter than heavier.

THAT greater and leſſer ſounds do, as to ſenſe, move with an equal ſwiftneſs, is that, whoſe contrary is taken for granted; and the more excuſably, becauſe it is evident and confeſſed, that great and ſmall ſounds do not move equally far: and yet, that this equivelocity of ſounds has been made out by the late obſervations of the diligent *Merſennus*, and others, you may remember to have been delivered in a foregoing eſſay, where I alſo endeavoured to ſhew, that this property of ſounds is not unappliable to human uſes.

The VIII.

THAT the loadſtone, which by immediate contact will take up iron, ſhould have ſo ſtrange a property, as to take up far more when a cap, or conveniently ſhaped piece of ſteel is interpoſed betwixt it and the body to be raiſed, is a thing ſo unlikely, that though the ancients knew and much admired the attractive virtue of the loadſtone, yet they ſeemed not to have ſuſpected it enough to vouchſafe it a trial: and yet ſince *Gilbert*'s writings came abroad, he muſt be a great novice in magnetical affairs, that either ignores or doubts it. But I muſt not do any more than touch upon magnetical experiments, ſince they alone would afford me ſo many truths (which the generality of men would not have thought likely enough to be worth trying) that to enumerate them, though it might convince your underſtanding, would, I fear, exerciſe your patience.

THAT it is the property of unſlaked lime to grow hot by antiperiſtaſis, upon the pouring on of cold water, and other cold liquors, and conſequently not to grow hot upon the effuſion of liquors, that are not cold, is not only generally believed, both by learned and unlearned; but this property of lime has been employed as an argument to prove other matters, as well by divers of the new philoſophers, as by many of them, that embrace the old Ariſtotelian principles: whereas I doubt not but a little trial might eaſily diſabuſe them: for by the affuſion of divers liquors actually warm, I have made lime ſlake with its wonted violence, if not with a greater. And in other liquors actually cold, like unheated water, and one or two of them far more thin or ſubtle than it, I have kept lime long without ſlaking, and without imparting to the ambient liquor any ſenſible heat. The quality of theſe inſtances makes me think it needleſs to increaſe their number, ſince we can ſcarce wiſh a greater inducement to expect, that many new attributes may be diſcovered in the works of nature, if men's curioſity were duely ſet on work to make trials, than that divers have been found out, that ſeemed ſo unlikely, that men thought it would be in vain to try them.

To theſe ſeveral ſorts of inſtances, that have hitherto been reduced to our firſt conſideration, might well be added, that bodies, which have the ſame denomination, and from whence men are therefore wont to expect the ſame, and but the ſame, operations and uſes, may yet have peculiar ones, and ſome of them very differing from thoſe of the generality of other bodies, that bear the ſame name. But examples of this kind will more conveniently be mentioned in the laſt eſſay: and leſt this ſhould ſwell too much, diſmiſſing this preſent conſideration, we will advance to the next.

SECTION II.

I Conſider in the ſecond place, that the faculties and qualities of things being (for the moſt part) but certain relations, either to one another, as between a lock and a key; or to men, as the qualities of external things referred to our bodies, and eſpecially to the organs of ſenſe, when other things, whereto theſe may be related, are better known, many of theſe, with which we are now more acquanted, may appear to have uſeful qualities not yet taken notice of.

I ſhall elſewhere, *Pyrophilus*, have occaſion to ſhew you more fully on what grounds, as well as in what ſenſe it is, that I take the moſt of the qualities of natural bodies to be but relative things. To our preſent purpoſe it may ſuffice to adumbrate my meaning by the newly hinted example of a lock and a key, where, as that, which we conſider in a key, as the power or faculty of opening or ſhutting, ſuppoſes, and depends upon the lock, whereto it correſponds; ſo moſt of thoſe powers and other attributes, that we call qualities in bodies, depend ſo much upon the ſtructure or conſtitutions of other bodies, that are diſpoſed or indiſpoſed to be acted on by them, that if there were no ſuch objects in the world, thoſe qualities in the bodies, that are ſaid to be endowed with them, would be but aptitudes to work ſuch effects, in caſe convenient objects were not wanting. As if there were no lock in the world, a key would be but a piece of iron of ſuch a determinate ſize and ſhape. And this compariſon I the rather imploy, becauſe it may be further applied to our preſent diſcourſe. For as if ſome barbarous American ſhould, among other pieces of ſhipwreck, thrown by the ſea upon the ſhore, light upon a key of a cabinet, he would probably look on it as a piece of iron, fit only for the inconſiderable uſes of any other piece of iron made much broader at each end than in the middle; but, having never ſeen a lock, would never dream, that this piece of iron had a faculty to ſecure, or give acceſs to all, that is contained in ſome well furniſhed cheſt or rich cabinet: ſo there is many a thing, that ſeems to us uſeleſs, whilſt we look upon it only in itſelf, which will perhaps hereafter prove highly uſeful, when we ſhall light upon ſome other bodies peculiarly fitted to act upon it, or receive impreſſions from it. But this will be better apprehended by the following inſtances.

Though iron be ſo common a body as it is, and its uſes are very many, and have been known as long as ſince *Adam*'s time, yet all thoſe differing bodies, on which men of all ſorts imployed it to work, and all thoſe various ways, whereby chemiſts, phyſicians, and mineraliſts have wrought on it, during ſome thouſands of years, did never diſcover to man one of its nobleſt and uſefulleſt properties, which, for aught we know, was never found out till within theſe three or four ages: for a ſteel needle, being applied to a loadſtone, manifeſted itſelf to be capable of conſtantly ſhewing the north and ſouth in all ſeas, in all weathers, and in all times of the day and night to navigators, who, by this property, which depends upon the relation that iron has to one only ſtone, have been enabled to diſcover the new world, and enrich the old with the drugs and treaſures of it.

After all the vain attempts, that even ſubtle chemiſts have made to arreſt the fluidity of quickſilver, the knowingeſt perſons, that have medled with that mineral, and eſpecially if they have obſerved, that the keeneſt froſts, that are capable of freezing even aqua vitæ, are unable to congeal it, have been very much indiſpoſed to reckon an eaſy coagulableneſs amongſt its qualities; and yet we ſee, that though the mixture of no other known body will diſcloſe its having any ſuch affection, yet the vapour of melted lead will ſometimes (for that experiment will not always ſucceed) reduce quickſilver, even in its maſs, into a conſiſtent and ſomewhat tough and hard body.

Vinegar being a liquor, that has been generally known and uſed for ſome thouſands of years, men have imployed it upon great variety of bodies, and to very many uſes, but eſpecially to communicate a ſowerneſs to the things, wherewith it was mingled; but when it came (probably by chance) to be applied to the diſſolving of lead calcined or crude, it manifeſted, that it had a faculty to exhibit a more than ſaccharine ſweetneſs, which, for aught I know, it exhibits with that metal only; for I have not yet known crude vinegar diſſolve tin, though calcined: and though by a ſlight artifice, elſewhere mentioned, we have been able to make ſtrong vinegar diſſolve the calx of Jupiter, yet was the ſolution far differing from, and inferior to, the taſte of the ſolution of lead newly mentioned.

Spirit of urine is a liquor, that has been long known to chemiſts, and might reaſonably be looked upon as likely to be a good menſtruum for ſeveral bodies: but it is not probable, that after it had been imployed to diſſolve divers compact bodies, it ſhould be ſuſpected, that it would coagulate ſo thin, light, and fugitive a body, as ſpirit of wine itſelf; and yet we have often (as there will be hereafter divers occaſions to relate) tried, that if both liquors be ſufficiently pure and dephlegmed, they will afford that ſtrange ſnow-white concretion, that *Helmont* calls his *Offa alba*; which, however by his followers aſcribed to him as the inventor, I find mentioned in ancienter books than his: and I remember, that even *Raymond Lully* relates, with what wonder he firſt ſaw this experiment (which indeed is conſiderable) performed.

And as the ſpirit of urine has ſuch an odd property, when it meets with ardent ſpirits dephlegmed; ſo the ſpirituous parts of urine, without being ſeparated from the reſt, have a faculty, that one may yet leſs expect, if they be duly imployed, to operate upon muſk: as I have had the opportunity to inform myſelf by inquiry of a ſcholar, who lived in *China*, and affirmed himſelf to have divers times ſeen muſk made. For this perſon anſwered me, that he had obſerved it to be the practice of others, and had made trial of it himſelf in thoſe eaſtern parts, that the muſk being made up, and put into cods or bags made of the ſkin of the ſame animal, (in which form I have received preſents of muſk ſent me from the *Indies*) they do, either before or after, hang it in a houſe of office, ſo as it may, without touching the groſſer bodies, receive the fœtid exhalations of that naſty place; by which urinous ſteams, which it is expoſed to for ſome days, the leſs active, or more immerſed ſcent is, as it were, called out, and excited or heightened. And I found, by farther enquiry of the ſame perſon, that having carried muſk from thoſe eaſtern regions, where it is made, to other and remote parts of the ſame *Indies*, he found, that, by the length of the voyage by ſea, his muſk had very much loſt its ſtrength, which he afterwards reſtored to it, by following the advice of ſome ſkilful perſons, according to which he tied the muſk cloſe in a bladder, wherein, having pricked many little holes with a needle, he hung it up for ſome days in ſuch a ſtinking place as has been newly mentioned. Whereto agrees very well what I have read in a late eminent phyſician of *Rome*, (where the art of perfuming is very much cultivated) who communicates it as the chief ſecret practiced by the perfumer's there, for recovering the ſcent of decayed muſk, that it be kept for a competent time in linnen well moiſtened with rank urine.

The uſes of geſſo (as the Spaniards and Italians call it,) or gypſum, are numerous enough in the ſhops of ſtone-cutters, moulders in plaiſter or wax, and divers other artificers; but one would ſcarce ſuſpect, that, beſides the various uſes theſe tradeſmen put it to, it ſhould have one ſo very differing from them, as to be an excellent medicine, if I may ſo call it, for wine: and yet, that they uſe great ſtore of it about thoſe choice ones, that come to us from the Canaries, is a noted tradition among thoſe, that deal in that ſort of liquor, and has been confirmed to me by an eminent wine-merchant, that lived ſeveral years in thoſe iſlands. And, that about *Malaga* they put up a good proportion of it into the juice of their grapes, when they tun it up, is affirmed to me by a curious eye-witneſs, who was there in vintage time, and of whom I purpoſely enquired about it.

Though ſilver be ſo noble a metal, and ſo much known and uſed, that it was the price of things as early as *Abraham*'s time, yet one very fine uſe of it has been known but ſince the art of annealing upon glaſs came to be practiſed. For among other experiments of this art we find, that prepared ſilver (and I have ſometimes done it pretty well with the crude metal) being as it were burned upon a plate of glaſs, will tinge it with a fine yellow or golden colour: there are alſo divers mineral earths, and other coarſe foſſils, of uſe in this art, which, by the help of the fire, makes them impart colours to glaſs, both tranſparent, and ſometimes very differing from thoſe of the bodies themſelves, as I may elſewhere have occaſion to ſpecify. In the mean time, give me leave to name this reflection upon the art of painting, that it is very hard for us to be ſure, that we know ſo much, as all the ſeveral ſorts of uſes, that may be made of the particular bodies we converſe with, ſince upon the invention of a new art or trade, of which we know not how many remain yet to be found out, divers uſes and applications of bodies come be diſcloſed, that were never ſuſpected before.

The uſe of lyes made with common aſhes to waſh linnen has rendered them for theſe many ages very familiar: but though their effects on the other bodies, upon which they have been employed, ſeemed not to have any affinity with what I am going to mention; yet when a ſtrong lixivium is applied to ſyrup of violets, (which is alſo a very known liquor) to which it has a peculiar relation, it will then immediately change the colour of that ſyrup from a blue to a perfect green, and ſo it will the violet leaves cruſhed on a piece of white paper, without the help of ſugar, or any preparation.

Redneſs, though a colour as obvious as moſt others, and to the generality of men very pleaſing, however it hath no offenſive property, in reference to other animals, familiarly known amongſt us, (at leaſt, that we have taken notice of;) yet being preſented to the eyes of turkey cocks, it has ſuch an incongruity with them, that oftentimes it is obſerved to make them very angry, as far as can be judged by the tokens of being diſpleaſed it produces in them.

The leaves of oaks, that are ſuch common things, and are not obſerved to have, in reference to any other body, which chance or induſtry applies them to, any ſuch property as that I am about to name; theſe leaves, I ſay, if when freſh, they be immerſed in the water of mineral ſprings, impregnated with the ſubtle corpuſcles of iron, I have ſeveral times found to turn the liquor blue or black, according to the proportion and vigor of the two ingredients.

One would not expect, that ſo dark and black a body as charcoal ſhould be the main thing employed, not only to cleanſe and brighten ſome metals, but to procure a clearneſs, and give a gloſs to ſome tranſparent bodies. And yet I learned from the makers of mathematical inſtruments, gravers, and other artificers, that the beſt way they have, and which I have ſeen them employ, to poliſh their plates of braſs and copper, (after they have been rubbed clean with powdered pumice-ſtone) is with charcoal, (which ſome of the more curious burn a ſecond time, and quench in appropriated liquors,) as that, which both ſerves to fetch out the ſcratches of the pumice-ſtone, and itſelf ſcours without ſcratching, and thereby poliſhes very ſmoothly. And by the ſame way they may cleanſe and poliſh the plates of horn, of which they make lanthorns, drinking-cups, &c. To which, as to the metalline plates, a gloſs may be afterwards given with tripoli.

Perhaps it will not be improper to take notice to you, *Pyrophilus*, in this place, that not only the nature of the body to be wrought upon, but ſome peculiar circumſtances relating to it, may contribute to the effects of ſuch experiments, as thoſe treated of in this ſection. As for example, one would not expect, that water, which is ſo apt to run out at the chinks of wooden veſſels, ſhould, without addition, become the fitteſt inſtrument for cloſing them. And yet I have more than once found by trial, as I preſume many tradeſmen have done, that when wooden barrels or firkins, and the like veſſels, by having been long kept too dry, come to have clefts and commiſſures, this inconvenience may be remedied by pouring water into them. For though at firſt the liquor quickly runs out again, yet by frequent affuſions of it, the wood, eſpecially thoſe edges between which the water runs out, becomes ſo ſoftened and plumped up, that the little intervals or chinks are, by the ſwelling of the neighbouring parts, cloſed up, and the veſſel becomes ſtanch.

And upon a like reaſon ſeems to depend that odd experiment, much talked of by ſome of our eminent Engliſh ſeamen, who, for the haſty ſtopping of a leak, that is not too great, much commend the thruſting into it a piece of powdered beef; for this being much more ſalt than the ſea-water, that liquor pierces into the compact and (in great part) dry body, and by opening the ſalts, and ſoaking into the fleſh, makes the ſwelling beef expand it ſelf, ſo as to bear ſtronly againſt the edges of the broken planks, and thereby hinders the water from flowing into the ſhip as it did before.

SECTION III.

I conſider in the next place, that a body in aſſociation with others may be made fit for new uſes, and ſome of them quite differing from thoſe, that were proper to it before.

This third conſideration is, in ſome regards, of affinity with the firſt, but yet is not the ſame, ſince in the former we conſider the power, that one body has to act upon another, or the diſpoſition it hath to be acted upon by it; whereas now we conſider the two bodies or more, as being by conjunction qualified to act on a third body, or ſuffer from it, as one entire concrete, upon the account of new and emergent properties, accruing to the compound by the aſſociation of the more ſimple bodies, that compoſe it.

You will meet with ſtore of inſtances, both in theſe eſſays, and other of my writings, eaſily applicable to the illuſtration of what is here delivered, and therefore it will ſuffice to name in this place the fewer.

He that takes notice, how flexible a metal tin is, and how dead a noiſe it yields, will ſcarce dream, that one of its uſes, and that none of the deſpicableſt, ſhould be, to make another metal, which is leſs yielding, and has a leſs dead ſound than its ſelf, not only hard, but ſonorous: and yet we ſee, that bell-metal, which, when caſt into bells, makes a hard mixture, that ſounds ſo loudly, is made principally, as has been already noted, by the addition of a certain proportion of tin to copper.

In the common experiment of making ink, the infuſion or decoction of galls is yellowiſh, or reddiſh, and the ſolution of vitriol will, as the concrete participates more of iron or of copper, be either green or blueiſh; but from the mixture of theſe two liquors there will emerge an inky blackneſs.

That oil, that is a body ſo mollifying and ſlippery, and whoſe unctuoſity makes its moiſture ſo much more difficult to be waſted or deſtroyed, than that of water, wine, or other not tenacious liquors, ſhould be one of the two or three main ingredients, and the only moiſt one of a hard and durable cement, is that, which probably you would very little expect from it: and yet, not to mention what trials of that nature I have made, becauſe I had not time to obſerve the full event, a very ingenious man, much employed about coſtly water-works and dams, aſſures me, that the beſt way he has to join together, and, if need be, piece and mend with a cloſe and laſting cement the pipes, that are uſed for ſubterraneal aquaducts, that are long to hold running water, is to take good clay (ſuch as tobacco-pipes are made of,) and having dried it, and reduced it to very fine powder, and mixed good ſtore of ſhort flocks with it, beat it up very diligently with as much linſeed-oil, as will ſerve to bring it to a ſtiff paſte, almoſt like well kneaded dough. This paſte he faſhions into pipes of the length and bigneſs required, which, though they will be long a drying in the air, yet, when once throughly dry, are very ſtanch and laſting. And I remember, that before I learned this, having occaſion to try divers experiments about cements, I chanced to meet with an ancient artificer, employed to keep in repair the conduits, that brought water to *London*, and in exchange of a lute or cement, that I taught him, he was forward to ſatisfy the curioſity I had to know what cement he employed about ſo important a work, and he aſſured me, that oil was one of the main ingredients (and the only liquid one) he employed.

He that conſiders, that lead is one of the moſt opacous and flexible bodies, that the world affords, will not eaſily imagine, that one of its uſes ſhould be to make up about three parts of four of a mixture tranſparent, and exceeding brittle; and yet this is eaſily performed by divers chemiſts (and I elſewhere mention my having often done it) in making of calcined lead, and powdered flints or ſand, a brittle and diaphanous compoſition, called by Spagyriſts Vitrum Saturni.

And this mention of glaſs ſuggeſts to me another inſtance, fit for my preſent purpoſe: for who would imagine, that ſuch a body as the fixed ſalt of kaly, which, as other alkalies, that take their denomination from it, has a ſtrong and fiery taſte, and is not only readily diſſoluble in water, wine, or any ſuch liquor, but will in a ſhort time, being but left in the air, be reduced into a liquor; who would expect, I ſay, that it ſhould be of any uſe, much leſs the main of this cauſtick, and eaſily diſſoluble body, to be one of the two main ingredients of ſubſtance both perfectly inſipid, and indiſſoluble, not only in water, wine, &c. but even in aqua fortis, aqua regia, ſpirit of wine, quick-ſilver, ſpirit of urine, and other menſtruums, ſome of them highly corroſive, and others extremely ſubtle and piercing? and yet ſuch a mixture is uſually afforded us in glaſs, (eſpecially the more durable ſort of it) wherein that there is actually a great proportion of alkalizate ſalt, I confeſs, I doubted, till having purpoſely enquired of an ingenious maſter of a glaſs-houſe, how much glaſs he uſually obtained, when he put in ſuch a quantity of ſand, I found by his anſwer, that the glaſs obtained was many pounds in the hundred more than the ſand, that was employed to make it: whence I gathered, (what he alſo affirmed) that the alkaly did not only ſeem (as one might ſuſpect) to promote the fuſion of the ſand, but does materially and plentifully concur with it to compoſe the glaſs.

And whereas I intimated at the very beginning of this third ſection of this eſſay, that bodies, when aſſociated, may be applied, not only to new uſes, but perhaps to ſome, that are quite differing from thoſe, that belong to ſome of the reſpective ingredients; this obſervation may be made good by ſeveral inſtances, and even by ſome that are very obvious, as well as by others that are not ſo familiar. For we may take notice, that though oil, and tallow, and other ſuch unctuous bodies, be thoſe, that do greaſe and ſpot linnen and woollen clothes; yet thoſe very bodies, being ſkilfully aſſociated with others, though with but a lixiviate ſalt and fair water, do plentifully concur to the making up of ſoap, by the ſolution of which greaſe is readily waſhed out of linnen cloths, and others, beſides thoſe, are alſo freed from the ſpots of it. But divers other inſtances applicable to this purpoſe belonging more properly to the following part of this eſſay, till we come thither, it may ſuffice, that I illuſtrate and confirm what hath been propoſed by the ſingle, but noble inſtance of Aurum fulminans. For though ſalt of tartar be a fixed body, and of a fixing quality, yet being ſkilfully aſſociated with gold, diſſolved in aqua-regis, though that be thought the fixedeſt, not only of metals, but of bodies; yet the gold precipitated by this fixed and incombuſtible ſalt becomes ſo exceeding fugitive, that by a gentler heat, than would kindle any known body in the world, it is made to fulminate like gun-powder, (but many degrees more violent than it;) and, which you will alſo think ſtrange, though ſulphur be a body of ſo quick accenſion as is obviouſly known, yet by an eaſy way, elſewhere to be taught you, of mixing thoſe two only, you may, as trial hath informed us, make it (which you will eaſily allow to be one of the unlikelieſt uſes of ſulphur) even by its being ſet on fire, to hinder the accenſion of this ſo eaſily kindled gold, which I have known thereby readily turned into a medicine, that ſome cry up for excellently diaphoretick, (though I doubt whether Aurum fulminans work not rather another way,) and which I remember I have, in a crucible, kept long in the fire without loſs.

I ſhall only add to this third conſideration this one particular, that is of too great moment to be pretermitted here, though it have been already in part taken notice of on another occaſion; namely, that the effects and uſes of mixtures do not only depend upon the nature of the ingredients, but may be oftentimes much varied by their proportion. And of this the mineral, which at the glaſs-houſes they are well acquainted with, under the name of manganez, will afford us a pertinent and conſiderable inſtance. For though it be a coarſe and dark mineral it ſelf, and though being added to the materials of glaſs in a fuller proportion, it make the black glaſſes, that are ſold in ſhops; yet not only a moderate proportion of it is uſed to make glaſs red, but, which is more remarkable, a ſmall and due proportion of it is commonly employed to make glaſs the more cleer and diaphanous.

SECTION IV.

IN the fourth place I conſider, that a body, by a differing preparation or management, may be fit for new, and perhaps unthought of, purpoſes. For the qualities of bodies depending for the moſt part upon the texture of the ſmall parts they are made up of, thoſe ways of ordering greater bodies, which do either by addition, detraction, or tranſpoſition of their component corpuſcles, or by any two, or all of thoſe ways, make any notable change of the former texture of the body, may introduce new qualities, and thereby make it fit for divers uſes, for which it was not proper before.

WE ſee to how many ſeveral uſes men, that were neither philoſophers nor chemiſts, but for the moſt part illiterate tradeſmen, have been able to put iron, by but varying the viſible ſhape of certain portions of it, and connecting ſome of them after a peculiar manner: as is obvious in the ſhops of blackſmiths, lock-ſmiths, gun-ſmiths, cutlers, clock-makers, iron-mongers, and others. But to give you a more phyſical inſtance in the ſame metal, be pleaſed to take notice, how much a change, made by a natural agent, the fire, in the inviſible texture of iron, does ſpeedily alter it; when of the ſame bar of iron, by the help of fire and water, the artificer makes hardened iron, and iron of a temper fit for drils, and knives, and ſprings, and I know not how many other inſtruments, which require diſtinct tempers in the metal they are made of; that temper, which renders them fit for one uſe, leaving them unfit for another.

BUT we need not confine our ſelves to inſtances, wherein no new ingredient is added to, or taken from the body to be altered; it being ſufficient, that the additament upon its own account do not bear ſo great a ſtroke in the change produced, but that it be principally aſcribed to the way of ordering the body wrought upon; and ſpeaking of the management of a body in this ſenſe, (which is uſual and proper enough,) I ſhall ſubjoin a few inſtances, of the many I might add, to make good our propoſition.

THOUGH paper be one of the commoneſt bodies, that we uſe, yet there are very few, that imagine it is fit to be employed otherways than about writing, or printing, or wrapping up of other things, or about ſome ſuch obvious piece of ſervice, without dreaming, that frames for pictures, and divers fine pieces of emboſſed work, with other curious moveables, may, as trial has informed us, be made of it, after this or the like manner. Firſt, ſoak a convenient quantity of whitiſh paper, that is not fine, about two or three days in water, till it be very ſoft; then maſh it in hot water, and beat or work it in large mortars or troughs, (much after the manner uſed in ſome places to churn butter) till it be brought to a kind of thin pap, which muſt be laid on a ſieve, without preſſure, to drain away the ſuperfluous moiſtneſs, and afterwards put into warm water, wherein a good quantity of fiſh, glew, or common ſize, has been diſſolved. Being thence taken out by parcels with a ſpunge, it muſt therewith (for the ſpunge will dry up the ſuperfluous moiſture) be preſſed into moulds of iron, or of ſuch plaiſter as ſtatuaries uſe, wherein having acquired the figure, which is intended to be given it, it is thence to be taken out, and permitted to dry, and is to be ſtrengthened, where need requires, with plaiſter, or grated chalk, made into pap with water, or ſome other convenient matter; and afterwards, having firſt been leiſurely dried, it is to be either painted or overlaid with foliated ſilver or gold, as the artiſt pleaſes. I may elſewhere have occaſion to mention another unlikely uſe of paper, namely, to ſtop the clefts and commiſſures of wooden inſtruments and veſſels, that are to hold water. For paper being thruſt into theſe narrow places, the firſt water, that comes to it, being ſoaked up, occaſions a forcible dilatation, which makes the ſwelling paper fill the chinks it is lodged in, according to what was lately delivered at the cloſe of the ſecond ſection.

THE ſugar-cane has been a plant well enough known to many countries and ages, who were not unacquainted with the ſweetneſs of its juice, and yet ſeem never to have made ſugar of it, for want of knowing the way of ſo ordering it, as to coagulate it into a durable, as well as delicious ſubſtance.

TOBACCO was likewiſe a noted plant in the *Weſt-Indies*, which was yet ſuffered yearly to rot and periſh like other herbs, till the induſtry of the moderns finding the way of curing it, as they call the method of ordering it, made it, by the help of mere ſkill, laſt in an improved condition for divers years, and fit to be tranſported, as it plentifully is, over all the world.

THE leaves likewiſe of indigo, which would uſeleſly periſh like thoſe of other ſhrubs, by the mere way of ordering them, which too is rather by ſubſtraction than addition, have been long made a laſting pigment or dying ſtuff, and one of the moſt ſtaple merchandiſes, that even the *Eaſt-Indies* ſend us.

I might add the great uſe, that we are enabled to make of madder, woad, and divers other periſhable plants, by the way of ordering them; but there is one inſtance of this kind ſo conſiderable, that though I have formerly named it to another purpoſe, and though I am willing to mention but one example more of this ſort, I cannot but pitch upon this; ſince it excellently manifeſts, what may be expected from a ſkilful ordering of nature's productions, by ſhewing us, what even the ſavages of *America* have been able to perform in this kind. For though their mandioca be confeſſedly a poiſonous plant, yet without addition they make of it their caſſavi-meal, whereof not only the Indians, but alſo many Europeans make their bread, which I alſo have made ſome uſe of without diſlike. And with no addition, unleſs it be perhaps that of ſpittle, they make of the poiſonous juice of the ſame root a not unpleaſant nor ſtrengthleſs drink, which divers, even of the Engliſh, compare with our beer. And of the bread made of that caſſavi-root, they brew, in ſome of our American colonies, a liquor by the planters called Perino, which I have known, even by perſons of quality, equalled, if not preferred to wine itſelf.

THE ſhreds of leather pared away and thrown aſide by the glovers, by ſo ſlight a way of ordering them, as only the boiling them long in fair water, diſſolves them in that liquor, and reduces them with it, the decoction being ſtrained and cooled, into a kind of jelly, that they call ſize, (which may be alſo made the ſame way of cuttings of parchment, and better yet with thoſe of vellom) which is of great uſe towards the production of very differing trades: ſome of which productions are already touched upon in this book, to which I ſhall here only add, for the eaſineſs of the experiment, that the fine red ſtands, and hanging-ſhelves, are made with ground vermilion, being only tempered up with it, and laid upon wood, which being thus coloured, is, when it is dry, laid over with common varniſh, which preſerves it from wet, and gives it a gloſs.

IT would ſcarce be ſuſpected, that ſo white a body as ivory ſhould, among other uſes, be proper, without the addition of any black, or ſo much as dark coloured body, to yeild one of the deepeſt blacks that has been hitherto known; and yet many of our eminent painters count that black, which they call ivory-black, the perfecteſt, that hath been hitherto employed in their art. And this ſable may be made of ivory, without addition, only by burning it a-while in a cloſe pot; and we have made it by keeping it a-while among coals and aſhes, only wrapped in ſtore of wet paper to keep it from ſpending its denigrating ſulphur in an actual flame; (to prevent which, the pots, it is burnt in, are wont to be cloſed with lute, or otherwiſe ſufficiently ſtopped) as if artificers were acquainted with the old rule, *aduſta nigra, peruſta alba*.

AND on this occaſion I ſhall add, that this black made of ivory is ſo excellent in its kind, that I ſcarce know any thing ſo proper to make foils of, for that nobleſt ſort of gems, diamonds. And I remember, that a very ſkilful jeweller, of whom I bought ſome of thoſe ſtones, and whom I employed to ſet others for me, confeſſed to me, that burnt ivory was the thing he made uſe of, for foils to the diamonds he had a mind to ſet well.

ANOTHER inſtance there is, which I muſt by no means pretermit, now that I am endeavouring to ſhew, what the preparation or management of a body, even by illiterate tradeſmen, may do to make it fit for unlikely uſes. For one would ſcarce imagine, that from ſo groſs and foul a body, as the *inteſtinum rectum* of an ox or cow, there ſhould be obtained a tranſparent ſubſtance, more thin by far than paper; and yet of ſo great a firmneſs and toughneſs, as is ſcarce at all credible to thoſe, that have not been, as I have, convinced of it by experience. But it is certain, that ſome of our gold-beaters in *London*, and perhaps not there only, do, by cleanſing and otherwiſe preparing the above-mentioned naſty gut of an ox, obtain exceeding fine membranes,

branes, ſome of which I keep by me, that though clear and ſtrangely thin, are yet of ſuch tenacity, that when the thin plates of gold are put between them, or in their folds, the force of a man frequently ſtriking them, with a vaſt hammer made of purpoſe, almoſt as heavy as he can well lift up, does uſually, as I have ſeen with ſome wonder, attenuate and dilate the included gold, without being able to break theſe ſo fine ſkins.

THESE inſtances, *Pyrophilus*, we have hitherto produced, are almoſt all of them ſuch, as either nature herſelf, or nature aſſiſted but by tradeſmen, and other illiterate perſons, has preſented us. And therefore queſtionleſs, the power, that a ſkilful management may have to produce great changes in bodies, and thereby fit them for new uſes, will be much advanced, when they ſhall be ordered by ſuch, as are either good chemiſts, or dexterous at mechanical and mathematico-mechanical contrivances, eſpecially, if in the ſame perſons a ſkill in theſe two ſorts of knowledge ſhould concur.

THAT ſkill in mathematicks may teach a man ſo to manage natural things, as to enable him to make other uſes of them, than thoſe, that want it, will dream of, we may be perſuaded by ſeveral particulars. For we ſee, that from a bare giving to a piece of ordinary glaſs a priſmatical ſhape, that diaphanous and colourleſs body may be made to exhibit in a moment all thoſe delightful and vivid colours, for which we admire the rainbow; and though merely by giving a piece of foliated glaſs or metalline ſpeculum a concave figure, it may be made to burn ſtrongly by reflection, yet by giving a piece of glaſs a convex figure, you may qualify it to burn by refraction, and even with water fitly figured, you may readily kindle fire. For though a round and hollow ſpherical vial of pure glaſs will tranſmit the ſunbeams without making them burn, and conſequently has not of itſelf the faculty I am going to name, but ſerves chiefly to terminate the water, that is to be poured into it, and give it its due figuration; yet by filling a ſpherical vial, I have taken pleaſure ſo to unite the ſunbeams, as, when froſt and ſnow was about me, to make them burn; (and perhaps ice itſelf, if choſen free from bubbles, and conveniently ſhaped, may, as ſome incompleat trials make me hope, be made fit enough for that purpoſe.) And much more vigorous the accenſion would be, if two bare concave glaſſes of like ſhape, equal bigneſs, and truly ground, had their edges ſo joined by a cloſe frame, that the cavity contained between the inſide of the glaſſes and the frame may be filled with fair water; for by this means (the convex-ſide of each glaſs being outermoſt) the whole inſtrument (one or two of which I have ſeen in a virtuoſo's hands) will ſerve for a double convex-glaſs, which may by this means be made far larger, and more efficacious, than other burning-glaſſes of that figure, which conſiſting each of them of a ſingle piece of ſolid glaſs, are wont to be far inferior in bigneſs to ſuch hollow ones, as may be eaſily enough attained.

AND now I have named ſolid glaſs, give me leave to take hence a riſe to add, that though glaſs ſtopples are made only by giving them an almoſt conical figure, and a ſuperficies fitted by grinding, for an exquiſite contact with the inſide of the neck of a glaſs-bottle; yet this way of ordering glaſſes, which is aſcribed not to mere philoſophers, but men verſed in optical and mechanical trades, produces ſtopples much ſurpaſſing all known before; not only in this, that neither aqua fortis, nor other corroſive liquors, work upon them, but alſo in their being able to keep in even the ſubtileſt ſpirits ſo ſtrictly, that I remember having once forgot ſome ſpirit of ſal armoniack in a large bottle, which it did not near a quarter fill, when I long after (as I remember about ſeven years) came to that part of *England* again, I chanced to find this bottle in a place, where, being without an inſcription, I knew not what the contained liquor was. And taking off the glaſs-ſtopple, to diſcover by the ſcent what it might be, upon ſmelling to that ſolid body, the adherent ſpirits operated ſtrongly enough upon my noſe and eyes to make me almoſt ſtagger, and wiſh my curioſity had been more cautious.

WHAT I have further obſerved about the way of making, and the applications of this kind of glaſſes, belongs not to this place, where it would be fit to proſecute my former diſcourſe, by ſhewing you, how much the chemical management of things may alter and improve them; were it not, that it would be improper to venture upon ſo copious a ſubject in one of the ſections of an eſſay, where I ſhall therefore but point at it, without pretending to treat of it.

WE ſee, that chemiſts can out of ſome fruits, that grow wild in the hedges, and are not edible, as alſo out of the lees of ale and beer, draw an inflammable ſpirit, which, for many purpoſes (not medicinal) may be made uſe of for that of wine. We ſee, that out of the dry body of hartſhorn, as likewiſe out of the ſkull and bones of dead men, and other animals, which have been wont to be looked upon to be ſo devoid of moiſture, that men proverbially ſay, as dry as a bone, chemiſts do ordinarily, to the wonder of the ignorant, draw ſtore of ſpirit, and oil, and phlegm, as they likewiſe do from the drieſt woods. Some of them alſo, of the opacous body of lead mixed with ſand, and a few grains perhaps of metalline pigment, can make in a few hours variety of amauſes, or metalline ſtones, which, by their tranſparency and lovely colours, do pleaſingly emulate rubies, emeralds, and other native gems; about the imitation of which, I may elſewhere acquaint you with ſome of my trials.

HOW unlikely effects may be ſometimes produced by a ſlight ſpagyrical preparation of things, may ſufficiently appear by the Bolonian ſtone, from which (though one would not, upon the ſight of it, expect any ſuch matter, yet) being duly prepared by chemical calcination, it acquires that ſtrange property of ſhining in the dark a-while after it has been expoſed to the ſun, for which it is ſo juſtly admired by us, that have ſeen it, that it is judged unfit to be believed by many criticks, that have not.

AND here let me take notice to you, *Pyrophilus*, that very ſlight circumſtances in the management of a body, may ſometimes produce conſiderable and unlikely effects.

THAT ſalt, diſſolved in water, is a powerful hinderer of the congelation of that liquor, is a matter of common obſervation; neither the ſea-water, nor brine, being uſually frozen with us by ſuch froſts, as turn common water, and ſome liquors more indiſpoſed than that is, into ice. And yet ſea-ſalt, which being diſſolved in water, keeps it from freezing, being outwardly applied to water, does ſo powerfully concur with ſnow or ice to make it freeze in artificial glaciations, and is ſo neceſſary to the effect, that the ſnow or ice, without the ſalt, would not ordinarily, here in our climate, produce in a ſeaſonable time any ice at all, as I more than once purpoſely tried.

THERE is a certain powder, which by the proportion and mixture of nitre (whereof it chiefly conſiſts) with other ingredients, obtains ſo odd a texture, that if putting it into a crucible, you ſhould place that upon the coals, as is uſually done in other fluxes, the powder would blow up, or take fire with violence enough, and perhaps not without ſome danger; and yet, if inſtead of kindling this power from the bottom upwards, you kindle it from the top downwards, there will be no danger in it, but it will make a powerful flux for the reduction of metalline powders mixed with it into a body.

SECTION V.

IN the fifth and laſt place I conſider, that the generality of effects to be performed, being not produced by one ſingle and unaſſiſted production, either of nature or of art, but requiring the concurrence of more; he, that knows not the nature or properties of all the other bodies, wherewith that, on which the experiment propoſed is actually, or may be uſefully aſſociated, or otherwiſe imployed, can hardly diſcern all the effects the experiment may poſſibly concur to produce. For, whereas many inventions or operations conſiſt, as it were, of ſeveral parts, and require, as it were, diſtinct actions; a body, that ſeems uſeleſs to the main and ultimate effect, may uſefully concur to the performance of ſome intermediate or ſubordinate part of the operation, (by being requiſite to which, it may be of uſe to the experiment conſidered in the groſs, though not to each diſtinct part of it.)

THOUGH ſpirit of wine will ſcarce (if at all) even in a very long time draw a red tincture out of the flowers of ſulphur, yet, when they have been opened, by having been fluxed together with an equal weight of ſalt of tartar, we have found, that they will in a few minutes, and in a gentle heat, give, in thoroughly dephlegmed ſpirit of wine, a tincture or ſolution as red as blood; which being freed from the ſuperfluous menſtruum, will afford us a balſam much finer than that vulgar one, which is wont to be made of the ſame flowers diſſolved in oil of turpentine.

THAT ſuch amalgams of gold and mercury, as goldſmiths are wont to gild ſilver with, cannot by ordinary ways be made to adhere either to iron or ſteel, is a thing ſo well known among gunſmiths, and ſuch artificers, as work upon iron, that when I inquired of ſeveral of them (as well Dutch as Engliſh) whether they could gild iron with water-gold, (as they call that way of gilding by the help of quickſilver,) they judged it a thing not to be done: and yet I know a very ingenious tradeſman, who was able to perform it, but not (that we may apply this experiment to our preſent purpoſe) without the the aſſiſtance of another body, which was to perform one part before the amalgam could perform the other. The artificer's way was to coat (if I may ſo ſpeak) the iron or ſteel to be gilt, with a coat of copper, to which purpoſe he uſed diſtilled liquors tempered with other ingredients, wherein the iron was to be immerſed with great warineſs and dexterity; for otherwiſe, not only the trial would not ſucceed, but oftentimes the iron would be ſpoiled. To obviate which inconveniencies, there occurred another way of caſing the iron with copper, namely, by diſſolving very good vitriol, that has copper in it, (for it is not every vitriol, that is fit for the purpoſe) in warm water, till the liquor be ſatiated with vitriol, and immerſing ſeveral times into this ſolution the iron, firſt ſcoured till it be bright, and ſuffering it each time to dry of itſelf; for this immerſion being repeated often enough, there will precipitate upon the iron enough of the cupreous parts of the diſſolved vitriol, to fill all its ſuperficial pores with particles of copper. So that by this ſafe, cheap, and eaſy way, having, as it were, overlaid your iron with copper, you may afterwards gild it as copper with the above-mentioned amalgam, which will adhere to copper, not to iron.

BUT here we muſt not omit an obſervation very conſiderable to our preſent ſcope, namely, that though the ſeveral parts of an experiment or a proceſs may in moſt caſes, each of them be purely phyſical, or chemical, &c. yet in divers other caſes, it may far more uſefully be ſo ordered, that one part of it may be phyſical, (taking here that term in contradiſtinction to ſubordinate parts of learning) and ſeveral, or each of the reſt may belong to other arts, as one may be chemical, and another ſtatical, another mechanical, another hydroſtatical, &c. and by ſuch a concurrence of differing parts of knowledge to the ſame operation or production, I doubt not but many things may be performed, that have not yet been attempted, nor ſo much as thought of. For he, that has ſkill but in one of theſe ſingle parts of learning, muſt needs have his attempts as well as his knowledge much ſtraitened, by confining himſelf to operate by ſuch means and inſtrument, as are within the compaſs of his own art; which, aſſiſted by others, may bear a good part in the performance of diverſe conſiderable things, which it is by its ſelf very inſufficient to accompliſh.

Of this we may take notice of ſome inſtances in the productions, that art and nature have preſented us with already; for not only handicraft trades, as we have formerly noted, do many of them aſſiſt each other in their operations, but even thoſe arts, that are counted ingenious, have ſometimes need or uſe both of the ſervice of the more mechanical trades, and of mutual aſſiſtance among themſelves. The maſters of captoptricks know very well what would be the properties of ſpherical, cylindrical, and other ſpecula; but to procure ſuch ſpecula, you muſt have recourſe to the chemiſt, or the founder, whoſe part it is by artificial mixtures of metals and minerals, and by mechanical contrivances, to caſt bodies, that give a more ſincere and vivid reflection, than the ſingle metals would do, and to give them withal that curious poliſh, for which the metalliſts and chemiſts are beholden to ſmiths, ſtone-cutters, watch-makers, or other handicrafts men.

Another eminent example to the ſame purpoſe may be taken from the conſideration of organs uſed in churches. For to deviſe the rules of making them well, there is firſt requiſite no ſmall ſkill in the ſpeculative part or theory of muſick: next, he, that would make the inſtrument well, muſt know how to chooſe wood proper for that purpoſe, (moſt woods being unfit for it,) how to ſeaſon it, and how to diſcern, whether it be duely ſeaſoned, and otherwiſe well conditioned. To excavate and faſhion the pipes, and other parts of the inſtrument, that are made of this wood, there is uſe of the turner's and joiner's crafts. It is often needful alſo, that the organ-maker be ſkilled in the effects of metals, and perhaps their mixtures; and the ways of caſting them, in order to the making of his pipes of a ſonorous matter, and to the giving them a due ſhape, and other deſirable qualifications. I might here borrow further inſtances from bells, lutes, harps, and other muſical inſtruments; but I haſten to examples of another kind.

He that has never ſo attentively conſidered the nature of ſalt-petre or of brimſtone apart, ſhall never be able to make the conſiderableſt uſes of either of them, till he ſkilfully aſſociate them to one another, and incorporate them into that wonderful body, called gunpowder, which will afford us an inſtance fit enough to explicate what we have been ſaying: for conſiſting of three differing ingredients, nitre, brimſtone, and charcoal, though neither of theſe be ſufficient, *in omni genere*, (as they ſpeak in the ſchools,) yet each of them is very uſeful by being ſufficient *in ſuo genere*, and really concurs to the effect produced by them all, as you may elſewhere find more particuly declared.

He muſt remain ignorant of another conſiderable uſe of ſulphur, that is unacquainted with ſome properties of common oil and calcined alabaſter. For artiſts have a way of making molds, wherein to caſt off the impreſſion of medals, and other works emboſſed on metals, which, though the effects of it ſeem ſtrange to thoſe, that know not how they are produced, they eaſily thus perform. They make about the emboſſed work, whoſe impreſſion they deſire to have, a little border or ledge of clay, to hinder the melted ſulphur to be poured on it, from running over; then they lightly (but very carefully) with a pencil or feather anoint the metalline work with oil, to hinder the ſulphur from adhering to it; then they melt good brimſtone in any convenient pot, (which they cover well to prevent its taking fire) and whilſt it is hot, they pour it gently upon the emboſſed metal, all whoſe extances will make perfect impreſſions on the lower ſurface of the thus melted brimſtone, which ought to be poured on in a conſiderable quantity, that the molds thus made may prove the ſtronger. About the edge of this mold they make a little rim or border of clay as before; and lightly anointing both all the ſurface of the mold, and the inſide of the clay with oil, (which if it be too copious, is, as we have tried, apt to prejudice the accurateneſs of the impreſſion,) they pour in by degrees to the thickneſs of about a fourth of an inch of that mixture I formerly mentioned (in the eighth eſſay,) to be made of recently calcined alabaſter, ſtirred and incorporated with ſuch a quantity of fair water, as may ſuffice to bring it to the conſiſtence of the thicker ſort of honey. And this mixture in about a quarter of an hour growing hard, and then being taken out of the mold (to which the oil hinders it from ſticking) will, if the work have been dexterouſly done, and the mixture before affuſion carefully freed from bubbles, perfectly exhibit the ſhape and dimenſions of the work emboſſed upon the metalline pattern. And by this way in a few minutes have we ſometimes caſt off a coin, a medal, and ſometimes too a whole landſcape, without any trouble, and not without ſome delight.

And here, *Pyrophilus*, let me perform what I lately intimated an intention of, by now taking notice to you in this fifth ſection of this eſſay (of what I had not long ſince occaſion to obſerve in a former part of it,) that you may oftentimes find ſuch particular bodies conducive to the main effect of an operation or experiment, by performing ſome ſubordinate part or office in it, as yet may ſeem nothing at all of kin to the ultimate effect promiſed by the perfected experiment.

That aqua fortis, that ſo greedily corrodes and devours ſilver and braſs, ſhould eminently conduce to the real ſilvering over of the latter metal by the former, is that, which few goldſmiths, or even chemiſts would judge probable. And yet this fretting liquor performs a principal part in that ingenious way of ſilvering over braſs and copper, which is more applauded than known. For firſt, aqua fortis ſerves very well to make clean ſuch emboſſed or otherwiſe uneven pieces of metal, whoſe inequality hinders us from being able to cleanſe their little cavities with Tripoli, or thoſe other powders commonly uſed to ſcour braſs: whereas if ſuch bodies be lightly waſhed over with aqua fortis, and immediately thrown into fair water, the foulneſs may be fretted off, and the work not disfigured. And this is eſteemed the beſt way of ſcouring ſuch metalline pieces of work by the beſt maker of mathematical inſtruments, that I have met with. And I the rather mention it to you, *Pyrophilus*, becauſe that though it be not always requiſite to our experiment of ſilvering, (for many pieces of brazen work may well enough be made clean after the ordinary manner) yet divers trials have aſſured us, that the ſcouring of the braſs and copper is neceſſary to the ſucceſs of this experiment; probably, becauſe any greaſe or filth remaining upon the ſurface of the metal is ſufficient to keep out thoſe little parts of diſſolved ſilver, which ought to lodge themſelves ſo thick in the pores of the metal, as to ſeem one continued ſilvered body.

The remaining part of this operation may be thus performed. The metal to be wrought upon being made very clean, you muſt diſſolve good ſilver (the finer the better) in aqua-fortis in a broad bottomed veſſel of glaſs, or at leaſt of glazed earth; and having, over a chafing-diſh of coals, or with ſome ſuch heat, evaporated away all the aqua-fortis, you muſt upon the remaining dry calx pour of water five or ſix times its quantity, or as much as will be needful perfectly to diſſolve it. This water with the like heat muſt be forced away as the former menſtruum, and the like quantity of freſh water muſt be poured on, and evaporated quite away the ſecond time, and, if need be, the third time, toward the latter end making the fire ſo ſtrong, as to leave a perfectly dry calx; which, if your ſilver has been good, will be of a good white, and will by theſe operations be competently freed from the ſtinking and fretting ſpirits of the aqua fortis. Of this calx you muſt take one part, and about as much (in quantity, not in weight) of common ſalt, and as much of cryſtals of tartar, (or at leaſt powder of good white tartar) as of either of the former ingredients; which, like this, ought to be finely beaten, and theſe three powders being exquiſitely mixed, you muſt plunge the ſcoured braſs, to be ſilvered over, into fair water; and then taking up as often as need requires, with your wet fingers, ſome of the newly mentioned mixture, you muſt rub it on well, till you find every little cavity of the metal ſufficiently ſilvered over; remembring, that if you would have it richly done, you muſt rub in more of the powder. And laſt of all, you muſt waſh well your ſilvered metal in fair water, and rub it very well and hard with a dry cloath, that it may appear ſmooth and bright. And this way of ſilvering, though it be preſently and cheaply performed without quick-ſilver, yet may be made to laſt ſome years, as experience has partly informed me, and may be eaſily renewed, when the ſilvering begins to decay or wear off.

And here, *Pyrophilus*, it will not be improper to give you this advertiſement, that we ought not to conclude, as we are very prone to do, that ſuch an uſe is not to be expected, or endeavoured to be obtained from ſuch a thing, becauſe we ſee the like uſe to be made of things, that are thought to be of a quite differing nature from that we conſider, or perhaps quite contrary to it: for in many caſes, as there are more ways than one, or even than a few, to bring to paſs a thing propoſed; ſo among the various inſtruments, that may be employed the ſame purpoſe, ſome may exceedingly differ between themſelves as to other qualities, and yet agree in that, which is requiſite and ſufficient for the performance of the thing deſigned. As though, for inſtance, roſin and ſal armoniack be differing in colour, ſmell, taſte, weight, hardneſs, &c. though the one be a vegetable concrete juice, the other an aggregate of urinous, fuliginous, and marine ſalts; the one readily diſſoluble in water, the other not diſſoluble in that liquor, but in oil; and though there be I know not how many other differences between them; yet either of them ſingle may be, and is, uſefully employed for the tinning of braſs and copper-veſſels, each of them being endowed with a fitneſs to make tin ſtick to thoſe metals, as I elſewhere more particularly declare. Thus, though water, ſand, and tin, are bodies in other reſpects very unlike, yet the two latter are found fit to make hour-glaſſes, as well as the firſt; though that alone, as is preſumed, were for many ages employed by the ancients for that purpoſe.

To the foregoing advertiſement I ſhall annex another, that may ſeem very differing from it, but yet is no leſs true; namely, that we are not always to ſuppoſe, that becauſe a natural body has ſuch an uſe on ſome occaſions, the ſame body cannot on other occaſions be employed to uſes, that ſeem of a quite differing, and perhaps of an oppoſite nature.

This I conceive may be done principally by theſe two ways. Firſt, by the differing conſtitutions of the ſeveral bodies the ſame agent works upon; as when the heat of the ſun melts wax and hardens clay; and the ſame ſpirit of vinegar, which on filings of copper will by digeſtion obtain an abominable taſte, will upon filings of lead acquire, by the ſame way, a very great ſweetneſs: and ſpirit of ſalt, that will diſſolve copper and iron, as aqua-fortis alſo does, will yet precipitate ſilver diſſolved in that menſtruum. And to this firſt way I ſhall ſubjoin the ſecond, which is, that ſuch a parcel of matter, as is wont to be conſidered as one and the ſame body, may contain in it parts of very differing natures, upon whoſe account its operations may be diverſified. Thus when we calcine ſome unripe minerals with nitre, the inflammable parts of the nitre do burn up and diſſipate into ſmoke the volatile and combuſtible parts of the mineral; but by virtue of the remaining alkali of the nitre, ſeveral other parts of the mineral are made far more fixed and capable of enduring the fire, than they were before. So ſulphur has in it ſome parts, that make it more readily inflammable than even nitre or oil; and yet it abounds with acid and vitriolate particles, that are not inflammable themſelves, and much reſiſt the accenſion of flame in divers other bodies. And accordingly, though in matches uſed in tinder-boxes to take fire readily, the kindled brimſtone acts upon the ſhivers of wood, whoſe ends were cruſted over with it, as an ordinary flame; yet the ſame burning body, by virtue of its acid parts, works in another capacity, than that of a common flame upon ſome metals, eſpecially iron, and likewiſe upon the leaves of red roſes, which its fumes turn white.

I could, if it were needful, propoſe in this place, ſundry other inſtances of the differing actions of the differing parts of a body, and could likewiſe ſubjoin other caſes, than I have yet mentioned, wherein bodies may be applied to uſes, that many would be unapt to expect from them. But judging it more convenient to reſerve thoſe for other places, eſpecially in the laſt eſſay, I ſhall conclude this with the two following advertiſements.

The firſt is, that I have in all this diſcourſe purpoſely forborn to treat of the medicinal uſes of things, becauſe my ſcope in the volume, whereof this eſſay is a part, obliged me ſo to do. But yet I am ſenſible, and would have you ſo too, that hereby I have forborn to employ a multitude of particulars, that would have much enriched this treatiſe. For there is a great number of bodies, both natural and factitious, that being employed as medicines for human bodies, have there very various and ſometimes ſeemingly repugnant operations, many of which would ſerve to illuſtrate and confirm ſundry paſſages of this eſſay. Thus rhubarb, whether taken in ſubſtance or infuſion, does by virtue of its differing parts, firſt purge, and then bind. Spirit of wine taken inwardly exceedingly heats the body; whereas outwardly it is employed to appeaſe the heat cauſed by ſome hot humours and inflammations. Mercury taken inwardly, crude as it is, has often, though not always, proved an effectual and harmleſs medicine in worms, and ſome other diſtempers, even to children and women in labour: but the ſame mercury rarified into fumes, (which yet may be condenſed again into running mercury,) and in that form taken into the body, does too often cauſe vehement and dangerous commotions in the juices of the body, as exceſſive ſalivations, fluxes, &c. declare. And he, that ſhall attentively conſider the various operations of that one mineral antimony, and the not only differing, but oftentimes contrary effects, that it produces, according to the complexions and diſpoſitions of the taker's body, and the preparation of the mineral itſelf, will not, I preſume, ſtick to allow me, that the medicinal uſes of things, if I had not thought fit to decline them in this eſſay, might have much increaſed the number of inſtances it contains; the effects of other bodies upon thoſe of men being no leſs proper inſtances of nature's ways of working, than the changes they produce, when they work only upon one another.

The ſecond advertiſement, wherewith I ſhall conclude this eſſay, is, that though what I have hitherto diſcourſed, hath almoſt ſolely related to the neglected uſes of particular natural bodies; yet I would not have you thence take occaſion to imagine, that there are not other natural things, whereof divers uſes may be made, that men have hitherto either ignored, or overſeen. By other natural things I mean the differing ſtates of matter, or of bodies, (ſuch as rarity and denſity, fluidity and firmneſs, putrefaction and fermentation, may ſeem to be,) as alſo the more operative qualities, ſuch as heat, cold, gravity, &c. the laws of local motion among the parts of matter, and the preſent fabrick of the univerſe, and eſpecially that of our terreſtrial globe and its effluviums; to which might be added other things in nature, that are not properly bodies in the uſual ſenſe of that word, but may be called things corporeal as they belong to bodies, and intirely depend on them. In favour of this advertiſement it were eaſy for me to ſuggeſt to you ſuch a multitude of particulars, that reſerving ſome few for the laſt eſſay, I here purpoſely forbear to mention any at all, to avoid being enticed or engaged to enter upon a ſubject, that could not be otherwiſe than very lamely handled, without enormouſly ſwelling an eſſay, that does already exceed its juſt dimenſions.

TRACTS.

OF

A Diſcovery of the ADMIRABLE RAREFACTION of the AIR.

New Obſervations about the DURATION of the SPRING of the AIR.

New Experiments touching the CONDENSATION of the AIR by mere COLD; and its COMPRESSION without Mechanical engines.

The admirably DIFFERING EXTENSION of the ſame Quantity of AIR rarified and compreſſed.

ADVERTISEMENT.

THE Author of the following papers ſuppoſeth his readers to have learned, either from the books he hath publiſhed, or from what hath been borrowed thence by other writers, the ſtructure and more familiar uſes of a pneumatical engine of his, metioned by ſeveral authors under the name of Machina Boyliana; with whoſe deſcription therefore thoſe are deſired to acquaint themſelves, that ſhall think it worth the while to underſtand, as well as read, the following papers; about which it might be further taken notice of, that the firſt of them was indeed written to a learned friend, though his name be not now annexed (for certain reaſons;) preſently after which the three others were thought fit to be ſubjoined. As for the omitting of the compliments and forms, uſual at the cloſe of epiſtles, the author did it, as well to ſpare the reader, as himſelf; who hopes he may be excuſed, if the tranſitions from one diſcourſe to another, and even the ſtile and method of them, be not ſo ſmooth and regular, in regard the enſuing writings were traced, when he was afflicted with a great fit of ſickneſs, that kept him from ſo much as once reading over himſelf, what he had indited.

A
DISCOVERY
OF THE ADMIRABLE
RAREFACTION OF AIR,
(EVEN WITHOUT HEAT)
IMPARTED
In a LETTER to a FRIEND.

DO not imagine, Sir, that I did at all wonder to ſee you yeſternight ſo much admire, to hear me talk with ſo much ſeeming extravagancy about the rarefaction and condenſation of the air; for I confeſs, that I did deliver ſomething on that occaſion, that might eaſily, at firſt, ſight appear ſo near impoſſible, as to be utterly improbable.

And though you were pleaſed, even on ſuch an occaſion, to expreſs a very favourable opinion of my veracity, yet thinking it fit, that ſuch an obligation ſhould not divert, but engage me, to endeavour to juſtify you to yourſelf, by confirming what I ſaid to you; I have already ſought and found among papers, many years ſince laid aſide, ſome, that will enable me to make good more, than what the diffidence of my memory allowed me to ſay in the very boldeſt part of my yeſternight's diſcourſe. For now that I luckily find not only the originals of the relations, whereof this paper contains copies, but that my engine is in good order; I am ſo far qualified to countenance a diſcourſe, wherein I kept ſomewhat within compaſs, that though it will perhaps coſt me much pains and trouble, to make *ex tempore* experiments fully equal to the encloſed; yet if any juſt doubt ſhould require it, I preſume, I can make ocular proof of, at leaſt, as much, as I laſt night told you.

And now it is time, after having contrary to my cuſtom, raiſed in you a high expectation, that I endeavour in ſome meaſure to anſwer it, which I hope I ſhall the more eaſily do, becauſe the agreement, you have often had occaſion to obſerve between the relations regiſtered in my adverſaria, and the phænomena of the experiments they deſcribe, will, I preſume, make it needleſs to perſuade you, that the enſuing trials, being tranſcribed thence, may be ſafely credited. Wherefore I ſhall proceed to annex them, as ſoon as I have premiſed a few hiſtorical lines, by way of manuduction to them.

It is now many years ſince, that having a deſire to reduce the air to a degree of rarefaction, that appeared to be conſiderable, upon ſurer grounds than ſlight conjectures, I attempted to do it by the help of heat, and particularly by that of an œolipile, which I have mentioned in another tract *: but finding, that the diligent *Merſennus* had, if there be no miſtake in his account, been able to rarify air that way, full as much, or more than I could, I betook me to try, whether I could not, by the ſpring of the air (without heat) procure a greater expanſion of it? I found (as I have long ſince elſewhere † related) that in the pneumatical engine, which has been ſince called Machina Boyliana, I could increaſe the expanſion of air, till the body attained to about one hundred fifty-two times its former and uſual dimenſions. But this expanſion, though it were above twice as great as the utmoſt procured by *Merſennus*, did not yet ſatisfy me, but put me, (according to what I there intimate) upon another contrivance, which though put in practice eight or nine years ago, (as the date of one of the trials may inform you) had the relation of its ſucceſſes laid aſide among thoſe of others, made in the ſame engine, which yet lie by me unpubliſhed. So that I may now proceed to give you the tranſcripts of the trials themſelves, as they were haſtily and inelegantly, but very faithfully, ſet down among my Pneumatical Collections. And this I am ready to do, as ſoon as I ſhall have intimated to you, that in that noble collection of experiments, that has about two years ſince appeared in publick, as the firſt-fruits of the juſtly famous Florentine Academy, I find, that thoſe virtuoſi had, according to their ſagacity, ſo advanced the extent of the air, as without the help of heat to bring the dilatation to exceed one hundred ſeventy-three times its former dimenſions; and that, which made their improvement the more conſiderable, and conſequently the more worthy of them, is, that they procured this great rarefaction, as well as I had done mine, by the air's own ſpring; and had ſurpaſſed without the help of my engine, what I was then at firſt able to do by the conveniencies that it afforded me. Whereupon, remembering what I had performed in that kind ſeveral years before, I ſought among my papers for the trials I had then made, and found thoſe notes, whereof, I now, at length, think it high time to give you the promiſed copies in the following terms.

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* New Phyſico-Mechanical Experiments, Exper. VI.

† Ibid.

EXPERIMENT I.

We took a round glaſs-egg (as they call them) of clear metal furniſhed with a pipe, or ſhank, of ſome inches in length; this we filled with water, and conveyed both it and a vial with water in it, into a receiver of a convenient ſize, and by pumping the air out of it, we made the bubbles both in the egg and the vial to diſcloſe themſelves in great numbers; ſo as to make the liquor in the glaſs-egg ſeem to boil, and to make all that was in the ſhank really to run over. When we thought the water was ſufficiently freed from air, which it was not quickly brought to be, we took out the glaſſes and filled up the pipe of the egg with water taken out of the vial, and inverted it into more of the ſame water, in ſuch manner, that the egg was quite full, ſhank and all, excepting a ſmall bubble of air, that we purpoſely left to gain the top of the egg; where, the glaſs being tranſparent, with a pair of compaſſes we meaſured as accurately as we could, and found it to be a tenth, and leſs than two centeſms of an inch. Then putting the glaſſes again into a receiver, we ſet the pump at work, and the little bubble, after a while, began to expand itſelf, which when it had once done, it did at each ſuck ſtrangely increaſe, till at length it drove all the water out of the round part of the glaſs. And leſt it might be objected, that it was only the ſubſiding of the water upon the withdrawing of the outward air, that before kept it up to the top of the glaſs, we cauſed the pumping to be ſo continued, till the expanded air had ſeveral times driven the water in the pipe of the egg, a pretty way beneath the level of the external and ſurrounding water in the other glaſs. This done, we let in the air by degrees, with a deſign to obſerve, what bubble we ſhould find at the top of the egg, when the water ſhould be again driven up into its cavity. But the expanded air had forced over ſo much water, that there remained not enough to fill the globulous part of the egg: wherefore we tried the experiment again, and when we had proceeded thus far, we compared the above-mentioned diameter of the ſmall bubble, with that of the ſpherical part of the glaſs, which we took with a pair of Callaper compaſſes: and though we found it to be ſomewhat more than 20 times as great, yet being willing rather to diſfavour than flatter the experiment, we ſuppoſed the two diameters to be as 1 to 20, and conſequently, ſince, as *Euclid* demonſtrates, the proportion between ſpheres is triplicate to that of the diameters, and in our caſe, the cube of the leſſer diameter being one, is alſo but one, the cube of 20, the greater diameter, muſt be 8000; and ſo the air appears to have, by expanding itſelf, acquired a place 8000 times as big as it poſſeſſed before. Nor was it overſeen by us, that the globulous part of ſuch glaſſes as we uſed is ſcarce ever made ſpherical. But not only I, but Dr. *Wallis*, who was pleaſed to aſſiſt at the experiment, concluded, that the cavity of the ſhank, which the expanded air drove the water from, but which, we did not compute, would make abundant compenſation for the two above-mentioned particulars. After this, for further ſatisfaction, we took water, laboriouſly freed from air, and putting it into the ſame glaſs-egg, we inverted it as before, but left not any bubble in it. This we did, that in caſe we could make the water ſubſide, the experiment might prevent a ſuſpicion, that ſome air latitant in the water might increaſe the bubble that was formerly left in it; having then exhauſted the receiver as much as before, and if we miſtook not, more, the water in the egg did not all ſubſide; but at length, with obſtinate pumping, a bubble diſcloſed itſelf, and drove all the water clear out of the round part of the glaſs; and though by reaſon of ſome ſmall leaks, that we could not find or ſtop, we were not able, as before, to make the expanded air depreſs the water in the ſhank, beneath the ſurface of the external water, yet we wanted very little of it; and then out of wearineſs giving over, we found, that when the water was impelled up again into the egg, there was at the top of it a bubble, whoſe diameter we meaſured as faithfully as we could, and found it to be to the diameter of the globular part of the glaſs, as 1 to 14; ſo that, though the little bubble had been a perfect ſphere, yet ſpheres being, as was lately noted, in triplicate proportions to their diameters, the bubble when expanded, muſt have been 2744 times as big as the bubble unexpanded. But Dr. *Wallis*, who will be allowed to be a very competent judge in theſe matters, obſerving (what we all took notice of) the great thinneſs of the bubble, poſitively and conſtantly affirmed, that he could not eſtimate it to be at moſt any bigger than the third part of a perfect ſphere of that diameter; by which eſtimate the expanſion of the bubble muſt have reached to 8232 times its natural dimenſions.

El. 12. Propoſ. ult.

N.B. By letting in water into the receiver, as much as it would admit, we found, that by reaſon of ſome ſecret leak, we had not been able ſo to exhauſt it, but that there remained ſome air.

EXPERIMENT II.

A SMALL and almoſt inconſpicuous bubble expanded itſelf, when the ambient air was pretty well exhauſted, to more than 10,000 times its former extent. The manner thus: we took a ſmall bolt-head, blown by a lamp, which

June 2, 1662.

which contained in all about 80 grains of water, and inverting the ſmall neck into a jar of water, it was included in the receiver, and the ambient air being exhauſted, ſtore of bubbles roſe out of the water, and expanding itſelf, quickly drove all the water out of the bolt-head. Then re-admitting the outward air, the bolt-head was preſently almoſt filled, and all the expanded air ſhrunk into a bubble, little bigger than a ſmall pin's head; then taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully filled it up with the water, that had been freed from air, and then inverting it as before into the jar with water, we again included it, and after ſome exſuctions found, that there was gotten out of the water into the neck a very conſpicuous bubble, which, upon the admitting of the air, ſhrunk almoſt into an inviſible one, and aſcended into the head of the glaſs. Then again exhauſting the receiver very well, we found it expand itſelf, ſo as to fill all the capacity of the bolt-head, and to drive out almoſt all the water. And upon the re-admitting of the air, it again ſhrunk into a bubble, whoſe diameter (according to our beſt eſtimate) was not bigger than one two and twentieth part of the diameter of the head of the above-mentioned glaſs; ſo that to fill the whole cavity of the head only, it expanded itſelf 10648 times: but becauſe it filled likewiſe the greateſt part of the neck, we found by weighing the water that filled that part, and the water that filled the head, that the capacity of that part of the neck, was almoſt a third of the capacity of the head, being as 141 to 481: if therefore 481, the capacity of the head, contained it 10648 times; 141, the capacity of the neck, muſt contain it 3121 $\frac{167}{481}$ times; ſo that in all, the ſmall bubble of air was expanded to above 13769 times its former bulk.

The diameter of the ſmall bubble retracted was $\frac{1}{27}$ of an inch.

The diameter of the outſide of the head of the glaſs was $\frac{22}{36}$ of an inch.

The water that filled the head only weighed 60 $\frac{1}{2}$ grains.

The water, that filled the head, and as much of the neck as the air had before expanded itſelf into, weighed 78 $\frac{1}{8}$ grains; ſo that that part of the neck weighed 17 $\frac{5}{8}$ grains.

The bolt-head itſelf weighed 15 grains.

I might have ſet down this ſecond experiment unaccompanied either with the firſt, or with that I am going to ſubjoin; becauſe the expanſion produced by neither of them was, at leaſt by meaſure, ſo vaſt, as that produced by the trial newly mentioned: but this was ſo ſtupendous, that I thought it not ſo fit to preſent it to you by itſelf alone, but rather accompanied with other experiments, the leaſt proſperous of which produced a dilatation of air ſufficient for my preſent purpoſe, and ſuch as may not a little confirm, that what is recited in the ſecond experiment, was neither a lucky chance, or miſtake. And that may be enough for my preſent purpoſe; for as for the little abatements, that ſome will perhaps think fit to be made upon the ſcore of the unequal thickneſs of glaſs or ſome ſuch circumſtances, they are not conſiderable enough to deſerve to be now ſolicitouſly debated, nor to hinder the expanſion, that muſt be granted from proving what they are alledged for: wherefore I will proceed to what follows.

EXPERIMENT. III.

We tried this experiment again, and found a ſmall bubble, much about $\frac{1}{12}$ of an inch in diameter, filled not only the ball at the end of the bolt-head (which was 1 $\frac{1}{2}$ of an inch in diameter) but the whole neck, which contained near as much water as the head, and beat down the ſurface of the water within the pipe, much below that of the water without the pipe.

These experiments already found among my old papers will, I hope, without ſeeking for more, ſuffice to manifeſt, that the expanſion, which the air may be reduced to without heat, is indeed admirable; for if we make an eſtimate of it but according to the experiment, which had the moſt moderate ſucceſs, it appeared, that one ſpace poſſeſſed, though not adæquately filled, by a portion of air, may have its air extended to at leaſt 2744 ſpaces equal to it; I ſay, at leaſt, becauſe very probably it was above twice as great: and if we make our eſtimate according to the moſt proſperous of our trials, we muſt allow the air to be rarefiable at leaſt 13000 times; I ſay again at leaſt, becauſe I am not ſure, that in that trial it was reduced (not fully, though perhaps very near) to the uttermoſt degree of rarefaction attainable in our engine: ſo that I preſume you will now grant, that I ſpoke warily and much within compaſs, when I mentioned but an expanſion from one to a thouſand.

And now having performed the promiſe I made you, it remains only, that I take notice of the requeſt, that you made me, about communicating theſe experiments to the curious. But this deſire of yours is oppoſed by no ſmall inconveniencies, that would reſiſt my compliance with it. For it would oblige me by tearing out theſe papers, to diſmember a collection long ago in making, and wherein they were placed to be much otherwiſe diſpoſed of, and not only make a great gap in it, but ſtrip or deprive it of ſome things, that were the likelieſt to recommend it. Beſides that theſe appearing before the reſt are odd enough to make theſe ſeem far leſs uncommon, than perhaps otherwiſe they would. Yet all this notwithſtanding, I find it uneaſy to refuſe, what you, and thoſe friends, that concur with you on this occaſion, deſire, that if after having once more peruſed theſe papers, you perſiſt in the ſame earneſtneſs you expreſſed yeſterday, when you had not yet ſeen them, I ſhall not refuſe you the diſpoſal of them, both for the reaſon now given, and becauſe I have been informed as well by you as by other means, that the rarefaction of the air is at preſent the ſubject, that buſies the diſquiſitions of ſeveral eminent virtuoſi, both domeſtick and foreign, to whom I pay ſo much reſpect, that I ſhall think it a happineſs, if it may be acceptable to them, not only becauſe it will be ſeaſonable, but becauſe, that though the engine, that moſt of the attempts were made in, has not been thought altogether barren, yet theſe trials will probably paſs for one of the leaſt inconſiderable productions of it: and theſe two ſervices I hope this ſhort writing may do ſeveral ingenious readers; the one, that it will invite and accuſtom them to take notice of, and conſider the great ſubtlety of nature, and the ſcarce imaginable ſmallneſs of thoſe aerial inſtruments, that ſhe employs even about viſible operations: the other, that theſe relations will excite the more curious and piercing wits to debate, and I hope help them to ſolve the two problems here propoſed to them; what figures and motions may be aſſigned to the particles of the air, to explicate it's ſo wonderful rarefiableneſs, and that perhaps without quite loſing its durable ſpring, and how the air comes to be rarefiable ſo many times more without heat, than hitherto we have found it to be by heat. To which might be added, as a third, what might be reaſonably conjectured about that part of the cavity of an exactly cloſed glaſs, where, though the eye diſcovers no viſible ſubſtance harboured in it, it appears not, that the common air does adæquately fill ſo much as the ten thouſandth part?

NEW OBSERVATIONS
ABOUT THE
DURATION OF THE SPRING
OF
EXPANDED AIR,
(Subjoined by way of APPENDIX to the foregoing EPISTLE.)

FORASMUCH as reviewing the former paper about the Rarefaction of the air, I took notice in the cloſe of it of an expreſſion (viz. and that perhaps without quite loſing its durable ſpring) which I fear may, to ſome readers, ſeem to need explication; it will not be improper on this occaſion to ſubjoin ſomething by way of appendix about it.

FIRST, then the reaſon, why, in this ſhort intimation, I thought fit to employ the diffident term *perhaps*, was, becauſe I had not (nor yet have) been taught by trial, whether and how far the utmoſt expanſion of the air actually produced in my engine, or otherwiſe procurable, and its retaining a ſenſible ſpring, are conſiſtent. I expreſs my ſelf thus, to inſinate, that I thought of other inſtruments and methods, whereby the dilatation of the air may not improbably be meaſured and promoted; as by making the Torricellian experiment in a glaſs with a very capacious head or globulous part, and applying the aerial particles, that will aſcend out of the ſubſiding mercury together with a bubble of other air, if it be needful, to the uſe we have been ſpeaking. Something alſo may be done, to ſome purpoſe, with very fine and large fiſh-bladders; but I ſhall not inſiſt on theſe, or the other expedients, that came into my thoughts, contenting my ſelf to have intimated, and thereby acknowledged, that there may be other means beſides the Machina Boyliana, to bring air to a very great expanſion. But whether any of them will ſurpaſs what has been actually attained in that engine, time muſt declare; till when, we ſhall be content to make uſe of the experiments it has already actually furniſhed us with.

WHEREFORE to come the ſecond or other remaining part of it; whereas in the mentioning of the ſpring of the expanded air, I employed the attribute of durable, you may eaſily gather the reaſon, from what I am now going to annex.

I had obſerved, not without ſome wonder, in the enquirers into the nature of the air, that they have not, that I know of, ſo much as attempted to diſcover, whether the air, either in the utmoſt, or in the intermediate degrees we can bring it to, does retain a conſtant and durable elaſticity?

FOR, firſt, men have not determined, whether a portion of our common air being exactly ſhut up in an hermetically ſealed glaſs, or ſome other exactly cloſed veſſel, will conſtantly and uniformly, for a moderate time at leaſt, retain the degree of elaſticity it had when it was ſhut up: And whether it will not ſometimes vary its preſſure, as we ſee, that the atmoſpherical (though I think upon peculiar grounds) is, by the help of our baroſcopes, obſerved to do? Next, it does not appear, whether included air, in caſe it retain an uniform elaſticity for a moderate time, will retain it for a very long one. Nay, whether it would not at length come not to have a weaker ſpring, but perhaps to have no ſenſible ſpring at all, as we ſee in happen it ſword-blades and divers other ſpringy bodies, which, after having ſtood too long bent, will continue ſo, and loſe their former power of ſelf-reſtitution, as they call it.

THIRDLY, Men have not yet determined any thing about the degrees of the air's elaſticity, whether the durableneſs and uniformity, or varying of its ſtrength, may not depend upon the differing degree it had, when it was firſt ſhut up.

FOURTHLY, Much leſs have we yet attempted to diſcover, whether the ſpring of an encloſed portion of air may be ſometimes weakened, and ſometimes ſtrengthened by the changes, as to gravity, of the outward atmoſpherical air, the new and full moon? To which I might add divers other external accidents, which, as yet, we ſcarce ſuſpect. And to theſe I might add ſome other doubts and enquiries, that may not be impertinently ſuggeſted, but here would, I fear, paſs for a digreſſion.

WHEREFORE I ſhall proceed to tell you, that having taking notice of it, as an omiſſion among the inquirers into the nature of the air, in whoſe negligence I was too long a ſharer, that we have not, that I know of, ſo much as attempted to diſcover this itſelf: whether the air, either in the utmoſt, or in the intermediate degrees of rarefaction we might bring it to, would for a conſiderably long time retain its elaſticity, or at leaſt, ſome determinate degree of it, or loſe it by determinate and regular decrements, I thought fit to make ſome trials about this matter, but cannot brag of the ſucceſs of my intentions, having been hindered either by want of inſtruments, or by removes, or by ſickneſs, or by unlucky accidents, or by one unwelcome thing or other, from accompliſhing what I had chiefly deſigned, and partly alſo made ſome progreſs in it; but yet to give you ſome hints, as well as ſome occaſion to more proſperous experiments, I ſhall not ſtick to annex, what I readily call to mind about my attempts on that occaſion.

I remember then, that when I firſt began to try ſomething in order to my deſign, being deſtitute of fit accommodations, I was fain to content myſelf, by cauſing a good bubble of glaſs with a ſtem to be ſo blown at the flame of a lamp, that whilſt the ball was yet exceeding hot, and conſequently contained none but highly rarefied air, the ſtem was very nimbly clapped into the flame of a candle, that was purpoſely kept ready at hand; ſo that being ſlender, it was in a trice ſealed up, and the air within remained as much expanded, as the great heat, it had been expoſed to, had brought it to be. This bubble many months after I inverted into a baſon of water, and having broken off the ſeal under the ſurface of it, the liquor was violently impelled into the cavity, but yet was not able to fill it, a conſiderable part being defended from the further aſcenſion of the water by the ſpring of the remaining air, which, for all the long ſtretch it had been put to, had not loſt any thing of its ſpring, that we could take notice of. But this was a trial, in which I could by no means acquieſce; and therefore when I was a little more befriended by opportunity, I tried another way, partly to give a ſomewhat pleaſing ſurpriſe to unaccuſtomed beholders, and partly, becauſe though it could not ſhew all, that I deſired, yet it might plainly ſhew, that the air, even at a very conſiderable extenſion, would hold out for a conſiderable time. Wherefore leaving a very ſmall proportion of air in the folds of a fine limber bladder, whoſe neck was very cloſely tied, I cauſed it to be, by the help of the Machina Boyliana, ſo expanded, that at length it ſo dilated it ſelf, as to ſeem to fill the whole bladder, and reduce it to the extent it had juſt before it was emptied; and the bladder, by a peculiar contrivance, was ſo included in another veſſel, that being protected from all intruſion of the outward air, it maintained its plump and tumid figure, and in that unwrinkled ſtate I ſhewed it, many months ſince, to ſome virtuoſi, now here in *London*, after it had continued ſo, if I miſtake not, near two years. Since the writing of this, I did, at length, find the newly mentioned veſſel, and ſhewed it to ſome curious ſpectators, who with me took notice, that the included bladder, inſtead of being wrinkled or ſhrunk, appeared to be plump and full, as well blown bladders are wont to be. So that many months, perhaps a dozen, may be added to the freſhly mentioned duration of the expanded air.

BUT this way ſatiſfying me neither as to ſome of the particulars I deſired my attempts ſhould diſcover, I deviſed a little inſtrument, whoſe contrivance, though it ſeemed very ſimple, promiſed, and for ſome time gave me a far more accurate account of what I expected. The inſtrument, if you deſire it, I can eaſily ſhew you, having lately been forced to make a new one, which is now by me: but it may ſuffice to tell you, that is is ſo framed, that it is fit to diſcover, beſides divers other things, whether, and how long air brought to the greateſt expanſion I could conveniently reduce it to in my engine, will retain its ſpring; and by what degrees or ſtages and periods of time, the decrement, if any be, is made? But of the iſſue of the trial made in it, I can give you but a very imperfect account, in regard, that, though I made it about three years ago, yet having left the inſtrument in a place, where it is ſo lodged, that I cannot have it without returning thither, till I ſee it again my ſelf, I dare not venture to judge of the ſucceſs of the experiment: only this I remember, that I took no notice of any obſervable diminution in the air's elaſticity though, it were preſſed, and, as it were, clogged with a weight, that one would wonder how it could, when it was ſo highly rarefied, ſupport for one minute*.

THERE is alſo another way, that I contrived, wherein the air in a little portable inſtrument, which I can ſhew you, being expanded, as one may gueſs, to five or ſix hundred times (perhaps a thouſand times) its wonted extent, has not only for a long preſerved its ſpring, but ſatisfies me alſo about one of my chief queries, which was, whether the air, very much dilated without heat, would be conſiderably ſenſible of external heat? which it plainly appears to be in this inſtrument, where, notwithſtanding the great rarity it has already attained and ſeems likely to preſerve, the heat of one's hand applied to the out-ſide of the veſſel has a quick and very manifeſt operation; and upon the withdrawing of it, the ſenſible air quickly returns to its former dimenſions, as well as temper; ſo that one may employ it as a kind of weather-glaſs, and perhaps make ſome diſcoveries by long comparing it therewith.

BUT hitherto I have been doing, what I do not love to do, and very rarely have done, when I mention my own experiments, that is, I have not punctually ſpecified any determinate quantities and proportions of the things ſpoken of; but one of my former trials I have newly found out regiſtred in a looſe note, and therefore the quantities being annexed, I hope it may both give ſome countenance to what I have been ſaying, and give ſome, though not an entire, ſatisfaction about the thing itſelf.

March 18 A glaſs, as cylindrical as we could get it blown at our lamp, and having a long ſtem coming out at the unſealed end, was quite filled with water and inverted into water placed at the bottom of a large pipe ſealed at one end, and of three or four foot in length. This external pipe, ſo called for diſtinction ſake, was exhauſted, till the air, that diſcloſed it ſelf in the water of the internal pipe, had drawn out the water in the cylindrical pipe, as low as the upper part of the ſtem; at which great expanſion of the air the external pipe being ſpeedily and ſecurely cloſed by a certain contrivance, the air thus rarefied was kept ſometimes in my own chamber, that was warmer, ſometimes in an under room; and after it had been kept from firſt to laſt about eleven weeks or three months, if I miſ-remember not, without any other remarkable variation, than that in the cold room the water aſcended, as I gueſs'd, about an eighth, or near a fourth at that part of the internal pipe; where the lower end of the cylinder gradually leſſened itſelf into the ſlender ſtem. Yeſterday I invited doctor *Wallis* to be preſent at the breaking of the glaſs, and to favour me with his aſſiſtance, for the better eſtimating the expanſion of the air upon the breaking of the cloſed apex. The water was but leiſurely (becauſe of the ſlenderneſs of the orifice, that was made for the air to get into it) impelled up into the formerly deſerted cavity of the cylinder, which it filled all, ſave a little bubble, which was exceeding ſhallow. We made uſe of our eyes at a fit diſtance, and of compaſſes both ordinary and callaper, to obtain theſe meaſures. The cylindrical part of the internal pipe was three inches in length, and three fifths of an inch, or leſs, in diameter on the outſide. The bubble was two tenths in diameter, and about two centeſms in depth: From all which, by the doctor's calculation, the bubble, to the ſpace it poſſeſſed unexpanded, was at one to one thouſand three hundred and fifty.

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* See the poſtſcript.

NEW EXPERIMENTS
TOUCHING THE
CONDENSATION OF THE AIR
BY MERE COLD,
AND
Its COMPRESSION without MECHANICAL ENGINES.

BECAUSE it is as truly, as commonly, ſaid, that *contraria juxta ſe poſita magis eluceſcunt*, and becauſe what I am now going to interpoſe, is little leſs than neceſſary to be premiſed, to clear the way to what follows, and to connect the paſt writing to that which is to enſue; it will not be improper, to add ſomething in this place touching the condenſation and compreſſion of the air.

AND here I cannot but a little wonder, that among ſo many, that have had occaſion to conſider the nature of cold, and the condenſation of the air by it, I have not yet met with any, that have had the curioſity to meaſure that condenſation; wherefore I long ſince attempted to do it, as I have related in another diſcourſe; but not having that by me at preſent, and remembring in general, that I did it in winter, when it may be objected, that the air, being already præ-affected with the coldneſs of the ſeaſon, was not capable of being ſo conſiderably contracted by an additional cold, as it would be at a time of year, when it is wont to be in a ſtate of greater laxity; I thought fit to make the experiment about the beginning of autumn, without tying my ſelf to make it with the ſame circumſtances, that I had done before the event of this trial I find regiſtered as follows.

AFTER the midſt of *September*, on a ſunſhiny day, and about noon (which circumſtances we made choice of, that the air might be the more rare and expanded) we took a bolt-head or round vial furniſhed with a long ſtem, and placed in a frame purpoſely provided, ſo that the ſtem was perpendicular to the horizon, and the globulous part was ſupported by ſuch a veſſel, that thorough a hole, purpoſely made at its middle, the ſhank reached downwards, till the orifice of it was a little immerſed beneath the ſurface of a glaſs full of water, that was placed at the bottom of the frame. This done, we took a good proportion of ice, and having beaten it in a mortar, and mixed with it a due quantity of bay-ſalt, we not only laid it round about the lower part of the ball, but the veſſel contiguous to that part, being purpoſely made with turned-up brims, we were enabled to heap up the frigorifick mixture, ſo as to bury the whole globulous part of the glaſs in it, and cover the very top of it therewith to a conſiderable thickneſs; upon which occaſion, the air within being exceedingly refrigerated, the water, into which the ſhank terminated, was made to aſcend ſomewhat faſt along the cavity of the ſhank, till we perceived it would reach no higher, but after a while began to ſubſide again; which nick of time being carefully watched, we made a mark at the higheſt ſtation of the water, and then taking out the bolt-head we filled it with water, making allowance for that ſmall part of the ſtem, which was immerſed at the beginning of the operation. This water we weighed, and found it amount to nineteen ounces and ſix drachms, then weighing as much water, as ſufficed to fill the ſhank up to the mark newly mentioned, we found that to be one ounce and three drachms, by which number the former being divided, the quotient was fourteen drachms, four elevenths, ſo that the proportion of the two quantities of water being as eleven to one hundred fifty-eight, the ſpace, into which the air was condenſed by refrigeration, was, to the ſpace it poſſeſſed in its former ſtate of laxity, as one hundred forty-ſeven to one hundred fifty-eight, and conſequently the greateſt condenſation, that ſuch a time of the year and in ſuch weather, ſo high a refrigeration could bring the air to, made it loſe but $\frac{11}{158}$ of its former extent.

NB. FIRST, the ſtem of the glaſs ought to be of a conſiderable length, leſt by the great contraction made of the air in the ball by its high refrigeration, the water ſhould aſcend into the cavity of the ball itſelf, and thereby become exceeding difficult to be meaſured.

SECONDLY, if one would be nice, one may take notice, that the height, to which the water aſcended in the ſtem, was about two foot; which cylinder of water, by its weight or tendency downward, might ſomewhat hinder the liquor from aſcending quite ſo high as it would, and conſequently keep the condenſation of the air from appearing fully ſo great as it was, but ſo light a cylinder as that of the ſuſpended water would ſcarce be very conſiderable.

THIRDLY, when the water was aſcended near as high in the ſhank as it would riſe, there was obſerved in it an odd kind of ſubſultus, or riſing and falling alternatively, almoſt like the mercury in the Torricellian experiment, before the mercury comes to ſettle after its firſt ſubſidence. [But the conſideration of this phænomenon belongs not to this place; for which reaſon I inſiſt not on this, and forbear mentioning ſome others.]

FOURTHLY, that though it appears not by this experiment, whether the cold thus produced is equal to that of froſty weather in winter, and conſequently capable of contracting the air as much as that ſeaſon is wont to do; yet by preceeding trials, made with fit inſtruments, I had found, that by ſuch an application of ice and ſalt as we had made in the late experiment, a greater degree of cold, and that in a warmer ſeaſon, might be produced, than had been found neceſſary to make froſty weather in winter. The way of experimenting for brevity ſake I omit, but if you pleaſe you may command it.

BUT it is not chiefly to acquaint you with the condenſation, that nature uſes to make of air, that I have been entertaining you with theſe memorials; for that, which makes it very pertinent to my preſent purpoſe, is, that it will ſhew you, that as to the condenſation or compreſſion of air, that I am to recite, though cold were employed about it, yet it was not really produced by cold, which could not contract the air to ſo much as half that degree, you will find it was reduced to by our operation, preſently to be mentioned; wherein the frigorifick mixture did not primarily or immediately compreſs the included air, but only ſo affected the water, that was ſhut up within the ſame veſſel, as to make it ſwell, and conſequently crowd the aerial particles into leſs room: wherefore it now remains, that we proceed to the experiment itſelf, a ſhort account of which be pleaſed to take in the enſuing tranſcript.

[To convince ſome ſtrangers, we took a new glaſs bolthead, with a neck not long, and filled it ſo far with common water, that being hermetically ſealed, the liquor reached within three inches of the top, as near as we could gueſs by meaſuring it with a ruler, and making an eſtimate of the ſharp end, made ſo for the conveniency of ſealing up the glaſs, which ſharp end we gueſſed to be about a quarter of an inch in length, then applying ſnow and ſalt to the lower part of the bolt-head, we readily drove out the water further and further into the neck, till at length it was got up to the baſis of the ſharp and conical end, where the glaſs was ſealed, and then juſt as I was looking upon it, the glaſs flew with a noiſe about my ears, being broke into many pieces, which argued the compreſſion of the air to have been very great. And Doctor *Wallis*, who was preſent, and meaſured it from time to time, deſired me to regiſter the experiment, with his eſtimate of the compreſſion, which was, that the air was reduced into the fortieth part of its former extenſion.]

I know ſo great a condenſation of air will ſeem ſtrange to thoſe, that have taken notice, that ſome of the beſt mathematicians of our age, that have made uſe of wind guns, and both forcible engines to crowd the air into as narrow room as poſſibly they could, confeſs themſelves not to have been able, with all their ſtrength and induſtry, to force the air into leſs than the fifteenth part of its uſual extent; and beſides, that this was done in countries, where the air may * well be ſuppoſed more lax and rare than in *England*. I confeſs I ſaw no trials made with wind-guns, that convinced me, that the condenſation was ſo great as that newly ſpecified; (about which *Merſennus* himſelf ſomewhat heſitates, ſeeming to doubt, whether the air were indeed reſtrained into a fifteenth, or but into one eighth part of its former room.) And he, that hath obſerved and conſidered, as I have done, that in wind-fountains, as they call them, of glaſs, the air will ſeem to be notably compreſſed, whilſt, indeed, we could not find it compreſſed into much leſs than its third part, will be the leſs unapt to be diffident of the great things, that are ſaid of the compreſſion of the air; but becauſe experience has informed us, that our Engliſh air may in peculiar inſtruments be forcibly crowded into a tenth, twelfth, or perhaps a fifteenth part of its former extent, I am content to take it for granted, what is related about the compreſſion of the air, into the fifteenth part of its uſual dimenſions; and yet our experiment will be a conſiderabler inſtance of the great compreſſibility, if I may ſo ſpeak, of the air; for, according to the eſtimate delivered in the foregoing narrative, our compreſſion, which was without mechanical inſtruments or engines, reduced the air into the fortieth part of the ſpace it had lately poſſeſſed; and how great a force is requiſite, when the air is once conſiderably condenſed, to ſurmount, though but a little, its great reſiſtance to further condenſation, may be gathered from the obſervations about the gradual renitency of the air to compreſſion, which we many years ſince made with mercury, and afterwards publiſhed in another treatiſe: but though upon the recited grounds, that great compreſſion of the air produced by our experiment may, as I was ſaying, ſeem very ſtrange, yet it would not ſeem incredible, if I ſhould here borrow thoſe experiments and obſervations from my already publiſhed hiſtory, and ſome unpubliſhed papers about cold, that would countenance what I have been delivering, and eſpecially if I ſhould ſtay to communicate to you the way, I not unſucceſsfully made uſe of, to eſtimate by weight the great force of the expanſion of water upon its freezing. But ſince an account of this contrivance is not here neceſſary, and would require more leiſure than I can ſpare at this time, it remains only, that by way of corollaries from what has been hitherto delivered in this and the two precedent writings we rather point at, than diſcourſe of ſome obſervations, that it ſuggeſted to us, in the enſuing paper.

Defence againſt *Linus*.

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* See *Merſen.* Phæn. Pneum. Prop. 32.

OF

OF THE ADMIRABLY DIFFERING EXTENSION OF THE Same QUANTITY of AIR, RAREFIED and COMPRESSED.

HAVING already declared, that what I pretend to in the cloſe, is but to ſet down ſome obſervations, that reſult from, or are ſuggeſted by what hath been already delivered, I preſume I need not trouble you or myſelf, with any other preface to what follows.

THAT then, which ſeems firſt worth taking notice of, is the differing alterations, that the air is ſubjected to by cold and heat: for whereas we could not find in this our climate, that cold would reduce the air into near the twentieth part of its former extenſion by condenſation, heat would advance it to near ſeventy times its uſual laxity by rarefaction.

NEXT, we may obſerve, that by engines and other artificial inſtruments, the air may be two or three times as much compreſſed, as nature is wont to condenſe it by cold; even in froſty weather; and ſo on the other ſide, the air may by the intervention of art and inſtruments be much more rarefied and expanded, than it has been yet found to be by the bare application of external heat, though it were that of an intenſe fire itſelf.

See as to the utmoſt expanſion by heat *Merſen.* in Cog. Phyſ. Mathem.

FURTHERMORE, it may ſeem worth while to obſerve, how much the utmoſt degree of rarefaction by heat, that experiment hath ſhewn us of the air falls ſhort of the degree of expanſion, to which it has been advanced in our pneumatical engine, the proportion betwixt theſe two expanſions being that of one to ſeventy, or thereabout.

BUT, perhaps, it will not be neceſſary to conclude, that the air is ſo much more rarefiable than compreſſible, as moſt readers will be prone to infer, by comparing the greateſt compreſſion and expanſion of it, that are mentioned in theſe experiments; ſince, if I miſtake not, it ought to be conſidered, that the air, we made our trials with, upon the ſurface of the earth, was not (no more than is the air we commonly breath) properly in a true natural conſiſtence, as they ſpeak; or, if you pleaſe, in a free and indifferent ſtate in reference to rarefaction and condenſation, but was already highly compreſſed by the weight of the atmoſpherical pillar, that leaned upon it, ſo that is had already a very ſtrong renitency to further compreſſion; whereas the air, that was to be rarefied, had, by virtue of its ſpring, (ſtrongly bent by the weight of the incumbent air) a ſtrong propenſion or tendency to dilate itſelf; which difference I muſt content my ſelf to have intimated, and leave you to conſider, whether and how much it may alter the caſe.

FOURTHLY, To ſome perhaps it will ſeem more fit to conſider, than eaſy to reſolve, how, ſince the corpuſcles of the air are acknowledged to be heavy, and thoſe, that remain, muſt be ſo wonderful thinly diſperſed in the cavity of the receiver, they come to be ſupported, and kept, as it were, ſwimming therein, and do not appear to ſubſide by their own weight, the *Materia ſubtilis* (though the preſence of that ſhould be admitted) not appearing to have gravity, wherewith to ſuſtain them; and the vacuum (if that be ſuppoſed wherever the aerial particles are not) being too near a-kin to nothing, to be able to oppoſe their deſcent: but though ſomething may be ſuggeſted about the ſolution of this doubt, my haſte obliges me to leave it as ſuch.

FIFTHLY, I will not make it my buſineſs to make mention in this place of the wonder, that may be juſtly excited in thoſe, that when they look on one of our well exhauſted receivers, attentively conſider, how ſmall a proportion the common aerial corpuſcles, which are very ſparingly diſperſed there, bear to the whole cavity of the veſſel, which, before it was exhauſted, was thought to be repleniſhed with air alone. This, I ſay, I ſhall not ſollicitouſly obſerve, becauſe I think I need not; for I little doubt, the thing will be obſerved and laid hold of, both by the *Carteſians* and *Epicurians*; the former of which will endeavour thereby to eſtabliſh the neceſſity of their *Materia ſubtilis*, to maintain the plenitude of the world, and the circle they attribute to moving bodies; and the latter will here triumphantly pretend to have a more illuſtrious inſtance than ever, of their *vacuum coacervatum* within the world, ſince there is an impenetrable veſſel, out of which it is manifeſt, that an almoſt incredible proportion of aerial ſubſtance hath been manifeſtly made to iſſue; whereas it is no ways manifeſt to any of our ſenſes, that any other body has got in to ſucceed in its room: wherefore leaving them to debate, what it is, that is contained in that far greateſt part of the veſſel, that the air pumped out of our receiver has deſerted, I take notice,

SIXTHLY, That to conclude with what was the main drift of this, and the foregoing papers, we are here invited to obſerve, with wonder, the ſtupendous mutability of the air, as to rarity and denſity, whereby the ſame quantity of air, being ſometimes compreſſed, ſometimes dilated, may change its dimenſions to a degree, that ſeems almoſt to tranſcend the power of nature and art, and by conſequence might probably be rejected as incredible, if it were abruptly and nakedly propoſed: and therefore it will be convenient to do, though very briefly, theſe two things.

FIRST, To conſider, what we have upon experience delivered in our defence againſt the learned *Linus*, touching the condenſation and rarefaction of the air, as it is expoſed to a greater or ſmaller preſſure; without the intervention of either external heat or elaborate engines. For from theſe experiments, (that may be found in the lately mentioned defence*) eminent mathematicians have inferred, that one can ſcarce ſafely put determinate limits to the ſtupendous rarity, which the upper part of the atmoſphere, being almoſt totally uncompreſſed by incumbent particles of air, may be ſuppoſed to have by nature, un-aſſiſted by art.

AND this is the firſt of the two things, I above deſired to have taken notice of. But the other (which though it be but the ſecond, is much the more conſiderable) is to confer together the ſmalleſt extent; to which we have reduced it by condenſation, and the greateſt, to which we have advanced it by rarefaction, after having taken notice, that according to the leaſt eſtimate of any recited in the foregoing experiments, the extenſion of the ſame air, is as 1 to 2744, or thereabouts; and if inſtead of the moderateſt, we take the greateſt expanſion of the air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncompreſſed air was highly rarified, that number being multiplied by 40, becauſe of the fore-mentioned compreſſion of the air, will amount to 520000, for the number of times, by which the air at one time exceeds the ſame portion of air at another time; which is a difference of expanſion ſo great, that I hope it will keep you from thinking the title of the foregoing epiſtle, where the expanſion of the air is called admirable, immodeſt, eſpecially ſince I have forborn to mention, what probable arguments might be offered, to prove it at leaſt poſſible, that the induſtry of men, and perhaps our own, may find means to make both the condenſation and rarefaction of the air to exceed the uttermoſt, whereto we have yet been able to bring them.

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* Chap. V. Whoſe Title is, Two New Experiments touching the Meaſure of the Force of the Spring of the Air compreſſed and dilated.

POSTSCRIPT.

Touching an Obſervation to be inſerted above, (Page 208) *immediately after the Mark *.*

SINCE the writing of this, the author chanced to find one of the lately mentioned inſtruments of a conſiderable bigneſs, which was preſumed to have miſcarried; and comparing it with a memorial made, when it was firſt compleated, to keep in memory the heights, dimenſions, &c. of the incloſed mercury and air; we found, that in about ten weeks, there was not any conſiderable variation of them; and the little ſhrinking of the air, which was diſcoverable by an attentive eye, was not ſuch, but that it might be probably aſcribed to the change of the weather to a far greater coldneſs which might be ſuppoſed, a little (and it did it but very little) to weaken the ſpring of the included air, and conſequently abate of its full reſiſtance to the preſſure of the mercury in the longer leg of the ſyphon.

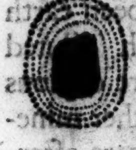
◆

AN OBSERVATION OF A SPOT IN THE SUN.

First Printed in the PHILOSOPHICAL TRANSACTIONS,
Nº 74, p. 2216, for *April* the 27th, 1671.

Friday, April 27, 1660.

"ABOUT eight of the clock in the morning, there appeared a ſpot in the lower limb of the ſun, a little towards the ſouth of its æquator, which was entered about $\frac{1}{40}$ of the diameter of the ſun, itſelf being about $\frac{1}{160}$, in its ſhorteſt diameter, of that of the ſun; its longeſt, about $\frac{1}{40}$ of the ſame. It diſappeared upon Wedneſday morning, May 9th, though we ſaw it the day before, about ten in the morning, to be near about the ſame diſtance from the weſtward limb a little ſouth of its æquator, that it firſt appeared to be from the eaſtward-limb, a little ſouth alſo of its æquator. It ſeemed to move faſter in the middle of the ſun than towards the limb. It was a very dark ſpot almoſt of a quadrangular form, and was incloſed round with a kind of duſkiſh cloud, much in this form, and in this proportion to the ſpot.



"WE firſt obſerved this very ſame ſpot, both for figure, colour and bulk, to be re-entered the ſun, May 25th, when it ſeemed to be in a part of the ſame line it had formerly traced; and was entered about $\frac{4}{33}$ of its diameter about ſeven of the clock in the afternoon. At the ſame time there appeared another ſpot, which was juſt entered, and appeared to be entered not above $\frac{1}{132}$ part of the ſun's diameter. It appeared to be longeſt towards the north and ſouth, and ſhorteſt towards the eaſt and weſt. There ſeemed to be diſperſed about it divers ſmall clouds here and there."

[THESE obſervations were made, as the noble obſerver told us, with an excellent teleſcope, in the preſence of divers curious and ingenious perſons, one of whom was Mr. *Hook*. And diſcourſing of thoughts he had entertained touching the effects of ſuch ſpots, he ſuggeſted this inquiry, whether they might not cauſe a conſiderable alteration both in the body of the ſun itſelf, and in our air, and the bodies in it upon their diſſipation?]

AN ESSAY ABOUT THE ORIGIN AND VIRTUES OF GEMS.

Wherein are propoſed and hiſtorically illuſtrated ſome Conjectures about the Conſiſtence of the Matter of Precious Stones, and the Subjects, wherein their chiefeſt Virtues reſide.

The PUBLISHER *to the* READER.

THE philoſophy and origin of gems, as well as their uſefulneſs and virtues, will, I am perſuaded, be found, upon the attentive peruſal of this eſſay itſelf, ſo rationally and warily delivered therein, that there will need nothing to be ſaid in the praiſe of the compoſure thereof. I dare venture, notwithſtanding the noble author's modeſty, to preſent it to the moſt critical taſte, without hanging out a buſh to it.

All I have to ſay in the publiſhing thereof ſhall be the ſame, that was alledged by the Engliſh interpreter of the learned *Steno*'s Prodromus to an intended diſſertation of his, concerning ſolids naturally contained within ſolids, printed the laſt year by *Moſes Pitt* in *Little Britain*; where in the Engliſh preface occur paſſages to this effect, *viz.*

“That the honourable author of this eſſay, before he would ſee or hear any thing of that Prodromus of *Steno*, did upon occaſion ſolemnly declare to the author of that Engliſh verſion (who there proteſts, that he ſpeaks it *bona fide*,) the ſum and ſubſtance of what is deduced at large in this tract; the manuſcript whereof the ſaid interpreter then ſaw, and received it into his cuſtody for publication: which ſum was this; firſt, that the generality of tranſparent gems have been once liquid ſubſtances, and many of them, whilſt they were either fluid, or at leaſt ſoft, have been imbued with mineral tinctures, that con-coagulated with them; whence he conceives, that divers of the real qualities and virtues of gems may be probably derived.

“Secondly, as for the opacous gems, and other medical ſtones, as blood-ſtones, jaſpers, magnets, emery, &c. he eſteems them to have, for the moſt part, been earth (perhaps in ſome caſes very much diluted and ſoft) impregnated with the more copious proportion of fine metalline or other mineral juices or particles, all which were afterwards reduced into the form of ſtone by the ſupervenience (or the exalted action) of ſome already inexiſtent petreſcent liquor, or petrific ſpirit, which he ſuppoſeth may ſometimes aſcend in the form of ſteams; from whence may be probably deduced, not only divers of the medical virtues of ſuch ſtones, but ſome of their other qualities, as colour, weight, &c. and alſo explained, how it may happen, what he hath (which he doubts not but others have done alſo) obſerved of ſtones of another kind, or marchaſites, or even vegetable and animal ſubſtances, that have been found incloſed in ſolid ſtones; foraſmuch as theſe ſubſtances may eaſily be conceived to have been lodged in the earth, whilſt it was but mineral earth or mud, and afterwards to have been, as it were, caſed up by the ſupervenient petrific agents, that pervaded it.

“Nor are theſe petreſcent liquors the only ones, to which he ſuppoſes, that many foſ-

"ſils may owe their origin, ſince he thinks, "there may be both metalleſcent and minera-"leſcent juices in the bowels of the earth, and "that ſometimes they may there exiſt and ope-"rate under the ſame ſpirits and ſteams."

So far the preface to that tranſlation; which is here repeated, to do right to this noble author, in the matter of the theory relating to the origin both of precious and other Stones. Which done, I ſhall keep the curious reader no longer from the contentment, which he will doubtleſs find in the peruſal of this eſſay.

The PREFACE.

THAT the ſcarcity, the luſtre, and preciousneſs of gems have made them in all ages to be reckoned among the fineſt and choiceſt of nature's productions, is generally granted. But whether the books, that have been divulged of them, be anſwerable to the nobleneſs of the ſubject, ſeems not to me ſo unqueſtionable. For, as for the origin of gems; to ſay with *Ariſtotle*, towards the cloſe of his third book of *Meteors*, that a dry exhalation, ξηρὰ ἀναθυμίασις, (whether) fiery or firing, (ἐκπυροῦσα,) makes, among other foſſils, the ſeveral kinds of unfuſible ſtones: or to tell us, according to the more received doctrine, that gems are made of earth and water finely incorporated and hardened by cold; this, I ſay, is to put us off with too remote and indefinite generalities, and to found an explication upon principles, which are partly precarious, and partly inſufficient, and perhaps alſo untrue. And as to the hiſtory of gems, that has been ſo fabulouſly delivered, that eſpecially among the moderns, many learned men, philoſophers and phyſicians, have, for the ſake of ſo many improbable, and ſometimes impoſſible virtues, that have been aſcribed to gems, been induced to deny them any virtues at all. It is true, that I am not altogether ſo ſevere, and that the eſteem, that I find made by learned men of the inquiſitive emperor *Rudolfus*'s phyſician *Boetius de Boot*, makes me diſcriminate him and two or three modern authors, that in books profeſſedly made on other ſubjects, have written incidentally of ſome gems, from ſuch notoriouſly fabulous writers, as *Mizaldus*, *Albertus Magnus*, (if his name be not injured by the imputation of a ſpurious book) *Baptiſta Porta*, *Kirannides* (and ſome others, that I forbear to name) from whoſe learning one would expect more warineſs and judgment. But though, for reaſons elſewhere mentioned, I do not unreſervedly think, that precious ſtones, eſpecially opacous ones, can have no medical virtues at all; yet when I conſidered, how difficult it was to aſſign any thing, that is poſſible and intelligible, (which I do not take a ſubſtantial form to be,) whence their virtues may probably be derived, without giving ſome ſuch account of the origin of gems themſelves, as was not to be expected from the followers of the peripatetick, that is, the received philoſophy; I could not but wiſh, that ſomething were attempted on that ſubject according to the principles of the Corpuſcularian.

THESE things made me the leſs backward to comply with the curioſity of my friends, which put me upon the following diſcourſe, wherein I was content to try, what, without ranſacking the authors, that had profeſſedly written *de Gemmis*, the conſideration of the ſubject to be treated of, my natural propenſity to take notice of nature's productions, and the trials whereto theſe conſiderations and obſervations lead me, would ſuggeſt to my pen.

WHETHER my conjectures and ratiocinations be as new to others, as to thoſe I chiefly wrote for, it is not my part to determine: only I deſigned to ſuit my diſcourſe to the phænomena of nature, without being ſollicitous, with whom I diſagreed or complied. And therefore, though it ſhould happen, that ſome conjectures of mine ſhould, unknown to me, be coincident with the opinion of ſome claſſic writer about gems; yet I preſume, the whole ſubſequent hypotheſis, and the arguments it is founded upon, will appear to have been ſuggeſted to me by the nature of the thing itſelf, and my way of conſidering it: not to mention, that ſometimes one may meet with a good particular conjecture in an author, that underſtands not the importance of it himſelf, and knows not how to make uſe of it, but builds it on ſome ſuch fabulous relation, or erroneous principle, as is apt to diſcredit it with wary readers, unleſs they be ſuch, to whom its compliance with the opinions, they have on better grounds already entertained, happen to recommend it. I know, it may be thought ſtrange, that I have been ſo very ſparing in the citation of thoſe authors, that have writ whole books about gems; but I have this to ſay for myſelf, that I had neither them, nor ſo much as my own papers about the origin of minerals at hand, when I writ the following eſſay. Which I was the leſs troubled at, upon two diſtinct accounts; the firſt, becauſe I remembered, that ſeveral paſſages, that I had met with about the virtues of gems, cited out of divers of thoſe authors, were ſuch, as I ſhould have much ſcrupled to vouch; ſome of them being ſuch as I knew to be falſe; others, that I ſhrewdly ſuſpected not to be true; and others, that appeared to me altogether incredible: and the ſecond, becauſe, to forbear tranſcribing what my friends might probably have met with in authors already, would beſt comply, both with their deſires, which was to know my particular thoughts; and with my deſign, which was partly to ſee, how far I could make out thoſe thoughts, by my own arguments and obſervations, aſſiſted only by ſome very few hiſtorical paſſages, that I lighted on in writers not claſſic; and partly, to take this occaſion to proſecute divers matters of fact relating to the ſubject I was treating of, which probably would otherwiſe have been quite loſt. And I doubted not, but if this firſt draught of my conceptions were by my friends thought worthy of being inlarged, it would not be difficult for me, when I ſhould come at my books and papers again, to inrich this tract with many hiſtories borrowed from famous writers; if that ſhould be thought neceſſary by perſons, that were poſſibly leſs diffident of me than of them. In ſhort, I propoſed this diſcourſe but as a conjectural hypotheſis, wherein I attempted to derive the origin of gems, and one of the main cauſes, (I do not ſay, the only cauſe) of their qualities and virtues, from principles leſs remote, and more intelligible than thoſe of the peripateticks; and having delivered divers obſervations and experiments of my own about the phænomena of gems, to explicate ſome of them by intelligible principles, and illuſtrate others by reſembling things, that may be really obſerved in nature, or eaſily performed by art. Which way of handling my ſubject permitted me to hope, that, whether or no I ſhould be thought a lucky conjecturer about the ſubject I attempted, I ſhould, at leaſt in ſome meaſure, prove a benefactor to what is, perhaps, preferable even to lucky conjectures themſelves, the natural and experimental hiſtory of ſuch noble ſubjects as gems.

AN
ESSAY
ABOUT THE
ORIGIN AND VIRTUES
OF
GEMS.

SECTION I.

THOUGH it will not perchance prove very difficult to propoſe to you my conjecture about the cauſes of the virtues of precious ſtones; yet I fear it will not be eaſy for me to acquaint you fully with the grounds of it. For unleſs I ſhould tranſcribe for you my whole diſcourſe of the origin of minerals in general (of which you know ſtones make a part) I cannot well lay before you all the conſiderations, by which I have been induced to take up the conjecture or hypotheſis I am about to propound: and conſequently, I cannot well comply with your curioſity about gems, without either omitting ſeveral things, which might much countenance the following diſcourſe, or propoſing (without amply proving them,) ſome things, that I confeſs ſeem not clear, nor ſome of them ſo much as probable, by their own light. But ſince you will have it ſo; I will, rather than diſobey you, preſent you in one diſcourſe ſeveral things concerning gems, whereof ſome belong to others of my little tracts about the origin of minerals from fluid, or at leaſt ſoft bodies; though ſome indeed were more directly written concerning gems: notwithſtanding that they were delivered not as an entire tract about that ſubject, but as corollaries, that might be drawn from, and applications, that might be made of, what had been in a more general way diſcourſed about the origination of ſtones and other minerals. And therefore preſuming, that you will ſuppoſe with me in this diſcourſe ſome few particulars, that, I think, I have elſewhere made probable, and might perhaps do ſo from ſome of the phænomena mentioned in this writing itſelf, I would immediately addreſs myſelf to the ſubject of it, if I did not think a previous admonition very requiſite.

FOR, I muſt, at the very entrance of this diſcourſe, deſire you to take notice, that when I propoſe my conjectures about the virtues of gems, I do not ſuppoſe the truth of all, or ſo much as the tenth part of thoſe wonderful properties, that men have been pleaſed to aſcribe to them. For not only ſome of the writers of natural magick, but men of note, who ſhould be more cautious and ſober, have delivered in their writings many things concerning gems, which are ſo unfit to be credited, and ſome of them perhaps ſo impoſſible to be true, that I hope the believers of them will, among the votaries to philoſophy, be as great rarities, as gems themſelves are among ſtones.

And thoſe, that can admit ſuch unlikely fables, will be as much deſpiſed by the judicious, as jewels can be prized by the rich.

For my part, I never ſaw any great feats performed by thoſe hard and coſtly ſtones, (as diamonds, rubies, ſaphires,) that are wont to be worn in rings. But yet, becauſe phyſicians have for ſo many ages thought fit to receive the fragments of precious ſtones into ſome of their moſt celebrated cordial compoſitions; becauſe alſo divers eminent men of that profeſſion, ſome of them famous writers, and ſome virtuoſi of my own acquaintance, have, by their writings, or by word of mouth, informed me of very conſiderable effects of ſome gems, (eſpecially cryſtal,) upon their own particular obſervations; and laſtly, becauſe, that (as I ſhall ſhew anon) I find no impoſſibility, that at leaſt ſome coſtly, and leſs hard, (though indeed more valuable) gems, may have conſiderable operations upon human bodies, ſome few of which I have had opportunity to be convinced of; I will not indiſcriminately reject all the medicinal virtues, that tradition and the writers about precious ſtones have aſcribed to thoſe noble minerals; contenting myſelf to declare in a word, that ſuſpecting moſt of them to be fabulous, my conjectures aim only at giving one of the cauſes of thoſe virtues aſcribed to gems, which experience warrants to be real and true.

Having thus explained in what ſenſe my conjecture about the virtues of precious ſtones is to be underſtood; it follows, that I propoſe the conjecture or hypotheſis itſelf; the ſubſtance of which may be comprized in theſe two particulars: firſt, that many of theſe gems, and medical ſtones, either were once fluid bodies, as the tranſparent ones; or in part made up of ſuch ſubſtances, as were once fluid: and ſecondly, that many of the real virtues of ſuch ſtones may be probably derived from the mixture of metalline and other mineral ſubſtances, which (though unſuſpectedly) are uſually incorporated with them: and the greatneſs of the variety and efficacy of thoſe virtues, may be attributed to ſome happy concurrent circumſtances of that commixture. The firſt of theſe heads relates properly to the origin of gems. The ſecond, partly to that, and partly to the kinds and degrees of their virtues.

But that any gems, eſpecially the hardeſt ſorts of them, ſhould have a later beginning, than that of the earth itſelf, will probably be thought to reliſh of a paradox; and I doubt not, it will paſs with many for a great one, that ſome of theſe hardeſt of ſolid bodies ſhould have been once fluid ones, or liquors: wherefore I ſhall endeavour to countenance this hypotheſis by the following conſiderations.

1. And firſt the diaphaneity of diamonds, rubies, ſaphires, and many other gems, agrees very well with this conjecture, and thereby ſeems to favour it. For it is not ſo likely, that bodies, that were never fluid, ſhould have that arrangement of their conſtituent parts, that is requiſite to tranſparency, as thoſe that were once in a liquid form, during which it was eaſy for the beams of light to make themſelves paſſages every way, and diſpoſe the ſolid corpuſcles after the manner requiſite to the the conſtitution of a tranſparent body. Therefore we ſee, that ſilver in aqua fortis, or lead in ſpirit of vineger, having by that ſolution had their particles reduced into a fluid form, thoſe particles, though before opacous, are ſo diſpoſed of, as to make, not only a diaphanous ſolution, but, if one pleaſes, tranſparent cryſtals. And what chemiſts uſually try with thoſe metals, I have had the curioſity to try with ſeveral ſtones, which I may hereafter have occaſion to name to you. But this argument I bring rather to confirm than evince my conjecture.

Secondly, the origin aſſigned to gems may be alſo countenanced by the external figuration of divers of them. For we plainly ſee, that the corpuſcles of nitre, allom, vitriol, and even common ſalt, being ſuffered to coagulate in the liquors they ſwam in before, will convene into cryſtals of curious and determinate ſhapes. And the like I have tried in ſeveral metalline bodies diſſolved in ſeveral menſtruums. But unleſs a concreting ſtone, or other like body, be either ſurrounded with, or in good part contiguous to a fluid, it is not eaſy to conceive, how it ſhould acquire a curious angular and determinate ſhape. For concreſcent bodies, as I may ſo ſpeak, if they have not room enough in an ambient fluid for the moſt congruous ranging of their parts, cannot caſt themſelves into fine and regular ſhapes, ſuch as I ſhall preſently ſhow, that divers gems ſeems to affect; but the matter they conſiſt of muſt conform to the figures of the cavity, that contains it, and which in this caſe has not ſo much the nature of a womb, as of a mold. And ſo we ſee, that ſalt-petre, and divers other ſalts, if the water, they were diſſolved in, be much too far boiled away before they are ſuffered to ſhoot, will, if the liquor fill the glaſs, ſometimes coagulate into a maſs, faſhioned like the inſide of the containing veſſel, or if a pretty quantity of liquor remains after the coagulation, that part of the nitrous maſs, that was reduced to be concreted next the glaſs, will have the ſhape of the internal ſurface of it, whatever that be; but thoſe cryſtals, that are contiguous to the remaining liquor, having a fluid ambient to ſhoot in, will have thoſe parts of their bodies, that are contiguous to the liquor, curiouſly formed into ſuch priſmatical ſhapes as are proper to nitre.

To apply this now to gems; that divers kinds of them have geometrical and determinate ſhapes, though it be not vulgarly obſerved, becauſe we are wont to ſee them when they are cut, if not alſo ſet in rings and jewels; yet I have often had the opportunity to take notice of it, by having had the curioſity to look upon many of them rough as nature has produced them, and the good fortune to take divers of them out of their wombs. For I remember, I have taken a good number of Indian granats out of a lump of heterogeneous matter, whoſe diſtinct cavities, like ſo many cells, contained ſtones, on ſome of whoſe ſurfaces you might ſee triangles, parallelograms, &c. And being once near the rock, whence thoſe ſtones are chiefly fetched, that are commonly called Briſtol-ſtones, I remember, I rid thither and procured a workman or two to dig me up a number of them, divers of which I found to be curiouſly and determinately ſhaped, much like ſome cryſtals of nitre, that I have taken pleaſure to compare with them. And the like figuration I have alſo obſerved in divers Corniſh diamonds, and in a fair and large one, which one, that knew not what it was, found growing, with many leſſer, in *Ireland*, and preſented me. And to let you ſee, that it is not only in theſe ſofter gems, that this curious figuration is to be met with, I ſhall add, that I found among many ſtones I had, and took to be rubies (and thoſe the jewellers will tell you are exceeding hard) a conſiderable number, whoſe ſhapes, though not the ſame with thoſe of the Corniſh and Iriſh ſtones, were yet fine and geometrical. And the like I have obſerved even in thoſe hardeſt of bodies, diamonds themſelves; of which remembering, that in my collection of minerals, I had a pretty large one, that was rough, I perceived, that the ſurface of it conſiſted of ſeveral triangular planes, which were not exactly flat, but had, as it were, ſmaller triangles within them, that for the moſt part met at a point, and did ſeem to conſtitute, as it were, a very obtuſe ſolid angle: encouraged by this, I examined ſeveral other rough diamonds, and found the moſt of them to have angular and determinate ſhapes, not unlike that newly mentioned. And having thereupon conſulted an expert jeweller, that was alſo a traveller, though he could not name to me the ſhapes of the uncut diamonds, he had met with; yet he told me, he generally found them to be ſhaped like that I ſhewed him; inſomuch, that ſuch a ſhape was a mark, by which he uſually judged a ſtone to be a right diamond, if he had not the opportunity to examine it by the hardneſs.

AND this I ſhall add in favour of the compariſon, I lately intimated betwixt the coagulation of petre and that of gems, that having once made an odd menſtruum, wherein I was able to diſſolve ſome precious ſtones, there ſhot in the liquor, cryſtals pretty large, and ſo tranſparent and well ſhaped, that they might well have paſſed for cryſtals of nitre; and yet, if I much miſremember not, they were inſipid. And I have divers times taken notice in ſuch ſtones, as the Briſtol diamonds, that though that part, which may be looked upon as the upper part of the ſtone, were curiouſly ſhaped, having ſix ſmooth ſides, which at the top were, as it were, cut off ſloping, ſo as to make ſix triangles, that terminated like thoſe of a pyramid in a vertex; yet that, which may be looked upon as the root or lower part of the ſtone, was much leſs tranſparent (if not opacous) and devoid of any figuration; of which the reaſon ſeems to be, that this being the part whereby the ſtone adhered to its womb, it was ſullied by the muddineſs of it, and reduced to conform it ſelf to whatever ſhape the contiguous part of the cavity chanced to be of; whereas the upper part of the ſtone was not only formed of the clearer part of the lapideſcent juice before the wateriſh vehicle was exhaled, but had room and opportunity to ſhoot into the curious figure belonging to its nature. And this is much more conſpicuous, where many of theſe cryſtals grow, as it were, in cluſters out of one mineral cake or lump; as I have ſeen not only in thoſe ſoft, but yet tranſparent concretions, which ſome of the later mineraliſts (for the ancients ſeem ſcarce to have known them) call fluores, and particularly in a very fine mineral lump, that I had once the honour to have ſhewed me by a great prince, and no leſs great a virtuoſo, to whom it was then newly preſented. For this maſs conſiſted of two flat parallel cakes, that ſeem compoſed of a dirty kind of cryſtalline ſubſtance, and out of each cake there grew, towards the other, a great number of ſtones, ſome of which, by their coheſion, kept the two cakes together; and moſt of theſe ſtones, having each of them a little void ſpace about it, wherein it had room to ſhoot regularly, were geometrically ſhaped, and, which looked very prettily, were coloured like a German amethiſt. And I have my ſelf a pretty large ſtone, taken up here in *England* by a gentleman of my acquaintance, which conſiſts, as it were, of four parts; the lowermoſt is a thin and broad flake of coarſe ſtone, only adorned here and there with very minute gliſtering particles, as if they were, (as probably they may be) of a metalline nature; over this is ſpread another thin white, but opacous, bed, which is ſo incloſed between the firſt named bed, and the two others, that, without defacing the ſtone, I cannot well examine it: the third conſiſts of a congeries of minute cryſtals exceedingly thick ſet, which therefore look whitiſh, having little or no tincture of their own; and this part, no more than either of the former, is not much thicker than a barley-corn. The fourth and uppermoſt part, which, yet ſeems in great part to be the ſame cryſtals, which, as they grow higher and ſpread, acquire a deeper colour, is made up of a great number of amethiſts, ſome paler, and ſome highly tincted, which are of very differing figures, and bigneſſes, according (as one may gueſs) as they had conveniency to ſhoot; theſe at one end of the ſtone lying in a flat bed, as it were, and ſcarce exceeding a barley corn in length; whereas thoſe at the other end ſhoot up to a good height into figured cryſtals, ſome of them as big as the top of my little finger, and thoſe are the moſt deeply coloured, being alſo of a good hardneſs, ſince I found, that they would eaſily grave lines upon glaſs.

I remember alſo, that going to viſit a famous quarry, that was not very far from a ſpring, which had ſomewhat of a petreſcent faculty in it, I cauſed divers ſolid pieces of rough and opacous ſtones to be broken, out of hope I had to find in them ſome finer juice coagulated into ſome finer ſubſtances; and accordingly I found, that in divers places, the ſolid and maſſy ſtone had cavities in it, within which, all about the ſides, there grew concretions

tions, which by being tranſparent, like cryſtals, and very curiouſly ſhaped, ſeemed to have been ſome finer lapideſcent juice, that by a kind of percolation through the ſubſtance, that groſſer ſtone was made of, had at length arrived at thoſe cavities, and upon the evaporation of the ſuperfluous and aqueous parts, or by their being ſoaked up by the neighbouring ſtone, had opportunity to ſhoot into theſe fine cryſtals, which were ſo numerous, as quite to overlay the ſides of the cavities, as I can ſhew you in ſome large cluſters of them, that I brought from thence. And enquiring of an ancient digger, whether he had not ſometimes met with greater quantity of them? he told me, that he had, and preſented me a great lump or maſs made up of a numerous congeries of ſoft cryſtals, (but nothing ſo colourleſs as theſe other newly mentioned) ſticking to one another, but not any of them to any part of the rock: ſo that they ſeemed to have been haſtily coagulated in ſome cleft or cavity, as it were in a mold, where meeting and mingling before concretion, with ſome looſe particles of clay, the maſs may thereby be diſcoloured.

OUR argument, drawn from the figuration of tranſparent ſtones, may be much ſtrengthened by the coalition I have ſometimes obſerved of two or more of ſuch ſtones, and the congruity in the ſhape of ſome of them to the figures of thoſe parts of the others, that were contiguous to them, and ſeemed to have been formed after them. But though this phænomenon be conſiderable to the ſcope of my diſcourſe, yet perceiving, that I ſhall have occaſion to inſiſt on it hereafter, I ſhall not do it now.

THIRDLY, nor is it only the external figuration of theſe gems, but the internal texture, that favours our hypotheſis, ſome of them ſeeming much to imitate in their coagulation ſeveral of thoſe ſubſtances, which I have obſerved to have once been fluid. That common ſalt may be made up of ſmall ſaline particles, that by a convenient juxta-poſition may be aſſociated into great lumps, divers of which are cubically ſhaped, is an obſervation eaſy enough to be made. And that ſuch coalitions of particles may conſtitute ſolid and conſiderably hard bodies, I have tried by breaking ſome of the larger cubes of ſal gem, and the lumps of the iſle of *Mayo* ſalt, whereof the firſt is foſſil, the other marine, and both natural. I have likewiſe found by trial, that, though ſilver diſſolved in aqua fortis appears uſually to ſhoot, if it be taken notice of, into flat and exceeding thin flakes; yet it is very poſſible ſo to order the coagulation, that many of theſe thin plates ſhall, in their convention, have their flat ſides ſo placed over one another, as to make up pretty large and thick cryſtals, whoſe very outſides will be finely ſhaped, as being ſome peculiar kind of vitriol. Nor are theſe the only fluid bodies, which I have reduced to coagulate into conventions, of ſuch a flaky texture; wherefore I began to ſuſpect, that divers tranſparent minerals may have the like; and in ſome diaphanous kinds of talk, whoſe outſides were mathematically figured, I found encouragement to try, whether even ſome gems themſelves, notwithſtanding their hardneſs, might not have ſuch an internal figuration. Nor was I deterred by conſidering, that it is taken for granted, that gems are of an uniform texture, and that there muſt be a ſtrange thinneſs in the plates, that make up tranſparent ſtones, ſince no ſuch thing has been noted by the moſt curious eye, but men have taken it for granted, that the texture of all gems is uniform, without any grain or fibres, no more than there is in gold. But as to the thinneſs of the plates, I remember, I have ſeveral times taken pleaſure to hold a piece of good Muſcovia glaſs againſt the light, when it was of ſuch a thinneſs, that the ſpectators, though provoked to look with curious eyes, could ſcarce ſee the plate itſelf, and would by no means be brought to think, that it was poſſible to ſplit it, till I did actually do it; and ſometimes I then ſubdivided it beyond even my own expectation. But to examine this conjecture, I took ſome ſtones, that had geometrical figures on part of their ſurfaces, and which I had other grounds to think to have been once fluid ſubſtances, and having diligently ſurveyed ſome of them, which ſeemed likelieſt to give me ſatisfaction, I manifeſtly enough perceived, not only with my aſſiſted, but with my naked eyes, divers parallel commiſſures, which ſeemed plainly to be made by the contiguous edges of little thin plates of ſtone, that appeared to lie one over another, almoſt like the leaves of a book, that is a little opened.

I remember, that holding a large and rough grizolette (as artificers call hard gems, of a blueiſh colour, brought them from *Eaſt-India*,) againſt the light, and curiouſly obſerving it, I have ſometimes diſcerned a grain, as they call it, in the ſtone, and was anſwered by a ſkilful artiſt, that uſed to make ſeals of them, that ſuch ſtones would uſually ſplit, according to the ductus of their grain. I will not urge, that in ſome other precious ſtones, that were cut and poliſhed, as particularly the hyacinth, and even the ſaphire, by obverting them ſeveral ways to the light, I have been able to obſerve, as it were, commiſſures, which were ſo fine, as not to hinder, or call in queſtion, the intireneſs of the ſtone, for the lapidary's purpoſe. This, I ſay, I forbear inſiſting on, becauſe the phænomenon is far leſs conſiderable, than what I have ſeveral times obſerved in New-Engliſh granats, wherein, eſpecially when they are broken, the edges and commiſſures of the thin plates or flakes, whereof they conſiſted, were very eaſily diſcernable. And to try, whether this obſervation would hold even in the hardeſt ſtones, I had recourſe to a pretty big diamond unwrought, which being placed in a miſcroſcope, ſhewed me the commiſſures of the flakes I looked for, whoſe edges were not ſo exactly diſpoſed into a plain, but that ſome of them were ſenſibly extant like little ridges, but broad at the top above the level of the reſt. And theſe parallel flakes together with their commiſſures, I could in a ſomewhat large diamond plainly enough diſcern even with my unaſſiſted eyes. And for further ſatisfaction, I went to a couple of perſons, whereof the one was an eminent jeweller, and the other an artificer, whoſe trade was to cut and poliſh diamonds, and they both aſſured me upon their repeated and conſtant experience, and as a known thing in their art, that it was almoſt impoſſible, (though not to break, yet) to ſplit diamonds, or cleave them ſmoothly croſs the grain, if I may ſo ſpeak, but not very difficult to do it at one ſtroke with a ſteeled tool, when once they had found out from what part of the ſtone, and towards what part the ſplitting inſtrument was to be impelled: by which it is evident, that diamonds themſelves have a grain, or a flaky contexture, not unlike the fiſſility, as the ſchools call it, in wood; which you will eaſily grant to conſiſt of aſſimilated water or juices; which having been once fluid bodies, were fit to have their particles ſo ranged or diſpoſed, as to conſtitute a body far more eaſy to be cleft according to the ductus of the fibres, or planes, than otherwiſe. And I remember, that having, as I thought, obſerved in a rough diamond, which I purpoſely examined, that the flakes, whoſe edges were terminated in one plain, were far enough from being parallel to thoſe, whoſe edges compoſed another plain, (I ſpeak of phyſical planes of the ſame ſtone,) I imagined, that if this diamond were to be cleft, it would not be ſmoothly ſplit into two pieces, becauſe the commiſſures did probably make angles in the body of the ſtone; and accordingly I learned of the ancienteſt of theſe diamond cutters, that ſometimes he met with ſtones, that eluded all his ſkill, and would by no means be ſplit like others into two parts, but, before they were cleft quite through, would break in pieces; which was a defect in the ſtone he could not certainly foreſee, but was fain to learn from the unwelcome event.

FOURTHLY, it ſeems not unprobable, that the colours of divers gems, (for I do not ſay of all,) are adventitious, and were imparted to them, either by ſome coloured mineral juice, or ſome tinging mineral exhalation, whilſt the gem or medical ſtone was either in *ſolutis principiis*, or of a texture open enough to be penetrable by mineral fumes. Which argument's conſiderableneſs makes me hold it unfit to be lightly touched in this place; though I cannot diſcourſe any thing fully of it in few words, becauſe it not only ſuggeſts divers obſervations and other particulars, but requires alſo the mention of ſome of the chief of them; which therefore I ſhall now ſubjoin.

1. AND the firſt ſhall be, that many gems, not to ſay almoſt all of them, have been obſerved to be deprived of their colour, if having fallen, or been put into the fire they have lain too long there; inſomuch, that I have found it affirmed upon the teſtimony of the learned and experienced *Boetius de Boot*, that all gems will loſe their colour in the fire, except Bohemian granats. How far this may be true, I have not had opportunity thoroughly to examine. But I well remember, that having porpoſely expoſed divers gems to the fire, though that were but moderate, and had a crucible interpoſed between it and them, ſome of them ſeemed to have their tincture much impaired, and others quite deſtroyed. But I muſt be ſo free as to admoniſh you, that if theſe trials be not warily made, they may eaſily impoſe upon us; eſpecially if we do not conſider the nature and cauſe of whiteneſs. For any diaphanous body, as far as I have yet obſerved, being divided into a multitude of very minute parts, and conſequently acquiring a multitude of diſtinct ſuperficies, which do briſkly reflect the light every way outwards, will appear to have a white colour, that will be more or leſs vivid, as the particles are more or leſs numerous, minute, and otherwiſe fitted to ſcatter the incident beams of light; as you may ſee by reducing to powder fine Venice-glaſs, which will be white; and even red ink, if ſo ſhaken or beaten as to be brought to a froth, conſiſting of many minute bubbles, will ſeem to have put on a whiteneſs. So, that if by too haſty an ignition, or too haſty a cooling of the fired gems, they come to be flawed with innumerable little cracks, they may be thought to be made white, by having their tincture driven away, when their whiteneſs really proceeds from the multitude of thoſe little flaws which are ſingly unperceived; and the rather, becauſe the body may ſtill retain its former ſhape or ſeeming intireneſs. To illuſtrate which, I have ſometimes taken pleaſure to heat a piece of cryſtal red hot in a crucible, and then quench it in cold water; for even when the parts did not fly or fall aſunder, but the body retained its former ſhape, the multitude of little cracks, that were by this operation produced in it, made it quite loſe its tranſparency, and appear a white body. In making which experiment, the multitude of produced flaws may be pretty well diſcovered to the incredulous, if, as I have ſometimes done, the ignited cryſtal be warily and dextrouſly quenched not in water, but in a very deep ſolution of cochineel made with ſpirit of wine, in which operation, if it be well performed, but not otherwiſe, enough of the red particles of the ſolution will get into the cracks of the cryſtal, to give it a pleaſing colour.

About Tincture of Coral.

THE other trials, that I have made about the reducing of whiteneſs or paleneſs in bodies, either tranſparent, or even ſemi-diaphanous only, belonging to another paper, I ſhall here forbear to mention them, having already ſaid enough for my preſent purpoſe, which is not ſo much to affirm poſitively, that no proof at all can be drawn from the operation of fire upon the colour of gems, as to make you cautious, what proofs drawn from thence you admit.

2. WHEREFORE declining to ſay any thing more about the firſt, I ſhall now proceed to the next circumſtance, that belongs to our argument, (which you may think to be more conſiderable than the former) namely, that the colours of ſeveral gems, when they are not deſtroyed by fire, will be altered thereby; which being a thing, that happens to divers foſſil pigments (of which ſome I employ to tinge glaſs) and other bodies confeſſedly mineral, argues a commixture of mineral ſubſtances in thoſe ſtones, whoſe colour receives ſome of the alte-

rations I ſpeak of: which laſt words I add, becauſe I would not impoſe upon you, by concealing, that there may be a great change of colour, produced by the fire, without any alteration of the tinging parts as ſuch. For by flawing the heated gem in very many parts, a degree of whiteneſs or paleneſs emerging thereupon may ſomewhat change the former colour. But this alteration being but a kind of dilution, is not that, which I here mean. For I remember, I have taken Indian granats, and having in a crucible expoſed them to the fire, I found they had exchanged their reddiſh colour for a dark and dirty one, like that of iron, that has been long kept in the air. And having taken ſome pieces of agate prettily enough adorned with waves of differing colours, and kept them a competent time (for they ſhould not be kept too long) in the fire, I found, as I conjectured, that the greateſt part of the agate ſeemed to be deprived of its tincture, being reduced to a pleaſant whiteneſs. But in ſome places, where there were ſtains of a different kind from the reſt, and where there ran little veins, that I gueſs to be of a metalline nature, there, I ſay, the colour was not deſtroyed, but changed, and the veins of pigment, thus coloured, acquired a deep redneſs, which they will retain, if let alone, though I was induced to think by ſome trials made on other pieces of Indian agate, that even theſe metalline tinctures were not ſo fixed, but, that a laſtinger fire would drive them away, and leave the ſtones partly white. Such a change of colour, as I lately mentioned in the veins of agate, is likewiſe found in thoſe of ſome other ſtones, as alſo in ſome pebbles, amongſt divers of which, that loſt only their tranſparency by ignition and extinction in water, one or two acquired ſo much deeper a colour, than it had before, that I thought it remarkable.

3. ANOTHER circumſtance, that ſeems to favour our conjecture, may be this, that it has been obſerved not unfrequently, that near many of the places, where coloured gems are found, ſome mines or veins of metals are to be met with. And I think it not unlikely, that if ſearch were ſkilfully made, many more diſcoveries would be made of veins either of metalline ore, or ſome other mineral, liquid or concreted, whence, by way of juices or fumes, the gems may be preſumed to have received tinctures. But uſually, where precious ſtones are found, men's induſtry and curioſity is too much confined to thoſe rich minerals, and does not make them ſolicitous to look after inferior ones. Beſides that in *Eaſt-India*, whoſe countries are beſt for the moſt gems, they are wonderfully unſkilful at digging mines; as I have gathered from the anſwers of ſome, who purpoſely went to viſit the diamond mines, as they call them. To this may be alſo referred, that gems are ſeveral times found in the metalline veins themſelves, or very near them; as I can ſhew you divers amethyſts, that an ingenious gentleman of my acquaintance took himſelf out of a piece of ground abounding with the ores of iron and tin, the latter of which was there plentifully dug up. And in thoſe colder countries, ſuch as *Germany* and *England*, where hard gems are more unfrequent, thoſe ſoft ones, that mineraliſts call fluores, are often to be found in, or near metalline veins, ſo finely tincted by mineral juices, that, were it not for their ſoftneſs, they might paſs, at leaſt among moſt men, for emeralds, rubies, ſapphires, &c. as I have been informed, not only by ſome mineral writers of good credit, but alſo by eye witneſſes, and partly by my own obſervation.

4. THE fourth circumſtance, which may be alledged to the ſame purpoſe with the three foregoing, is, that it ſeems poſſible, from ſome gems, by menſtruums, to obtain tinctures, that ſeem rather extractions, than diſſolutions, ſtrictly ſo called. I will not urge the chemical proceſſes, that may be met with in ſome authors to this effect; becauſe ſome circumſtances in the things, and in the writers, made me ſo far ſuſpect thoſe I could try, (and thoſe, that required undiſcovered menſtruums, as they may be true, ſo, for aught I know, they may not,) as to keep me from meddling with them. But I remember, I once made a menſtruum, (I ſay once, becauſe its preparation is ſo ſubject to caſualty, that I have often failed in it) which being poured upon well coloured granats, not only not calcined, but entire, was in no long time beautified with a high and lovely tincture, which was admired by very ſkilful perſons, to whom I ſhewed it, becauſe the menſtruum was not more corroſive than white wine; and which yet I therefore took to be a genuine tincture, partly becauſe it was drawn in the cold, partly becauſe the liquor would not tinge it ſelf by ſtanding, if no body were put in it; and partly becauſe it drew a tincture from antimony of a very differing colour from this we ſpeak of. Nor are granats the only gems, which I have made the liquor work on, in the cold.

5. To theſe four circumſtances I ſhall add this fifth, that ſome gems, which jewellers affirm, without ſcruple, to be rubies, ſapphires, &c. either are colourleſs, or have other colours, than thoſe, that are wont to belong to them. That famous goldſmith *Benvenuto Cellini*, in his little Italian tract of his own profeſſion, admoniſhes his reader, that there are one kind of rubies, that are naturally white, (and not made ſo by art) which he proves by the degrees of hardneſs peculiar to rubies. And the ſame author elſewhere tells us of beryls, topazes, and amethyſts, that are white. And it ſeems, by what he ſays not far from that place, that the Italian jewellers did not look upon the tinctures of gems, as any thing near ſo eſſential to them, as they are commonly reputed, ſince they reckon topazes and ſapphires, whereof one is blue and the other yellow, but both extremely hard in compariſon of other gems than diamonds (and perhaps rubies,) to be of the ſame ſpecies. The degree of hardneſs of rubies and ſapphires is oftentimes ſo equal, that I knew an expert Engliſh jeweller, who for that only reaſon (for he knew not whence the difference of colour might proceed) took rubies and ſapphires to be of the ſame kind of ſtone.

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AND that gems, referred by lapidaries to the ſame kind, may be very differingly tinged, is a truth, whereof I have ſeen notable inſtances in diamonds themſelves; which I therefore prefer to other inſtances, becauſe the extreme hardneſs of diamonds is ſuch, as keeps jewellers from miſtaking any other ſtone for a true diamond, if they are permitted to put them on their rapidly moved wheels employed to cut them. Now, of true diamonds I have ſeen ſome, that were yellowiſh; others, that were more yellow; and, among the reſt, one that was ſo perfectly yellow, that I at firſt took it for a fair topaz, though it were a diamond valued at near three pound weight of gold. I have alſo ſeen diamonds, and thoſe rough, as they came directly out of the *Indies*, and were ſoon after bought by traders in diamonds for ſuch, which were either blueiſh or greeniſh. And I particularly contemplated one ſtone, which, if its ſhape and other things had not convinced me of the contrary, was ſo green, that I ſhould have taken it for an emerald.

I remember I had once occaſion to buy a conſiderable number of ſmall rubies, divers of which were very curiouſly ſhaped; and coming to look upon the whole parcel more leiſurely, than my haſte would permit me when I bought it, I found in a great number of other ſtones, one, and but one, that was devoid of any colour; but in any other reſpects was ſo like the reſt, as invited me to conclude, that it would have encreaſed their number, but that it was coagulated and hardened before the mineral pigment had tinged it of the ſame colour with the reſt. In which gueſs I was confirmed, when, having met with a gentleman, who had been in the chief places of the *Eaſt-Indies*, where rubies are found, and particularly at the river of *Siam*, or *Pegu*, near which he lived a good while, and where he frequently ſaw rubies taken out of the bottom of the water, and ſometimes took them out himſelf; I learned of him by enquiry, that he had there ſeen ſeveral ſtones, each of which was partly a ruby, and partly colourleſs: and ſometimes in the ſame ſtone, there would be two portions of one ſort, and the third, though lying betwixt them, of another; which has frequently obliged the jewellers conſiderably to leſſen the bulk of ſuch ſtones, by cutting off the untincted part. And, if my memory do not much deceive me, I ſaw, in a great and curious prince's cabinet, among other rarities, a ring, in which was ſet a ſtone of a moderate bigneſs, whereof only one half, or thereabouts, was well tincted, the other being colourleſs. In gems, that are leſs precious, and not ſo tranſparent, eſpecially in agats, and in opacous gems, I could eaſily give a multitude of inſtances of the differingly tincted parts of the ſame entire ſtone. And I uſually wear in a ring a ſmall ſardonix, that was once a great prince's, wherein there are three portions, one within another, the uppermoſt black, the middlemoſt of a kind of cheſnut colour, the other of a blue, almoſt like a turquois; each of which portions is exactly of a fine oval figure, and each of the two uttermoſt is throughout of a very uniform breadth as well as colour, and exactly parallel to the other. But it would not be here ſo proper, as it will be hereafter, to multiply inſtances of opacous gems: wherefore (having mentioned only the ſardonyx, becauſe it is not always opacous,) I ſhall add concerning tranſparent ones, that jewellers reckon among ſapphires not only that ſort of azure gems, which uſually paſs for ſuch, but alſo another ſort of ſtones, becauſe of their ſapphirine degree of hardneſs; though for their want of tincture, they call them white ſapphires.

6. THE ſixth and laſt circumſtance belonging to the foregoing argument or conſideration is this, that ſometimes one may find gems, that are partly tincted and partly not; as if the tinging pigment mixing with one part of the matter, whereof the ſtone conſiſted, whilſt it was liquid or ſoft, were not copious enough to diffuſe itſelf to the whole, nor to give an equally intenſe colour to all that portion, that it tinges. It is true, that in ſome caſes, the diffuſion may be ſtopped by the petreſcent juices coagulating firſt in another part than that, with which the tincture was mixed. And perhaps, in ſome other caſes, the different colours may have belonged to differing portions of matter, coagulating upon or againſt each other, at different times, yet ſo as to ſeem one intire ſtone; as I may have hereafter occaſion to declare. Yet ſince, which ſoever of theſe explications be admitted, it will, if it belong not to this place, at leaſt, confirm our main hypotheſis (of the origin of gems from fluid or ſoft materials,) I ſhall return to what I was ſaying about gems, partly tincted, and partly colourleſs. And having only intimated upon the by, that in ſome hard ſemidiaphanous ſtones, European and Eaſt-Indian, I have obſerved a very unequal and irregular diffuſion of the tincture; I ſhall add to the things, that may be gathered in favour of the propoſed conjecture from ſome of the things before (as alſo ſince) related, theſe two particulars:

THE one, that I have (as I think I elſewhere mentioned) ſeen in *Italy*, among rarities, a large piece of cryſtal about the bigneſs of my two fiſts, whereof the pyramidal part was of a tranſparent green, the vertex being richly tinged like an emerald; but the further the colour ſpread from the vertex, the fainter and paler it grew; ſo that, before it came near the baſe, it was quite ſpent, if I may ſo ſpeak, leaving the bigger part of the ſtone tranſparent, but colourleſs, like ordinary cryſtal. And by this, perhaps, we may explain an expreſſion of *Joſephus Acoſta*, where he ſays, that emeralds grow in ſtones like unto cryſtals; and that he had ſeen them in the ſame ſtone faſhioned like a vein; and they ſeem, adds he, by little and little to thicken and refine. And in the ſame place, this learned author has a memorable obſervation, that may confirm both what I have juſt now related, and what we mentioned a little above, about colourleſs gems: I have ſeen, ſays he, ſome, that were half white and half green; others all white, and ſome green and very perfect. And this is the firſt particular I was to mention.

THE other is afforded me by the way I have uſed, and elſewhere deſcribed, of giving to pieces of rock cryſtal paſſably good tinctures by mineral fumes. And ſuppoſing the thus coloured pieces to be as intire ſtones, as the beholders have generally believed them, the inſtance will be pertinent to our purpoſe in ſpite of an objection. For though the colours thus given are not wont to pervade them very deep, and have their penetration aſſiſted by no faint degree of heat; yet it is to be conſidered on the other ſide, that theſe pieces of cryſtal had attained their full hardneſs, and after their colouration, are cut and poliſhed like other cryſtals: whereas the gems, that our conjecture means, are ſuppoſed to have been tinged under ground, when they were yet fluid, or at leaſt ſoft. That there are ſometimes generated in the bowels of the earth, mineral exhalations capable of applying themſelves to the ſtones they meet with there, I have, in another diſcourſe, ſufficiently declared. That alſo ſome hard and ſtony ſubſtances have been actually tinged with ſuch mineral ſteams, I ſhall, in the ſubſequent part of this diſcourſe, have occaſion to take notice. And I remember too, that even in ſo hard a gem as a ſapphire, I have obſerved the efficacy of theſe ſubterraneal fumes; having divers times ſeen one of thoſe ſtones, wherein a fine ſeal was cut, which continued ſo oddly tinged, notwithſtanding what had been taken off to reduce it to an exquiſite ſhape, that having inquired of a ſkilful perſon of my acquaintance, by whom it had been engraven, he both aſſured me, that he had found it of the full hardneſs of a ſapphire, and confeſſed to me, that the mineral fumes had ſo oddly tinged it, that in his opinion, it might, by the looks, paſs rather for a Chalcedonian.

Of ſubterraneal fires, &c.

AND now, Sir, I fear I may need your pardon, for having been ſo prolix in diſcourſing of one of the particulars belonging to our argument; to excuſe which, I have no other apology to make, but that I hope what hath been delivered will ſcarce ſeem impertinent, and that I might eaſily have made it more tedious, if, to decline doing ſo, I had not purpoſely made ſome omiſſions.

HAVING then ſaid thus much about our fourth conſideration, I proceed now to add, in the fifth place, on the behalf of the hypotheſis hitherto favoured, an argument, which I preſume you will not think inconſiderable; namely, that ſolid gems may include heterogeneous matter in them. Several inſtances of this ſort, in opacous ſtones, I elſewhere recite upon my own obſervation; but in tranſparent ones they are very great rarities; and therefore it will not, I preſume, be thought ſtrange, if I mention but a few.

FIRST then, on this occaſion I remember, that a very ingenious and qualified lady, who had accompanied her huſband in an embaſſy to a great monarch, aſſured me, that ſhe brought thence, among ſeveral rich preſents and other rarities, (ſome whereof ſhe ſhewed me,) a piece of cryſtal, in the midſt of which there was a drop of water, which by its motion might be very eaſily obſerved, eſpecially when the cryſtal was made to change its poſture. And, if my memory deceive me not, I have, in ſome pieces of rock-cryſtal, taken notice of things, that ſeem to argue, that ſomewhat or other was intercepted within the body of the ſtone.

A curious perſon, that traded much, and was very ſkilful, in Indian gems, particularly grizolettes, which he got from the *Indies*, and whereof he ſhewed me the largeſt I have yet ſeen, being aſked by me, whether he had ever found in them any heterogeneous ſubſtance, which ſomething, I had obſerved, made me ſuſpect, that ſome of them might harbour, notwithſtanding their hardneſs; he averred to me, that among divers rough ones, that were brought from the *Indies*, he had with wonder ſeen one, that was about the bigneſs of a philbert, in the ſolid ſubſtance whereof there was a cavity with a certain liquor in it; which, by changing the poſture of the ſtone, might be made to move to and fro in the cavity: and when the drop was ſettled, it was of the bigneſs of a round pearl, that he ſhewed me, which wanted ſomewhat of a moderate ſize for a neck-lace. And when he had anſwered the queſtions I propoſed him, to clear my doubts, he added, that this rarity made the ſtone, which was otherwiſe of a ſmall value, prized at an hundred pound. And I have myſelf ſeen a monſtrous gem, if I may ſo call it, and little leſs a rarity then the former, that an acquaintance of mine had bought, (as I afterwards learnt,) from this relator; whoſe narrative about the grizolette I think the more credible, becauſe that having had the curioſity to break a ſtone, that was brought as a rarity from the *Eaſt-Indies*, where gems are often harboured in ſuch ſtones, I found in the ſolid ſubſtance of it (which was ſo hard as to ſtrike fire, like a flint, and in its little flakes was at leaſt ſemi-diaphanous) a cavity, wherein were coagulated very minute but poliſhed and cryſtalline ſtones, which ſeemed to have their points inwards; which argued, that there had been ſome liquor, in which theſe gliſtering particles had ſhot, though in proceſs of time the remaining and incoagulable part of it may have been imbibed by the ambient matter, if not have eſcaped thorough it, by virtue of ſome peculiar congruity of it with the pores of the ſtone. Which need not be thought impoſſible, ſince experience has aſſured us, that ſome ſolid ſtones, and even gems, may be, though ſlowly, penetrated, or have their texture altered by common water. Nor are theſe the only heterogeneous ſubſtances I found included in this ſtone.

AND if, as amber is reckoned among gems, and is ſometimes of a greater hardneſs, than one would expect, ſo I could reckon it among true ſtones, it were eaſy for me to borrow thence a great confirmation of what I have been ſaying; and, however, it will afford me an illuſtration of it. For, not to mention many things, of what I elſewhere recite my ſelf to have ſeen in amber, I have now by me a fine piece of clear and ſolid amber, (preſented me by a perſon no leſs extraordinary than it) in which is included a large intire fly, in ſhape and ſize much like a graſs-hopper, but variouſly and curiouſly coloured, with his wings diſplayed.

To theſe obſervations I ſhall add only this; that I have had my ſelf, and ſhewn to others, one of that ſort of pale amethyſts, that ſome call white amethyſts; which had been cut, to be ſet in a ring, or turned into a ſeal, and was, like that ſort of gems, ſo hard, that I could readily cut glaſs with it; and yet in the body of this ſtone there appeared to be a conſiderable number of things, that looked juſt as if they had been hairs, ſome of them lying parallel, and others inclining to one another; and having contemplated them, as well by day-light as candle-light, and in divers poſitions in reference to the light and the eye, ſome of them ſeemed at times to be of a lovely reddiſh colour, but reflecting the light, as if they were well filled either with air or water: but for the moſt part they did, as I was ſaying, ſeem to be hairs of a browniſh colour, which made the ſtone not a little wondered at, even by curious and ſkilful men. I leave you to judge, whether it will be fit here to add, that I have ſometimes ſuſpected, that even in diamonds themſelves there may poſſibly be found intercepted, or mingled with a pure lapideſcent ſubſtance, ſome particles of heterogeneous matter. And that in this ſuſpicion I was ſomewhat confirmed, as by the odd clouds I had obſerved in an extraordinary diamond, and by ſome hydroſtatical, and other obſervations I made about thoſe ſtones, (ſome of which I found heavier than either cryſtal or white marble;) ſo by my having purpoſely demanded of an ancient cutter of diamonds of great practice and experience, whether he obſerved not a ſenſible difference of weight among diamonds of the ſame place: for to this he replied, that he had, eſpecially in thoſe, that were cloudy or foul; inſomuch, that ſhewing me a diamond, that ſeemed to me to be about the bigneſs of two ordinary peaſe, or leſs, he affirmed, that he ſometimes found in diamonds of that bigneſs about a carrat (which is by common eſtimation four grains) difference in point of weight.

SIXTHLY, the laſt argument I ſhall employ to ſhew, that the matter of divers gems may have once been fluid, may be taken from the proofs you will meet with (in the following part of this tract) of the ſecond member of our hypotheſis. For if it ſhall appear, that ſeveral even of the tranſparent gems have metalline or other extraneous mineral bodies mingled with them, *per minima*, it will be very agreeable to reaſon to ſuppoſe, that ſuch a mixture was made, when the mingled bodies were in a fluid form; ſince, beſide that one may well aſk, how elſe the metalline corpuſcles came to be conveyed into ſuch compact and hard bodies as gems, it is very eaſy to conceive, if our hypotheſis be admitted, and very hard otherwiſe to apprehend, how among bodies, that differ *toto genere*, as metals and ſtones, there ſhould be made mixtures ſo exquiſite, as many of theſe appear to be, partly by the uniform coloration of the gem, and partly by the diaphaneity retained notwithſtanding this diſperſion of mineral pigments through the whole maſs; and in many inſtances alſo by the curious figuration, that we have lately been diſcourſing of.

POSTSCRIPT.

To all the foregoing circumſtances, I can now add ſomething, that I met with, ſince I thought to conclude with the laſt of them, and that tends highly to the confirmation of our hypotheſis. In a tract, that makes part of a ſmall book freſhly publiſhed in French, principally to acquaint men with the ways of eſtimating gems, according to the rates of modern jewellers, the anonymous, but curious author, takes occaſion, to give us, from the mouth, as he affirms, of the famous late travellers he converſed with in divers places, and whoſe relations are indeed the recenteſt I have ſeen in print, an account of the number, and names of the places, where diamonds and rubies are found in the *Indies*; adding ſome circumſtances and particularities about the qualities of the ſoil in thoſe places, that I have not elſewhere met with. This author, then, ſpeaking of the firſt of thoſe three diamond-mines, which he makes to be the only ones in the *Eaſt-Indies*, having told us, that the ſtones are there found, ſome in the ground, and ſome in the rock, ſubjoins, that thoſe that are drawn from the rock, or the neighbouring parts, have ordinarily a good water; but for thoſe, which are drawn out of the ground, their * water partakes of the colour or ſoil, wherein they are found. So that if the earth be clean and ſomewhat ſandy, the diamonds will be of a good water; but if it be fat or black, or of another colour, they will have ſome tincture of it. Nay, he immediately annexes, that if there be ſome black or red ſand among the earth, the diamond will alſo have ſome grain of it. And elſewhere mentioning the ſecond mine of diamonds, which the natives call gems, he admoniſhes his reader, that in this, as in the mine of *Viſapour*, which is that formerly mentioned, the ſtones partake of the quality of the ſoil, where they are found; ſo that if that be boggy or moiſt, the ſtone will incline to blackneſs, and if it be reddiſh, it will have an eye of that colour. Elſewhere he tells us, that of late years there were found in the kingdom of *Golconda* ſtore of diamonds, which were brought to the Nababe, or firſt miniſter of ſtate, who forbad the making any further ſearch after them, finding not one in the whole number to have a good water, all of them being black or yellow. But by the way, whereas this author affirms it as a clear truth, that as gold is the

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* Que s'il y a quelque ſable noir ou rouge parmi la terre, le Diamant auſſi en aura quelqu'un. *P.* 9.

heavieſt and moſt precious of metals, ſo diamonds are the hardeſt, and heavieſt of all ſtones, he muſt excuſe me, if I declare, that what he aſſerts agrees not with my experience, who having tried the weight of an uncut diamond hydroſtatically, have taken ſuch a courſe to eſtimate its ſpecifick gravity, as I find not to have been yet taken by any other, and which you will eaſily grant to be more exact than any other of the known ways can be.

THE argument, that hath detained us all this while, compriſed ſo great a variety of matter, and may, I hope, perform ſo great a part of my taſk in this diſcourſe, that, though I ſhall not much apologize for having dwelt ſo long upon it, yet I ſhall think my ſelf obliged to make ſome amends for my paſt prolixity, by being ſuccinct in the remaining part of this treatiſe; and therefore, having left off with an intimated promiſe to ſhew more fully, that divers gems contain metalline, or other mineral ſubſtances, in them, I ſhould immediately connect thoſe arguments to what hath been lately ſaid, but that I think it altogether requiſite, to make way for what is to follow, by firſt taking notice of a main objection, that may be urged againſt the doctrine we have been propoſing.

THIS is taken from the figuration of ſome gems, and eſpecially the priſmatical one of cryſtal, and ſeems the more fit to be urged againſt us, becauſe we our ſelves have, in the ſecond of the above-recited arguments, given ſeveral inſtances of it. For it ſeems ſcarce poſſible, that ſo curious a ſhape ſhould be ſo uniformly produced in ſuch a multitude of cryſtals, great and ſmall, unleſs there were ſome ſeminal and plaſtick power to faſhion the matter after ſo regular and geometrical a manner.

BUT he, that ſhall attentively conſider, what I elſewhere ſay concerning the figuration of ſalts, and of metalline and other magiſteries diſſolved by, and concoagulated with ſalts, may be very much aſſiſted to diſcover the invalidity of this objection. But yet, becauſe I confeſs it is very ſpecious, if not important, I am content here to conſider it a little more particularly.

To this plauſible objection then I have two or three things to anſwer; firſt, that there is no abſurdity to conceive, that, if there be a ſeminal and plaſtick power in mineral bodies, it may be harboured in liquid principles, as well as otherwhere. For we ſee, that the ſeed of animals, which oftentimes, as in elephants, rhinoceroſes, &c. produces hard and ſolid bones, teeth, and horns, is at firſt but a liquid ſubſtance; and the formative power in ſome trees and their fruits does convert the alimental juice into woods, ſhells, and other bodies very ſolid and ponderous.

BUT ſecondly, I elſewhere * ſhew, that even in the figures of allom, vitriol, and other ſalts, that are ſo curiouſly and geometrically ſhaped, there is no neceſſity to fly to a diſtinct architectonick principle; but that thoſe bodies themſelves may receive their ſhapes from the coalition of ſuch ſingly inviſible corpuſcles, as by the motion of the fluid, wherein they did ſwim, and by divers aſſiſtant circumſtances, are determined to ſtick together rather in that manner, than in another. That this may be applyed alſo to other bodies, I ſhall need to ſhew in this place, by no other inſtance than that of the ſalt, that (in this or ſome other paper) I formerly told you I made of common ſalt, only by the help of oil of ſulphur, or of vitriol and water. For though it be manifeſtly a factitious body compounded of ſalt and ſulphur, and ſuch a body, that therein the ſea-ſalt, whereof it was chiefly made, has had its own nature deſtroyed; yet, by reaſon of the figure of the reſultant corpuſcles, and their fitneſs to convene, when diſſolved in water, into curiouſly ſhaped bodies, this factitious ſalt, when I have rightly prepared it, did ſundry times ſhoot into long cryſtals with points like diamonds, that did emulate native cryſtal as well in the regularneſs of the ſhape, as in the tranſparency of the ſubſtance. And to make it the more evident, that it was partly the figure, that happened to reſult from the operation of the oil of vitriol upon the ſea-ſalt, and partly other circumſtances, that determined the ſhape of the cryſtals; I ſhall add, that uſually, when the quality or proportion of the oil of vitriol was other than it ſhould have been, or an error was committed in ſome important circumſtance or other of the operation, the ſaline concretions, though they did not ſhoot at all like cubes, as the ſea-ſalt, which they were made of, would alone have done; yet they did not ſhoot any thing at all like rock-cryſtal, as did thoſe formerly mentioned; and for all this did, by reaſon of the curious ſhapes of the corpuſcles, they conſiſted of, ſhoot into cryſtals, for the moſt part, finely figured; though ſometimes of one ſhape, and ſometimes of another. And that you may not have any ſuſpicion, as if the regular figure, which ſea-ſalt is naturally of, is any way neceſſary to ſuch figurations, I will add an experiment, that I deviſed to ſhew, that even out of a petreſcent juice ſuch curiouſly figured bodies may be made. I took then ſome ſtony ſtiriæ, elſewhere mentioned to have been found in caves or grottos, where petreſcent liquors coagulated before they have time to fall down; and and having diſſolved them in ſpirit of verdigreaſe, I put the clear ſolution to evaporate in a digeſtive furnace, after the ordinary manner; by which means, though I made the experiment more than once, I had rather a coagulated maſs, than any thing like cryſtals. Whereby you may learn the truth of what I was ſaying, that a concourſe of divers circumſtances may be requiſite to determine the figuration of conſiſtent bodies, made out of fluid ones; ſince here, for want of time for making occurſions enough for the particles to concrete in after the moſt convenient manner, the experiment ſucceeded not: wherefore it being agreeable to my notions, that ſome ſorts of bodies may require a longer time to make ſuch a convention in, than others, I allowed many days to another ſolution of ſtiriæ made in the ſame

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* See the Origin of Forms and Qualities, now publiſhed by the Author.

menſtruum; after which there ſhot, as I deſired, about the ſides and bottom of the glaſs a number of diſtinct cryſtals, long, tranſparent, and curiouſly ſhaped, moſt of which, I think, I can yet ſhew you.

Perhaps it will be ſaid, that the petreſcent juice, when broken, does oftentimes appear to abound, within, with ſtiriæ, or narrow ſtreaks like thoſe of antimony; and that I myſelf obſerve ſome gems to be made up of thin flakes or plates; which internal figuration ſeems to be much more difficult to be accounted for, without a plaſtick form, than the external.

I will not reply to this, that, for aught I know, divers known ſalts would, when broken, appear to be geometrically figured, even in the leſſer corpuſcles, as well as they are evidently ſo in their entire bulk, if we had eyes quick enough to diſcern the ſhapes of the minuter, as well as of the bigger bodies. And we have great inducements to think, that whether or no *Carteſius* do rightly make the inviſible particles, of which the ſmalleſt viſible grains of ſea-ſalt are made up, to be long and rigid like ſticks; the minute viſible concretions, of which the bigger grains of ſalt conſiſt, are, as well as themſelves, of a cubical figure; I will not, I ſay, inſiſt on this reply, but proceed to alledge, that there are divers bodies ſo luckily ſhaped, that upon a ſlow coalition, they will convene into a multitude of manifeſt concretions; ſome of which will conſiſt of ſtreaks, and other be made up of flakes; as in the ſal armoniack, commonly ſold in the ſhops (for I ſpeak not of the native, that is ſaid to come from *Armenia,*) though it be avowedly a factitious body, you may often obſerve, upon breaking the bigger maſſes, great multitudes of ſtreaks, like thoſe we may uſually obſerve in the broken ſtiriæ of petrifying water. And I have more than once ſeen, and alſo made, artificial concretions (of whoſe preparation I elſewhere ſpeak) ſome of which conſiſted of ſalts alone, and others of ſalts and minerals, as ſtones or antimony, which look very like talk, being white bodies, made up of a multitude of very ſlender ſtreaky particles lying long-ways one upon another, as in that mineral. And as I have taken out of earth many concretions, which, as they were for the moſt part outwardly ſhaped like rhombuſſes or lozenges, were compoſed of a multitude of flat and extremely thin plates; ſo I have ſometimes taken pleaſure to imitate ſuch concretions by art. And though a ſolution of ſilver in putrified aqua fortis does uſually afford only a great company of ſmall, thin, and ſeemingly ſimple flakes, like ſcales of fiſh, becauſe men have not any deſign like ours in procuring the concretion; yet having diſſolved a good quantity of the metal together, and ſuffered it to ſhoot leiſurely, and with due circumſtances, I have obtained ſundry cryſtals, which both were geometrically figured without, and conſiſted of a multitude of exceeding thin flakes orderly ſticking to one another. And I remember, that whilſt the objection, I am anſwering, was in my thoughts, I pitched upon a yet more pregnant experiment for the clearing of it. For conſidering how tin-glaſs, though a compact and ponderous body, does naturally conſiſt of a multitude of ſhining poliſhed flakes, (which may be eaſily perceived and diſtinguiſhed by breaking a lump of it into three or four pieces;) I found by trial what I expected, that though a maſs of this mineral were beaten to powder, yet if it were melted and ſuffered to cool of it ſelf, the diſpoſition of the component particles would determine them to ſtick to one another in broad and ſhining flakes, whereof many will be incumbent one upon another, and ſome croſs to one another at various angles, according as the matter happened in its ſeveral portions to be diverſely refrigerated. And ſome factitious bodies may afford us the like inſtances, as I have obſerved in ſome mixtures of copper, iron, and other minerals; and very conſpicuouſly in good regulus martis ſtellatus, whoſe internal configuration may be found by breaking it; by which way I have obſerved with pleaſure, that the regulus abounded with flat and ſhining flakes of an almoſt ſpecular poliſh.

If it be urged, to confirm the former objection, that ſome lapideſcent juices, even of thoſe we mentioned in theſe diſcourſes, do concrete, even whilſt men are looking on; and yet our ſtony ſtiriæ, often mentioned (which probably may be alſo haſtily coagulated) have in ſome places a ſtreaky, and in other places an angular configuration of parts; I anſwer firſt, that I have ſeen divers of that kind of concretions, which, as far as the eye took notice of, were made up of parts confuſedly jumbled together. And next, that (to conſider now thoſe, whoſe texture is more uniform) I have found by trials, that, if there be a due diſpoſition in the component corpuſcles of bodies to ſuch configurations, they may be brought to concrete accordingly in a far ſhorter time, than almoſt any, that have not tryed, would expect not to ſay, believe. Having ſometimes, for curioſity's ſake, warmed ſix or ſeven ounces of aqua fortis, glutted with fine ſilver, 'till the mixture was all brought into a tranſparent liquor; and having then put the clear, but ſtrong glaſs, that contained it, into cold water, that the menſtruum might be the more haſtily refrigerated, I obſerved, that when once the diſſolved metal began to ſhoot, the coagulation into figured cryſtals proceeded ſo faſt, that a naked eye could ſee the progreſs of it. And having ſometimes put a quantity of ſalt and ſnow, or of ſome other ſtrongly refrigerating mixture, into a convenient glaſs, and wetted the the outſide with a ſtrong ſolution of ſal armoniack, or ſome urinous ſpirit, though in leſs than a minute of an hour it would be coagulated; yet the ſalt, into which it ſhot, had uſually a curious and determinate figure, according to the nature of the liquor, that afforded it; as I have often ſhewn the curious.

Perhaps you will ſay, that theſe inſtances are taken from ſaline bodies, which are, for the moſt part, diſpoſed to convene in ſmooth ſurfaces, and angular ſhapes, and eaſy enough to be wrought on by the external cold; and it may yet ſeem ſtrange to philoſophers themſelves, what in ſome caſes muſt have happened, if our hypotheſis be admitted, namely, that external cicumſtances and accidents, ſuch as the figure of a mold or womb, the coldneſs of the ambient, &c. ſhould viſibly, and ſometimes not a little, diverſify even the internal figuration of cloſe and ſolid minerals and gems, without excluding all thoſe, that are ſuppoſed to be of a quicker concretion.

WHEREFORE to clear this difficulty, it may not be amiſs to ſubjoin an experiment, that I deviſed to ſhew, that if the corpuſcles of a body be ſo ſhaped, as to be fitted by their coalition, to conſtitute ſmooth (and if I may ſo ſpeak) gloſſy planes, though they be variouſly ſhuffled and diſcompoſed, as to their priſtine order, yet, if they be but a little kept in a ſtate of fluidity, that they may the fitlier place themſelves, or be placed by other agents, they will preſently be brought to convene into ſmooth and ſhining planes, and the ſituation of thoſe planes, in reference to one another, will be more uniform and regular, than almoſt any one would expect in a concretion ſo haſtily made; notwithſtanding which, their internal contexture will be much diverſified by circumſtances, as particularly the figure of the veſſel or mold, wherein the fluid matter concretes.

CONSIDERING then, that, according to what I noted already, if we break tin-glaſs (taken for the biſmuth of the ancient mineraliſts) as it is wont to be ſold in lumps in the ſhops, it will diſcover a great many ſmooth and bright planes, larger, or leſſer, according to the bigneſs of the lump; which ſometimes meet, and ſometimes croſs one another at very differing angles: conſidering this, I ſay, I thought it probable, that a body, that had already been melted, and was apt to convene into ſuch planes, not only would do ſo upon another fuſion, but might have the order and bigneſs of thoſe planes, diverſified by the figure and capacity of the veſſel, I ſhould think fit for my purpoſe. Wherefore having beaten a ſufficient quantity of it to powder, and, when it was well melted, caſt it into a good pair of iron molds, whoſe cavity was an inch in diameter, we had a bullet, which being warily broken, did, as we expected, ſeem to be, as it were, made up of a multitude of little ſhining planes, ſo ſhaped and placed, that they ſeemed orderly to decreaſe more and more, as they were further and further removed from the ſuperficies of the globe; and they were ſo ranked, that they ſeemed to conſiſt of a multitude of theſe rows of planes reaching every way, almoſt like ſo many radius's of a ſphere from the center or middle part, to the circumference: whereas, if we melt tin-glaſs in a crucible, and let it cool there, the matter being taken out and broken, will appear indeed full of ſmooth planes, but (as was lately intimated) very irregularly and confuſedly aſſociated or placed.

I will not now ſtay to enquire, whether the orderly compoſition of the planes in our bullet (which ſome curious perſons, that I ſhewed it to, looked on, as a not unpleaſant ſight,) may be derived from this, that the matter was cooled firſt on the outſide, by the contact of the cold iron mold, and the neighbourhood of the ambient air; and that the coagulation being once thus begun, the parts of the remaining fluid, as they happened to paſs by this already cooled matter, with a motion, which, by reaſon of their removal from the fire, was now ſlackened, they were eaſily faſtened againſt the already ſtable parts, (as may be illuſtrated by the concretion of diſſolved nitre and allom, both about the injected ſticks, and the grains, that firſt concrete againſt the ſides of the veſſel) and the refrigeration ſtill reaching further inwards, till it came laſt of all to the middle of the globe, that being the remoteſt part from the refrigerating agents; the appoſition was ſucceſſively and orderly made, till the whole matter was concreted. But, (as I was ſaying) I muſt not now ſtay to inquire, whether the figuration of our bullet may be explained after this or ſome ſuch way; or whether we are not to take in ſome ſubtle or pervading matter, or ſome other catholick agent? For though ſuch points may be well worth diſcuſſing, and we may poſſibly elſewhere ſay ſomething of them; yet here it may ſuffice to ſay, that we have varied the foregoing trial, by caſting bullets of ſome other bodies, (and particularly the ſimple regulus of antimony) wherein it ſucceeded well enough, though the produced contexture were not ſo uniform as in tin-glaſs. And I alſo tried, that having caſt melted ſulphur itſelf into a globulous body of about five or ſix inches in diameter, and warily broken it, though one would think it an unlikely mineral to make any other, than a confuſed concretion, it preſented me great fibres, almoſt like little ſtraws, whoſe number, and, in great part, orderly ſituation, afforded me a much leſs unfit inſtance for my preſent purpoſe, than one would have lightly expected. But what I came from ſaying, may ſerve to make out what I propounded to my ſelf; which having named already, I need not here repeat.

BUT one thing more there is, that may be pertinent on this occaſion, namely, that I have broken divers marchaſites of a peculiar ſort, that were either of a roundiſh, or of an almoſt cylindrical figure, to obſerve their internal ſtructure and qualifications; whereupon, I found in more than one of them (for I remember not, that I did in all) a great many rows of little planes or gliſtering corpuſcles, reaching from the innermoſt to the external ſurface, and in thoſe, that were ſomewhat cylindrically ſhaped on the out-ſide, theſe ranks of gold-coloured particles in the ſeveral planes of the broken mineral ſeemed like ſemi-diameters iſſuing out from a row of phyſical points, conceived to be placed on an imaginary line, lying almoſt like the axis of a cylinder between the oppoſite ends, (though I do not well remember, how near it reached to them;) as if the cavities of the chalk or clay, wherein theſe marchaſites were found, had made the ſoil like a mold, wherein the matter of the marchaſite being detained, whilſt it was in a fluid form, did afterwards concrete much after the manner, that the bullets of tin-glaſs, regulus, &c. did in our molds. But the proſecution of this conjecture belongs to another diſcourſe.

I ſhall therefore now proceed to a further anſwer to the formerly raiſed objection: wherefore, as to the exquiſite uniformity of ſhape, which is ſo admired in gems, and is thought to demonſtrate their being formed by a ſeminal and geometrizing principle; though I have, in the ſecond of the above-mentioned arguments, aſcribed to them ſuch curious figures, as argue their having been generated after the way propoſed in our hypotheſis; and though alſo I willingly allow their ſhapes to deſerve from us a delightful wonder at the curiouſneſs of nature's, or rather her author's, workmanſhip; yet, upon a more attentive ſurveying of them, I do not find the uniformity to be near ſo great as is wont to be imagined; but have rather met with ſuch diverſities, as agree well with our hypotheſis about their figuration.

In ſeveral tranſparent gems, it ſeemed manifeſt enough to me, (as I lately alſo noted) that the ſhape was, in great part, due to the figure of the womb, or mold, wherein the matter, whilſt liquid or ſoft, happened to ſettle. In ſome other tranſparent and well figured gems of the ſame kind or denomination, and ſometimes growing very near one another, by a diligent inſpection I found a manifeſt and ſometimes very conſiderable difference in their ſhapes, either as to the number, or the figures, or the bigneſs of the ſides or planes, that made up the reſpective gems; or as to two, or all, of theſe; comparing theſe deviating particulars with what would have been in a ſtone of that kind or denomination, that were perfectly figured. This I had opportunity to take notice of, particularly in two ſorts of ſtones; the firſt granats, of which I had a conſiderable number brought me out of *America* growing in one lump of matter; but in diſtinct parts of it, and without touching one another: among which I took notice of a manifeſt diſparity of ſhape, and ſo I did in ſome African ones, that were preſented me; as alſo in others, that were European, one of which, that was of an extraordinarily large ſize for a figured gem of a tranſparent kind, (for it weighed above eleven drachms and a half,) I conſidered with a particular attention, and found, that though it ſeemed to have been coagulated in a fluid medium, and to conſiſt of twelve planes, at the concourſe of two or three of which it ſeemed to have been broken off from the womb or root; yet it was very far from the dodecahedron of geometricians. For, whereas that conſiſts of twelve æquilateral and æquiangled pentagons, almoſt all the planes, that made up our granat, were quadrilateral and very different from what regularly they ſhould have been, not only in magnitude, but in ſhape: for one of them ſeemed to have five ſides, and of the reſt, ſome were moſt of kin to a rhombus, others to a rhomboides; but the moſt were but little better figured, than thoſe, that the geometricians call the trapezia. And thus much for the firſt ſort of gems, whoſe ſhapes I obſerved to be not regular. The ſecond conſiſts of thoſe cryſtalline ſtones, which they call Corniſh diamonds, and which are ſome of them much harder, than the Briſtol diamonds, or perhaps, than rock cryſtal it ſelf; it being eaſy to write upon glaſs with them. Of theſe ſtones having procured a good number (many of which I have yet by me) I took notice, by comparing them heedfully together, that though ſome of them were geometrically and curiouſly ſhaped like rock-cryſtal, having each ſix ſides, whereof every two, that were oppoſite, were throughly like and equal enough to one another; and though the ſtone had a pyramidal termination, made up of ſeveral reſembling and curiouſly figured planes, that terminated in a ſolid angle or apex; yet the greateſt number, by much, of theſe titular diamonds was made up of ſtones, far from being ſo exactly and uniformly ſhaped, as thoſe newly deſcribed. For though moſt of them had ſix long planes; yet oftentimes the oppoſite ones (beſides that they were not ſo parallel to one another, as they ſhould have been) were unlike and exceeding unequal; and thoſe planes, that were to make up the apex, though apart, they were uſually angular; yet being compared to one another, or to the regular patterns abovementioned, their figures, their bigneſſes, and their manner of concurring (which was ſometimes not in a point or apex, but in a line,) was ſo remote from being uniform, that this great diverſity and irregularity agreed far better with our hypotheſis, than with its rival. And yet in theſe ſtones, the want of room to coagulate freely in, could not with probability be pretended; for they ſeemed to have been formed ſeparately in a fluid ambient, ſave at the bottom, where they were faſtened to the rock, as appeared by an opacous root, if I may ſo call it, which ſtill adhered to moſt of them. And, if I much miſremember not, I have more than once in diamonds, newly brought from the *Indies*, and ſome of them very fair ones, obſerved a great want of uniformity in the areas of the ſuperficial planes, or in their figures, or both; and ſometimes too in the very number, as well as ſituation, of their ſolid angles or corners: about which I hope to recover ſome notes. And ſo I have done with the firſt part of my anſwer to the abovementioned objection; whereby it may appear, that there is no ſuch regular and conſtant uniformity in the ſhapes of gems, but that their real likeneſs may be reconciled to our hypotheſis.

But now in the ſecond part of my anſwer, I ſhall endeavour to ſhew, that the figuration of gems may not only conſiſt with our conjectures, but confirm them. For I have, more than once, taken notice in the Corniſh diamonds I have been mentioning, that ſometimes a ſmall ſtone of the ſame kind has made up, as it were, one body with a greater; ſo as that the leſſer ſtone did not only adhere cloſely to the other, but was, if I may ſo ſpeak, ſet or bedded in it. So that when the ſeparation was made, there remained in the greater ſtone a cavity, whoſe figure did curiouſly anſwer that of as much of the ſmaller ſtone, as chanced to be harboured there. And, as ſometimes I obſerved, that there was ſuch an adnaſcency, (if you will pardon the word,) of a leſſer ſtone to a much greater; ſo at other times, I met with the like of a greater to a much leſſer, with a cavity in the leſſer, anſwerable to that part of the greater, that had been lodged in it. Which, for aught I know, allows us with high probability to conjecture, that the ſtone, to which the other grew, was firſt formed and hardened; ſince it retained its own ſhape, and that, whilſt this remained adherent to the rock or ſoil, ſome more liquor, either, that came afterwards by chance into the ſame cavity, or (in caſe it were there before,) that was leſs diſpoſed to an early concretion, began to be coagulated by faſtening itſelf againſt the ſolid body, that was already concreted: upon which account theſe two diamonds muſt ſtick cloſe together, and yet be but contiguous, and a cavity, ſuch as I freſhly mentioned, muſt be left in the laſt concreted gem. Which may be illuſtrated by putting into a ſtrong ſolution of pure nitre, or rock-allum, ſome little ſticks of wood or any ſolid body, that may be kept ſteadily in the poſture; for you will ſee many coagulations begin to be made againſt them, and the cryſtals thus concreted will neceſſarily have their figures incomplete, and have in them cavities correſpondent to thoſe parts of the ſtick, whereto the ſaline corpuſcles faſtened themſeves. To which I ſhall only add, that though I have given inſtances of the adnaſcency of figured ſtones only in Corniſh diamonds, yet they are not the only tranſparent minerals, wherein I have been able to obſerve it. And particularly I remember, that I obſerved among ſome minerals, left by a gold-ſmith to his widow, a fine tranſparent and neatly figured ſtone, which ſeemed to be pure cryſtal, but was coagulated about a kind of branching wire, whereof a good part was incloſed by the ſtone, that ſeemed to grow out of a piece of ore, that looked like ſilver-ore, and which the woman, that was a curious perſon, upon the ſtrict enquiry, that I made, affirmed to be, together with the abovementioned branch, good ſilver, produced by nature in that form, (which I thought the more credible, becauſe of the odd and almoſt hair-like ſhape, wherein I have ſeen ſilver-ore to have, as it were, grown;) which will excellently agree with the reſemblance, I was juſt now propoſing betwixt the coagulation of diſſolved ſalts and the liquid matter of gems, about ſtable bodies partly immerſed in thoſe fluids.

THE very many circumſtances belonging to our firſt argument, and the laſt anſwered objection, have ſo long detained us, that I doubt, you now think it more than time I ſhould advance to, and diſpatch the ſecond of thoſe grand conſiderations, whereon I at firſt intimated our hypotheſis was founded; and this is built upon the weight of ſome gems, which being greater than that, which ſeems to belong to them, as hard and tranſparent ſtones, I think we may probably derive it from metalline or mineral mixtures.

I queſtion not, but as you will think this allegation new, ſo you will be apt to queſtion, how I come to know the truth of what I here deliver; ſince, though gems are wont to be eſtimated by lapidaries, as they weigh ſuch or ſuch a number of carrats, or of grains, yet they compare only the weight of this and that ſtone of the ſame kind in reference to one another, as the greater or leſſer weight argues the greater or leſſer bulk, without looking after, or knowing how to diſcover the ſpecifick gravity of ſeveral gems, which depends not on the greater or leſſer bulk; as (if you know it not already) you will gather from what I am now going to relate.

CONSIDERING then with my ſelf, that for my purpoſe, it was requiſite to have a gem as free as I could get from the metalline mixtures, that I ſuſpected many precious ſtones to have; and remembring, that rock-cryſtal, as it is by mineraliſts reckoned among gems, ſo it is hard enough, as I tried, both to cut glaſs, and to ſtrike fire; and that its having ſo great a tranſparency, and its being devoid of colour, makes it exceeding likely to be free from adventitious mixtures; I pitch'd upon it as the ſtandard, whereby to make a probable eſtimate of the weight of gems; and having hydroſtatically, and with a tender ballance examined the weight of it, firſt in the air, and then in water, I found its weight to be to that of water, of equal bulk, as two and almoſt two thirds to one: which, by the way, ſhews us, how groundleſly many learned men, as well ancient as modern, make cryſtal to be but ice extraordinarily hardened by a long and vehement cold; whereas ice is bulk for bulk lighter than water, (and therefore ſwims upon it) and (to add that objection againſt the vulgar error) *Madagaſcar* and other countries in the torrid zone abound with cryſtal.

HAVING thus found the ponderouſneſs of cryſtal in reference to water, when I met with a coloured gem, whoſe ſpecifick gravity I gueſſed to be ſenſibly greater; I ſometimes gave my ſelf the trouble (for a trouble 'tis) to weigh them in the air and in the water, and ſo diſcover, whether I conjectured aright. And if its ſpecifick gravity did much exceed that of cryſtal, I thought it a probable argument, that there might be ſome metalline or mineral corpuſcles mingled with the ſtony ones of the gems, and that alſo it may probably derive its tincture thence. I will not tell you, that I then found many ſorts of tranſparent ſtones much heavier than cryſtal: for, beſides that the tryals were troubleſome enough to make, I chanced to fall upon them in a place, where I had not any ſtore and variety of gems to examine. But one inſtance among thoſe that occurred to me, I ſhall here ſet down, becauſe, being ſo notable, it may ſuffice to ſhew, that, as to ſome gems at leaſt, my opinion of their having an adventitious gravity, and conſequently ingredient, is very probable. I had ſome American granats, which I had a great and peculiar reaſon to believe had been once liquid bodies, and therefore thought them the more worthy to be examined; and finding their colour to be ſo deep, that they were almoſt opacous, and judging by my hand, that they were much heavier than pieces of cryſtal of the ſame bulk would be, I weighed them in a pair of nice ſcales in the air, and in the water, and found them, as I expected, to be almoſt four times as heavy as water of the ſame bulk, and conſequently heavier by about a third part than, pieces of cryſtal, equalling them in bigneſs, would be. Whence ſo great an acceſſion of ponderouſneſs proceeded, I ſhall tell you, when I come to my next argument; to which I ſhall advance, as ſoon as I have noted, that though, when coloured gems have a greater gravity than cryſtal, 'tis a probable argument, that they have ſome metalline pigment, or other mineral ſubſtance, mingled with them; yet, if ſuch gems have no ſuch ſurpluſage of weight, it will not follow, that their colour cannot proceed from any mineral tincture; ſince 'tis not unreaſonable to conceive, that a mineral ſubſtance may be preſent in a liquor (ſuch as the lapideſcent juice,) that we ſuppoſe gems to be made of, even when it adds no manifeſt weight to the body, that harbours it; ſince I have obſerved (what is odd,) that a mineral water, which by its taſte, its effects, and the colour it would ſtrike, appeared to be richly impregnated with iron, being carefully by me examined hydroſtatically, did appear very little, if at all, ſenſibly heavier than common water.

The third and laſt argument, I ſhall now make uſe of, is taken from hence; that out of divers medicinal ſtones, and even out of ſome fine gems, real and corporeal metals, or other mineral ſubſtances, may be extracted.

Of this argument, I ſhall at preſent ſay the leſs, becauſe the further proſecution of it will be more proper in the ſecond part of this diſcourſe, where I ſhall be obliged to handle it, with reference to opacous gems, in which its force will beſt appear. And therefore I ſhall deſire you to take notice, when you arrive at that part of the ſubſequent diſcourſe, of thoſe particulars, that may ſerve to ſtrengthen the newly propoſed argument: and if it be objected, that the bodies, there treated of, are opacous ſtones, not gems, I have theſe things to anſwer.

First, that divers ſtones, that are reckoned amongſt precious ones, are opacous too; as the turquoiſe, the onyx, the ſardonyx, &c. not to mention divers others, as cats-eyes, opales, &c. which are as it were ſemi-opacous. Beſides I much queſtion, whether diaphaneity be abſolutely neceſſary to the eſſence, though it be to the beauty, of thoſe precious ſtones, wherein 'tis uſually found. And I might here make it probable by diſcourſe, that tranſparency and opacity oftentimes depend but upon the manner of the pigment's diſperſion thorough the ſtony matter of the gem, and the convenient or inconvenient ſituation of the pores, in reference to the beams of light. But waving this ſpeculative argument, I ſhall rather take notice, that ſeveral precious ſtones, and even diamonds themſelves, have ſometimes great clouds, which make them in thoſe parts almoſt (if not quite) opacous, without being thereby hindered from being true diamonds or gems, of this or that kind, to which their hardneſs, colour, &c. makes them appertain: and not to mention cornelians, agats, and ſome other ſtones, that we may obſerve to be (as the tinging corpuſcles happen to be, in a due or an over great proportion, mixed with the petreſcent matter, and to be uniformly or inconveniently mingled with it,) ſome of them tranſparent, and ſome of them ſemi-diaphanous; I have ſeen worn in a ring a ſardonyx itſelf, that was tranſparent, as unlikely a gem as that is to be ſo. And as for granats, though you know, that both of them are diaphanous, yet I have had ſome figured ones, that ſeemed quite opacous; and I have others by me of ſeveral countries, (whereof one very remarkable for its large ſize and geometrical ſhape,) that are in ſome places diaphanous, but, as to the main bulk of their bodies, appear at leaſt almoſt as dark as ordinary ſtones.

I further add, that I little doubt, but that experiments, not unlike thoſe I ſhall hereafter tell you, I try'd to obtain mineral or metalline ſubſtances from load-ſtones, native cinaber, blood-ſtones, &c. might ſucceed in ſeveral other of the more ponderous gems, if it were not, that the glaſſy nature, or exceeding compactneſs of many of them, makes the mineral corpuſcles, that are harboured in the ſtony and inſoluble parts, to be inacceſſible to our common menſtruums. And when the metalline and mineral ingredient is very abundant, and the tincture of the ſtony parts not ſo very cloſe, I queſtion not, but even from tranſparent gems the adventitious ingredient may, in part at leaſt, be diſſolved. And to ſatisfy you about this matter, I ſhall now inform you, that having by the ponderouſneſs of the lately mentioned kind of granats been induced to conclude them impregnated with ſomewhat metalline, and for that reaſon to think it fit to try, whether I could ſeparate it from them, or otherwiſe diſcover it in them; I kept ſome of them (in a crucible) for a competent time in the fire, and found, that they had exchanged their colour, for one not unlike that of unbrightned iron; and having reduced them to very fine powder, and digeſted ſome acid menſtruums and particularly rectified ſpirit of ſalt upon them, they afforded me a rich tincture: encouraged by which, I hoped, that, without their being previouſly burnt, they would in aqua regis afford a tincture, and accordingly I obtained from crude granats, (only reduced to very fine powder) a rich ſolution, which though in colour it ſomewhat emulated a ſolution of gold; yet partly by the colour of the burdened granats and partly by the taſte of this ſolution, I ſuppoſed, that another metal was likelier than gold to be the predominant mineral; and having gently evaporated part of that menſtruum, I obtained from ſome of the reſt certain cryſtals, whoſe ſhape, by reaſon of their ſmallneſs and diſorderly coagulation, I could not well determine; and touching with the tip of my little finger the uncoagulated portion of the liquor, this part of a drop being put to a great many drops of the infuſion of gall, did ſo immediately turn it into a ſubſtance, that ſeemed full as black, if not blacker than ink, as you would, I think, have been ſomewhat ſurprized to behold.

Which trial I made to examine the conjectures I had, that one mineral (for perhaps it was not the only, that helped to conſtitute theſe granats, was of a martial nature; which, if it were, I ſuppoſed it would, like other bodies, that participate of iron, afford with galls and inky colour. I tried alſo with a parcel of ſmall and red tranſparent ſtones, which ſome gueſſed to be granats, others, more probably, rubies, that being finely powdered, they would in an appropriated menſtruum, (made extraordinary ſtrong) give a colour like that of diſſolved gold. And that there were really ſome parts of the gem diſſolved in the menſtruum, appeared not only by the above mentioned colour, but by theſe two indications; the one, that having put ſome of this liquor to ſome of the ſame ſolution of galls, I juſt now ſpoke of, it produced indeed, at the very firſt, a dark colour, but not near ſo black as that of the granats, and in a trice let fall a copious precipitate, that was almoſt white: the other, that I was able to precipitate from it, by an urinous ſpirit, a reddiſh ſubſtance, which being ſuffered to dry in air, ſeemed to grow into bodies, in ſhape not unlike moſs, and here and there ſmall muſhrons, all of them prettily coloured. And from certain granats, that were in ſome places opacous, as well as in others diaphanous, I obtained a ſolution, from whence the ſuperfluous liquor being abſtracted, the reſidue, which was deeply coloured, did in the cold afford me a kind of ſaline concretions, which yet were not large enough to enable one to determine their figures.

AND on this occaſion, I hold it not unfit to intimate, that perhaps, if men had curioſity enough to make trials, there would be other tranſparent minerals found capable of being wrought on by appropriated menſtruums. For I do not think, that every ſeemingly glaſſy contexture of a mineral makes it unfit to be wrought on: for though the clear ſpar, which in moſt of our weſtern lead mines in *England* is found next to the metalline veins, be at leaſt ſemi-diaphanous, and be of ſo glaſſy a contexture, that it uſually breaks into ſmooth and gloſſy ſuperficies, and looks like a talk, and alſo, for the moſt part, is made up of, and preſently reducible into geometrically figured bodies, ſhaped like rhombuſſes or rhomboides; yet ſome other trials, that I have made with this ſpar, inducing me to ſuſpect, that it was not indeed a talk, but a body of a much more open texture, I found I could diſſolve it in ſeveral liquors, and particularly in good ſpirit of ſalt, which would preſently work upon it, even whilſt it was in lumps, and that without the aſſiſtance of heat; which obſervation may perhaps give ſome encouragement to ſuch a curioſity as yours.

BUT by what I have ſaid of the uſefulneſs of menſtruums, I would not have you think, that they are the only inſtruments, where with ſomething metalline may be obtained from ſome gems: for in an other paper of mine (to which ſuch trials more properly belong) you may find an account of ſome attempts of that kind by fuſions and appropriated additaments. And however ſuch trials may ſucceed with you, that aim at ſeparating from a gem a metalline or mineral body of a determinate ſpecies; I can teach you an eaſy way, whereby I have (by the help of fuſion) more than once maniſeſted in the general, that there may be ſubſtances, partaking of a metalline nature, in ſome kinds even of tranſparent gems. And partly by the ſame way, and partly by ſome others, I have been able to determine probably enough, in ſome caſes, that the mineral ſubſtance is predominant in it.

AND here, before I diſmiſs the firſt part of our eſſay, I think, I may poſſibly ſomewhat illuſtrate our hypotheſis, if I briefly mention to you an experiment, I remember I once made to that purpoſe. And it was this: I reduced to powder ſome of thoſe ſtiriæ, that I have often ſpoken of, of water petrified, as it were, ſpontaneouſly. I alſo conſidered with my ſelf, that I had found ſpirit of verdigreaſe, (which I make without the tedious preparations, that *Baſilius* and others preſcribe, by barely diſtilling, without additaments, good French verdigreaſe, and rectifying the obtained liquor) I had, I ſay, found this menſtruum to be, not only, as I elſewhere obſerve, a good ſolvent for many bodies, but alſo to be diſtillable from many of them, without leaving near ſo much of itſelf behind, as other ſaline ſolvents are wont to do: conſidering this, I ſay, I diſſolved the ſtony ſtiriæ in this liquor; and having ſuffered ſome of it to evaporate away, and put the reſt into a cool place, I obtained, as I expected, ſtore of ſmall, but finely figured and tranſparent cryſtals, that ſhot much after the faſhion of thoſe of the purer ſort of nitre. With ſome part alſo of the ſtony ſolution I mixed, in a convenient proportion, a high coloured ſolution of copper, made likewiſe in ſpirit of verdigreaſe; and theſe two ſolutions being made with the ſame menſtruum, and warily enough put together, did not precipitate one another, but afforded me, upon the evaporation of the ſuperfluous moiſture, among divers cryſtals, that were tranſparent and colourleſs, ſome, that were richly adorned with a greeniſh blue tincture of the diſſolved metal. What trials I made by this way, little varied, to imitate nature, by aſſociating into tranſparent bodies ſtony and metalline ſubſtances, I cannot now give you a full account of; ſince I neither have by me the notes I ſet down about thoſe trials, nor think it fit to make this firſt part of our diſcourſe more prolix, than I now perceive it to be already.

SECTION II.

Containing a conjecture about the cauſes of the virtues of gems.

WHAT has been hitherto delivered in the firſt part of our diſcourſe, will, I ſuppoſe, make it allowable for me to be more ſuccinct in the ſecond. I ſhall now, therefore, proceed to thoſe other conſiderations, which, being aſſiſted by what has been already ſaid, may, I hope, ſuffice, to keep our conjecture about the cauſe of the virtues of gems from ſeeming unreaſonable.

And my firſt obſervation ſhall be, that not only there is in the earth a great number and variety of minerals, already known by particular names; but probably there are very many others, that are not yet known to us.

The former part of this propoſition will not be doubted by thoſe, that conſider, how great a multitude of metalline ores, marchaſites of ſeveral ſorts, antimonies, tinned glaſs, fluores, talks of various kinds, ſpars, ſulphurs, ſalts, bitumens, &c. are mentioned partly by chemiſts, and other mineraliſts, and partly by thoſe, that have given us accounts of muſeums and other collections of natural rarities; inſomuch, that of only one kind of foſſils, the diligence of ſome modern writers hath reckoned up between two hundred and two hundred and fifty; beſides animal ſtones, as lapis bezoar, lapis manati, oculus cancri, lapis porcinus, &c.

And as for the ſecond part of our propoſition or obſervation, you will ſcarce deny it, though you conſider with me but theſe two things.

The firſt is the ſmall and inconſiderable proportion, that the perpendicular depth, that the generality of mines bears to the ſemidiameter of the earth, reckoned to be above 3500 miles; ſo that, though our globe were inhabited by ſome hundreds of millions of men more than now it is, and they had curioſity enough to dig mines every where, and conſequently there were millions of inquiſitive and laborious men, more than really there are, their ſpades and pickaxes would, except here and there, penetrate ſo little a way into the earth, that a vaſt multitude of foſſils might, by lying deeper in the bowels of it, continue undiſcovered.

And to this firſt obſervation I ſhall ſubjoin this ſecond; that, as far as I have obſerved, almoſt every region affords minerals of its own, differing from thoſe, that are taken notice of in other regions. And in particular countries, as in ſome ſhires of *England*, a curious and heedful eye may, I doubt not, obſerve ſeveral, that are not taken notice of by the inhabitants themſelves; eſpecially, if well-made borers were diligently and ſkilfully employed to pierce the ground, and bring up ſamples of divers foſſils, that lie hidden under it. But having elſewhere diſcourſed of this matter, I ſhall here only tell you, in general, that in ſome parts of *England*, where I had more opportunity than in others, to exerciſe ſome curioſity about minerals, I met ſometimes, in a ſmall compaſs of ground, with a much greater variety than I expected, and ſeveral of them undeſcribed, that I know of, by any writer; of which ſort I have received divers others from ſeveral parts, both of the old world and the new.

In the next place, I conſider, that nature has furniſhed the earth with menſtruums, and other liquors of ſeveral ſorts, and endowed it with divers qualities. This I have already manifeſted in the diſcourſe of ſubterraneal menſtruums, whereto I ſhall therefore refer you; only taking notice in this place, that whereas water is abundantly to be met with under ground, and, for the moſt part, very copiouſly in mines, by which it is capable to be variouſly impregnated; this liquor itſelf, eſpecially being thus altered, may, in ſome caſes, act the part of no deſpicable menſtruum, and, on ſome occaſions, otherwiſe concur to the production of mineral bodies.

I further obſerve, that the ſubterraneal liquors, upon one account or other, (for we need not now particularly determine it) are qualified to work, either as corroſive menſtruums, or as other ſolvents, upon many of the medicinal earths, and other minerals they meet with under ground: which minerals, having never been expoſed to our fires, have their texture more open, and their parts more ſoluble, than thoſe, that have been melted by the violent heats of our furnaces.

And that even common water will ſuffice to diſſolve and impregnate itſelf, both with the ſaline, and, oftentimes, with metalline parts, that it meets with in its paſſage, is obvious enough in the differing taſtes, and other qualities of liquors, that all paſs for common water, whereof ſome is found better, and ſome worſe than others, to brew, ſome to waſh linen, ſome to dye ſcarlet, or other determinate colours; ſome to temper ſteel, and ſome for other uſes.

But others, unqueſtionably more eminent inſtances, are given us by the mineral ſprings, whether thermæ or acidulæ, as authors diſtinguiſh thoſe, that are actually hot, (as at *Bath*) and thoſe, that are ſaline, and, for the moſt part, ſouriſh (like thoſe at *Tunbridge*, and the *Yorkſhire Spa*;) of which two ſorts, good ſtore are enumerated by phyſicians and geographers; and of which a far greater number would be diſcovered, if men wanted neither ſkill nor diligence. And here I ſhall deſire you to take notice, that, though common water do the moſt readily diſſolve the ſalts more properly ſo called, though not altogether pure, it meets with in the bowels of the earth, as we ſee it happens in thoſe ſalt-ſprings that come not from the ſea; yet there are alſo many other ſubterraneal bodies, which, upon the ſcore of their abounding with ſaline particles, will be diſſolved by water, though they be of a compounded nature, and contain very differing ſubſtances; as it is plain in thoſe waters of *Hungary*, and other regions, which, by the evaporation of their ſuperfluous moiſture, will yield vitriol, a mineral not only compounded, but decompounded, as containing in it a ſaline, a ſulphureous, a metalline, and an earthly part, (which, itſelf, I have found to be none of the ſimpleſt bodies;) every one of which may be made diſtinctly to appear.

Lastly, I conſider, that the petrific juice or ſpirit coming to be in a ſufficient proportion mingled with theſe impregnated waters, ſo as to coagulate them, and concoagulate with them; from their coalition may reſult thoſe precious ſtones, that we call tranſparent gems. For it is certain, that bodies, that were a while before in the form of waters, may coagulate into ſtony ſtiriæ, of whoſe odorouſneſs and reducibleneſs into lime, I have already given an account in my diſcourſes of lapideſcent juices; of which you may command a ſight. And that even diamonds themſelves, the hardeſt of gems, were once fluid ſubſtances, the firſt part of this diſcourſe has, I hope, evinced.

To which I ſhall now add, that procuring ſome petrified bodies to be brought me from a place in *England*, which I could not be admitted to, I found, that the petrific juice or ſpirit, that abounded in the earth of that ſpot of ground, was ſo penetrating, and ſo operative, that it made ſome of the vegetable ſubſtances, that were found in it, in their priſtine ſhape, and, for ought I could perceive, bigneſs, hard enough to cut glaſs, as well as grave on iron. And it was among theſe rarities (if I much miſ-remember not) that I picked up a (moderately) tranſparent body (which, I think, I have yet by me) that, by the ſhape, and other circumſtances, I judged to have been a diaphanous gum, belonging to one of the pieces of petrified wood, that had been brough me, and was hardened to a degree, that made it capable of ſcratching glaſs.

AND now to bring home theſe things to my preſent ſubject, I conceive, that ſome, at leaſt, of the real virtues of divers gems, may be derived from this, that whilſt they were in a fluid form, or, at leaſt, not yet hardened, the petreſcent ſubſtance was mingled with ſome mineral ſolution or tincture, or with ſome other impregnated liquor; and that theſe were afterwards concoagulated, or united and hardened into one gem, as a diamond, a ſapphire, a granat, an onyx, a blood-ſtone, &c. And as divers of the virtues of gems may be, in a general way, deduced from the commixture of theſe mineral corpuſcles; ſo the greatneſs of thoſe virtues, and the variety of thoſe properties, in particular, may be aſcribed to the peculiar nature of the impregnating liquors, to the diverſity of them, and to the greater and leſſer proportions, wherein they are mixed with the petreſcent juice.

To render this conjecture (for I propoſe it as no other) thus ſummarily and briefly expreſſed, the more probable, it will be fit to recall to mind the arguments, whereby we have already ſhewn, both that gems were once fluid or ſoft bodies, and that divers of them were not ſimple concretions of a petreſcent liquor, but conſiſted alſo of other mineral adventitious corpuſcles: which may appear, partly by the ſeparableneſs of ſuch ſubſtances from ſome gems, (as we exemplified in granats) partly by the ſpecific gravity of others, and partly by the differing tinctures (whereof one, at leaſt, may well be ſuppoſed adventitious) to be met with in gems of the ſame ſpecies, as rubies, ſapphires, granats, and even the hardeſt ſtones, that we yet know of, diamonds themſelves; of which (as is before noted) I have ſeen ſome yellow, and that to a great degree, ſome of other colours, but not ſo vivid; and ſome green, almoſt like emeralds.

Now ſince there may be in gems, and, in ſome of them, abundantly ſuch adventitious corpuſcles; and ſince there is cauſe to think, that ſome may be endowed with divers properties, and medicinal virtues; ſince alſo there is a great difference among theſe impregnating particles, and, probably, of a greater variety of them, than is known to us; ſince, laſtly, divers gems are not ſparingly, but richly impregnated with thoſe ennobling corpuſcles, I ſee no ſufficient reaſon, why ſome of the virtues of divers gems are not more likely to proceed thence, than from thoſe unintelligible and precarious ſubſtantial forms, to which they are wont to be referred.

BUT becauſe there are ſome difficulties, that the objections of others, or my own thoughts, have ſuggeſted againſt our hypotheſis; though I neither have time, nor do think it very neceſſary, to diſcourſe amply of them: yet to clear the way for what I am afterwards to repreſent, I ſhall (though I can but briefly do it) ſay ſomething to each, that may, perhaps, appear no inſufficient anſwer; eſpecially after I have declared, as I here do once for all, that I ſpeak of the true and medical virtues, that belong to gems; and that, as to thoſe magical, and other extravagant properties, that either notoriouſly fabulous, or other credulous writers have made bold to deliver, I am ſo far from pretending to afford them an explication, that I do not allow them the leaſt degree of aſſent.

THIS premiſed, let us conſider the chief difficulties themſelves; among which, I doubt not, but it will be objected, that it is not credible, that the mineral ſubſtances, wherewith our hypotheſis would have gems to be impregnated, ſhould have any medical operation at all on the human body, in regard, that they are ſo locked up, that they can communicate nothing to it, eſpecially being indigeſtable and unconquerable by ſo ſmall a heat, as that of the ſtomach and other parts of the body.

BUT to this ſpecious objection I have ſeveral things to return by way of anſwer. And firſt of all; had there yet never been any actual trial made, whereby to know, whether a gem be capable of having any medical virtues, I confeſs I ſhould find probability enough in the objection to ſuſpend my judgment, till experience ſhould determine the queſtion. But ſince upon the very credible teſtimony of eminent phyſicians and patients themſelves of my own acquaintance, I find much leſs cauſe to diſbelieve, than to aſſent to ſome matters of fact about the operations of gems; and ſince ſuch matters of fact do ſtrongly argue, in the general, that a precious ſtone may have medical virtues; I think, the objection, as it is propoſed in general, is ſufficiently enervated by ſuch particular inſtances, and ought not to keep us from believing upon experience the poſſibility of the thing denied; eſpecially ſince there are other things beſides, that may be alledged in favour of our hypotheſis.

FOR it may be conſidered in the next place, that vigorous load-ſtones emit copious and very plentifully effluvia; and yet, beſides that ordinary magnets are uſually a very hard ſort of ſtones, I have met with ſome load-ſtones much harder than ordinary ones, and poſſibly than divers gems. And it is farther conſiderable, that there are load ſtones, (ſome of which I can ſhew you) which do not only work upon iron, and other magnetical bodies, but have a manifeſt and inconvenient operation upon human bodies, by being worn in men's pockets, or long held in their hands; as thoſe, that have reſented ſuch operations themſelves, and obſerved them in others, have complained to me; which I might confirm by ſome analogous obſervations, if I had time to relate them.

But now I proceed to obſerve, that among tranſparent pebbles, ſome of which, you know, are, by being barely well cut and ſet, made to counterfeit diamonds, I have found ſeveral, that may be brought, in a trice, to emit copious, and even ſtrongly-ſcented ſteams. And if you allow the opinion of the generality of modern philoſophers, who aſcribe electrical attractions to the effluvia of bodies excited by rubbing, you will, I preſume, allow me to infer, that very light alterations may ſuffice to procure expirations, even from tranſparent gems; many of which are electrical, and ſo are the hardeſt of them, diamonds themſelves; one of which I keep by me, that, upon a little friction, attracts vigorouſly enough to be wondered at by the ſpectators.

And as to that part of the objection I am anſwering, which contends, that gems are not to be digeſted or conquered by the heat of the ſtomach; I will not ſtay to examine, whether, and how far, the digeſtion of things in the ſtomach be to be aſcribed to heat, contenting myſelf to ſay at preſent, that, to make the objection valid, it ſhould be firſt proved, that ſuch bodies cannot have any operation upon the human body as paſs thorough it, without any ſenſible change of bulk, figure, &c. as gems, that are ſwallowed down, are ſuppoſed to do. For, we know, that ſome chemiſts make bullets of the regulus of antimony (which we alſo have made, and obſerved ſomething odd about them) which they call pilulæ perpetuæ, becauſe, when they have performed their operation in the body, and have been ejected with the excrements, they are by ſome more thrifty, than cleanly perſons, waſhed, and employed again and again to the former purpoſes. Nor do we know, what analogy there may be between ſome juices in the body, and ſome of the mineral ſubſtances, that impregnate gems with their virtues.

For, though the oculus mundi be reckoned, by claſſic authors, among the rare gems (as indeed good ones may be juſtly accounted rarities;) yet, if one of the beſt ſort be but a while kept in common water, it will, as experience aſſures me, receive an alteration obvious to the eye. I might here alledge the concurrent authority of many, and the common practice of moſt phyſicians, who, in their publick diſpenſatories, as well as private preſcriptions, ordain the fragments of precious ſtones to be taken inwardly, upon the ſcore of the cordial, and other virtues, they aſcribe to them. But I ſhall rather make uſe of leſs queſtioned arguments, and, without inſiſting on the manifeſt operation, that the juices of the body have not only on the chalibeat preparations, where the metal is preſumed to be opened, but upon crude ſteel itſelf; or urging the examples of Lazarus vitri-vorax, or the devourers of ſtones, as being rare ἰδιοσυγκρασίας; I ſhall proceed to acquaint you, that with a faint liquor, diſtilled from a vegetable ſubſtance, as temperately qualified, and as plentifully eaten as bread, I have obtained, and that without heat, from divers hard bodies, and amongſt them, from a tranſparent ſort of gems, a manifeſt tincture. And whether ſome juices of the body, aſſiſted by the natural heat of it, may not, in reference to ſome gems, ſerve for extracting menſtruums, though it may well be more than either I, or the objectors, certainly know, yet the inſtance, I come from alledging, favours our hypotheſis more than theirs.

And even the natural heat of a human ſtomach, nay, perhaps, the outward parts of the body may be able, though not to digeſt precious ſtones, yet to ſolicit out ſome of their virtues; ſince I am ſure, it makes a ſenſible alteration in the hardeſt ſort of them. For I have a diamond, whoſe electrical faculty may be excited not only by rubbing, but, without it, by a languid degree of adventitious heat. And I have had in my keeping a diamond, which, by water made a little more than lukewarm, I could bring to ſhine in the dark.

Object. If it be further alledged, that, though ſome virtues may be conceded to gems upon the account of the minerals, that impregnate them, yet it will be no way likely, that their virtues ſhould be ſo various and great, as even the modeſter ſort of authors pretend: if this, I ſay, be alledged, I ſhall readily acknowledge, that I do not think others, or myſelf, obliged to believe all the ſtrange things, that even ſome learned writers do ſometimes aſcribe to gems: And if any man will think, that ſome of them are fabulous, and more of them hyperbolical, he may ſooner find me his aſſociate, than his adverſary in that point. For the rarity of tranſparent gems, their luſtre, and the great value, which their ſcarceneſs, and men's folly ſets upon them, imboldens ſome to ſay, and inclines others to believe, that ſuch rare and noble productions of nature muſt be endowed with proportionable, and conſequently, with extraordinary qualities.

But this being freely granted, I anſwer to the objection; firſt, that 'tis not improbable, that there may be in the earth a much greater variety of minerals diſſoluble by the ſubterraneal menſtruums, and capable of concoagulation with petreſcent juices, than authors have yet taken notice of: to which conjecture divers ſubterraneal productions, that I have met with, do ſtrongly incline me. And from the number and various mixtures of theſe may proceed, not only a great variety of operative particles in precious ſtones, but a high degree of energy in ſome of them.

And next I conſider, that the efficacy of thoſe mineral tinctures or ſolutions, that are already known to us, and may be concoagulated with the petreſcent juice, may be reaſonably preſumed to be much greater in ſome gems, whereof they became ingredients, whilſt they were (as chemiſts ſpeak) *in ſolutis principiis*, than may be expected in our ſhops, or laboratories, from the vulgar ſolutions of the ſame metals or minerals, after they have, by vehement fires, been reduced into gold, or ſilver, or lead, or antimony, &c. For whereas, in theſe vehement fuſions, requiſite to bring metalline or other ores into ſuch ſubſtances, the volatile and ſpirituous parts are wont to be driven away, and the remaining body becomes more hard and compact, and has his virtues, as it were, locked up; in the ſtate of fluidity thoſe ſubtle and efficacious parts are preſerved, and united to the other ingredients of the gems, whence ſome emanations of them may be eaſily enough drawn out; as in the inſtance I not long ſince mentioned, of the eaſy eduction of ſtrongly ſcented ſtreams from pebbles ſo hard, that I found them more diſpoſed to ſtrike fire, than flints themſelves, that are uſed in guns. And from the greater or leſs plenty, and natural activity of the impregnating particles in this or that gem, may, probably, be deduced the difference in colour of ſome, and in virtue of other ſtones of the ſame denomination: of which we have, in a learned writer or two, eminent examples given us, of the great virtue of ſome, and the inefficacy of other, that experience has diſcovered, among thoſe ſtones, that go under the title of lapis nephriticus. For, though they be not properly tranſparent gems, yet the analogy betwixt them, and thoſe that are, ſeems ſufficient to warrant the mentioning of them on this occaſion.

See *Unzerus* de nephritide.

And here we may ſubjoin two things, in favour of both the foregoing anſwers: the firſt, that for aught we know, the petreſcent juices themſelves may have all, that is requiſite to make them ſuch, and yet have diſtinct natures, and be endowed with peculiar qualities, abſtracting from thoſe, which they acquire upon the ſcore of their coalitions with adventitious liquors. This I cannot ſtay to make probable by the differences I have obſerved in petreſcent fluids, and therefore I haſten to the ſecond.

The next thing, which I would repreſent, is, that having obſerved petrific liquors, or ſpirits, to pervade and give a high degree of hardneſs to bodies, that chanced to lie within their reach, though one would have thought them ſufficiently indiſpoſed to receive ſuch an induration; I ſee no abſurdity in ſuppoſing, that, ſometimes, ſuch a liquor may invade, permeate, and ſubdue tranſparent minerals, abounding in ſaline, ſulphureous, and bituminous particles; which conſequently being duly excited, may be made to emit their more ſubtle and more active parts. And as I have cauſe to think, that ſubterraneal fires and menſtruums do divers times make various compoſitions and decompoſitions in the earth, (as it were not hard for me to ſhew, if I had leiſure;) ſo it is not impoſſible, but that the ſpirit, we have been ſpeaking of, ſupervening, may mingle itſelf with ſuch bodies, and petrify them together with itſelf into gems. On which occaſion I remember, that I have had ſalt, made by nature in the bowels of the earth, juſt like that, which chemiſts compound by art on the ſurface of it. And I have ſometimes made, by an eaſy operation, and a moderate degree of fire, a certain compoſition of volatile particles of ſalt and ſulphurs (ſome of which I have yet by me) which, after diſtillation, did, in a fluid medium, ſhoot into cryſtals tranſparent, and more curiouſly figured, than I have ſeen divers natural gems to be. So that, if either beneath, or upon the ſurface of the earth, ſuch kind of ſubſtance happen to be pervaded and ſubdued, by a clear petrifying liquor; we may well preſume, that the reſulting concretions may be indued with qualities, as well uncommon for the kind, as conſiderable for the degree.

Object. If it be yet objected, that it is very unlikely, that gems ſhould part with any effluvia, or portions of themſelves, ſince they loſe not of their weight, and ſome of them are very little heavier than cryſtal itſelf, and conſequently are not like to have much adventitious ſubſtance to part with; I might leave the anſwering of one part of the objection to phyſicians and chemiſts, who teach, that the antimonial glaſs and cup imbue wine, and other liquors, with a ſtrong emetic quality, without any ſenſible loſs of weight. But having elſewhere ſpoken of thoſe things, I ſhall rather here demand, whether the objectors have tried the truth of what their argument ſuppoſes by any way ſufficiently accurate? For I much doubt, that that has neither been attempted, nor would be found eaſy to be performed. And till due trial be made, let me repreſent, that though they will not allow common water to be a menſtruum fit to draw any thing with, from ſuch a body as mercury, which is wont to mock the chemiſts aqua fortis and aqua regis; yet both *Helmont*, and others, inform us, that mercury kept for a day or two in common water, or boiled a while in it, though it be taken out without any ſenſible diminution of weight, or bulk, will have imbued a conſiderable quantity of water with a virtue of killing worms; for which purpoſe, it is much uſed, and often with good ſucceſs, in a great hoſpital in *London*, as the chief phyſician of it (a very judicious and experienced man,) has more than once informed me.

And as for the lightneſs, that is objected againſt ſome gems, beſides that it may ſafely be granted, that, *cæteris paribus*, ſuch may have fewer or more languid virtues, than others of the ſame kind; it may alſo be anſwered, that the adventitious ſubſtance, that impregnates the petreſcent juice, may be of ſo ſmall ſpecifick gravity, as not to make the gem at all heavier in ſpecie, than cryſtal itſelf. For this, (as we have formerly obſerved,) being about two times and a half heavier than common water of the ſame bulk, I have hydroſtatically found, that divers ſalts and ſome other mineral ſubſtances are of leſs ſpecific gravity; and conſequently, if they were concoagulated with the petreſcent juice, that hardens into cryſtal, need not increaſe the ponderouſneſs of it, and yet may embue it with conſiderable virtues: nor is it neceſſary (to add that *in tranſitu* on this occaſion) that, not to alter even the colourleſneſs of cryſtal, or the colour of another gem, the adventitious ſubſtance ſhould be purely ſaline: for I have divers times made bodies, which though tranſparent and colourleſs like cryſtal, and ſometimes curiouſly and regularly figured, were yet of a compounded nature, and particularly abounded with an eaſily ſeparable and ſtrongly ſcented ſulphur. But to give yet a farther and more direct anſwer to the objection; I ſhall add, that though when a gem has much more ſpecifick gravity, than cryſtal, or will ſuffer an adventitious mineral to be ſeparated from it, it is a very probable argument, that the petreſcent juice is that body compounded with an adventitious ſubſtance; yet it will not neceſſarily follow, that, when neither of theſe ſigns appear, the gem is quite devoid of any ſuch ſubſtance. For, (according to what I elſewhere declare,) the petreſcent liquor, it mainly conſiſts of, may be impregnated, not with the groſſer ſubſtance, but with the finer and more ſpirituous part of the mineral, without having the ſpecifick gravity ſenſibly encreaſed. Of which I remember, I ſhewed a notable inſtance to ſome curious perſons, at a mineral ſpring, which many were then drinking of by the advice of learned phyſicians for ſeveral diſeaſes. For, though this water, both by its inky taſte, by its blacking the excrements of thoſe, that drank it, and by other ſigns appeared to participate richly enough of iron; yet the ferruginous particles, it abounded with, were ſo light and ſpirituous, that not only they would, as I tried, be eaſily loſt, if the liquor were kept too negligently ſtopped; but when I came, whilſt the ſpirits were yet there, (it being but newly taken from the ſpring it ſelf) to examine it hydroſtatically with very good ſcales, and much diligence, I convinced the virtuoſi, that aſſiſted, that this ferruginous water was very little, if at all, heavier in ſpecie than other water, which was brought as common water to be compared with it, and examined with the ſame ſcales, and after the ſame manner.

AND now, if you recal to mind what I have elſewhere ſaid, partly of the atmoſpheres of ſolid bodies, and partly of the great efficacy of effluviums; I hope you will not think it abſurd to conjecture, both, that ſome precious ſtones may have medical virtues, and, that divers of theſe may be aſcribed to the mineral ſubſtances, whereof they participate or conſiſt; and eſpecially to thoſe, which are beſt fitted to exert their powers, by the copious effluxions of their more agile and ſubtle parts.

AND by this time it may be ſeaſonable to tell you, that though what I have hitherto diſcourſed do chiefly belong to tranſparent gems; yet divers of the things already delivered may, with no great alteration, be applied to opacous gems: of which I ſhall ſpeak much more briefly, not only for the reaſon juſt now given, but becauſe, if we have ſhewn, as I hope we have, that even diaphanous gems may be endowed with virtues by the mineral ſubſtances they contain or are in part, made up of; the arguments will hold more ſtrongly as to opacous gems: both becauſe theſe are, for the moſt part, much leſs hard than the others, and becauſe it is far more eaſy to ſhew, by their ſpecific gravity, and the compoundedneſs of divers of them, that the dark ones, than it is, that the clear ones, may partly, and ſometimes plentifully, conſiſt of mineral ſubſtances, embodied with, and hardned by petreſcent juices, or petrific ſpirits.

IN favour of this doctrine, I ſhall endeavour, in the firſt place, to ſhew, that what has been delivered is poſſible; and afterwards, ſet down ſome particulars to make it very probable.

THE firſt part of my taſk might be eaſily performed, or, perhaps, would be needleſs, if I were ſure you had no need to be told of any thing I have written about lapideſcent juices. But for greater ſecurity I ſhall, in this place, briefly intimate, that, among the kinds of thoſe liquors, I have obſerved a ſort, that is of ſo fine a ſubſtance, and yet of ſo petrifying a virtue, that it will penetrate and petrify bodies of very differing kinds, and yet ſcarce, if at all, viſibly encreaſe their bulk, or change their ſhape or colour. To which purpoſe, I remember, that I have ſeen divers animal and vegetable ſubſtances ſo petrified, as ſcarce, at all, to be taken notice of, by their appearance to have been altered by the operation of the petreſcent liquor. I have, with pleaſure, ſeen a thin cream-cheeſe turned into ſtone, where the ſize, ſhape, and colour, even of the wrinkles, and the blueiſh mold (which, it ſeems, it began to have, when the liquor invaded it) were ſo well preſerved, that an hungry man would not have ſcrupled to have fallen upon it for a good bit. And as for the hardneſs, that this petreſcent juice can give to the body, that it penetrates, I ſhall now only remind you of what I lately told you; that I have had, and, I think, yet have, in another place, a pretty quantity of wood petrified in *England*, which retaining its former figure, and grain, and ſcarce at all viſibly encreaſed in bulk, was ſo very hard, that I could make impreſſions with it upon iron, and glaſs it ſelf, and make it ſtrike fire like an excellent flint. To which I ſhall here add, that the ſtony parts did not ſuffer the wood, which they had penetrated, to be reduced in the fire, either to aſhes or charcoal. And I have by me a lump of mineral ſubſtances, wherein a petreſcent liquor, that fills the large intervals between them, is tranſparent enough, and harder than moſt ſtones, as far as we could gueſs by ſome trial of it made by a ſkilful engraver of gems.

AND to theſe inſtances might be added many others, if it did not by theſe few ſufficiently appear, that petrifick agents may inſinuate themſelves into the pores of various bodies, and turn them into ſtone, without otherwiſe deſtroying their priſtine nature, or ſo much as their former figure.

WHEREFORE having in general ſhewn our hypotheſis to be poſſible, we may now deſcend to four or five particular arguments, that it is hoped may help to render it very probable. And theſe I ſhall fetch, partly from the great ſpecific gravity of divers opacous and medicinal ſtones; partly from the fitneſs of our hypotheſis to render a reaſon of divers phænomena relating thereunto, ſome of them ſcarce at all, and others much leſs probably to be accounted for without it; partly from the metalline ſubſtances to be manifeſtly ſeparated or obtained from the ſtones we are treating of; and partly from the nature of the bodies, whereof medicinal ſtones ſeem to be compounded.

Arg. I. THAT the ſpecific gravity of divers opacous ſtones, whereunto medicinal properties are aſcribed, is very conſiderable, is a truth, which if thoſe, that have written of ſuch concretions, had been verſed in hydroſtaticks, and had had the curioſity to examine them that way, they might have eaſily diſcovered; as will quickly appear by particular examples: before the mention whereof, it will be fit for me to take notice to you, that conſidering with my ſelf, that white marble is generally allowed to be a pure and ſolid ſtone, and upon the ſcore of its whiteneſs is likelier, than moſt others, to be free from mineral mixtures; I thought, I might at leaſt as well pitch upon that, as on any other, for the ſtandard of the ſpecifick gravity of opacous ſtones, as they are merely ſuch. And accordingly having weighed a piece of white marble in air and water, I found it to be in weight to an equal bulk of that liquor very near $2\frac{72}{100}$ to 1, or, that the proportion with very little error may be the better remembered, as two and ſeven tenths to one. And to make trial in a ſtone uncoloured, but, becauſe harder, ſuppoſed to be of a cloſer texture, we examined a fine white pebble, which we found to be to an equal magnitude of water, as two and above ſix tenths to one. This being determined, it was not difficult for me to think, both that divers bodies, that commonly paſſed for mere ſtones, are more ponderous than white marble of the ſame bulk; and that if there were any ſuch great ſurpluſage of ſpecific weight, as I gueſſed, many will be found to have above that of marble, it might proceed from ſome metalline body, though not viſibly, yet really, and perhaps plentifully mingled with the petreſcent matter of theſe ſtones. The latter part of this conjecture will hereafter be confirmed in the third argument; which makes it unneceſſary for me to give you now of the former more than a few inſtances: which I ſhall ſoon diſpatch by telling you, that I quickly found by weighing the following minerals, firſt in the air, and then in the water, that a blood-ſtone (bought at the druggiſt) was in weight to water of the ſame bulk at $5\frac{7}{10}$ to 1; the loadſtone, I then tried, (for all are not equally heavy in ſpecie) as 4 and $\frac{6}{10}$, to 1; lapis calaminaris, uſed for rheums in the eyes, and to turn copper into braſs, as $4\frac{7}{10}$ to one; lapis tutiæ, as they call it, which is alſo much employed in rheumatick eyes, as very near 5 to 1.

BUT here I muſt advertiſe you, that I have not found the proportion of each of theſe bodies and water to be any thing near conſtantly the ſame, but ſometimes to differ very much in particular ſtones of the ſame kind; which agrees very well with our hypotheſis. For, according to that, thoſe particular ſtones, that happen to partake more plentifully of mineral ſubſtances, heavier in ſpecie than ſtone, as ſuch needs to be, ought to be more ponderous than others of the ſame kind, that are not ſo qualified: I ſaid, heavier in ſpecie than a ſtone, as ſuch need to be, becauſe there are ſubſtances, that are reckoned among minerals, and are capable of endowing the ſtony matter, wherewith they are coagulated, with medical virtues, and yet thoſe ſubſtances may make the ſtone or aggregate, whereof they are made, not to be heavier, but lighter in ſpecie. From jet, which in ſome parts of *Europe* being found in quarries of mines, is indeed a foſſil, which is wont to be reckoned among ſtones, and by many worn as a gem, I obtained no inconſiderable proportion of oil; and having weighed choice jet itſelf in water, I found it to be, bulk for bulk, to that liquor, but as $1\frac{22}{100}$ to 1. And there are ſome other foſſils, hard as ſtone, and poliſhable as marble, from which I have by diſtillation obtained two kinds of oil, whereof one was lighter than common water; which ſhews, that even bituminous and light ſubſtances may be ingredients of a ſtone: and that ſalts, which are moſt of them leſs heavy in ſpecie than white marble, may plentifully concur to the making up of ſtones, I ſhall have occaſion to manifeſt at the cloſe of this diſcourſe by thoſe ſtones, whereof we in *England* uſe to make vitriol. The foregoing reflection I have here touched upon, becauſe I would intimate to you, that ſtones, that are lighter in ſpecie than white marble, may be compounded of foſſils, whence they may derive peculiar qualities, at the ſame time, when I tell you, that in my opinion ſuch ſtones, as are conſiderably more heavy in ſpecie than marble, may afford us a ſtrong preſumption of their owing their gravity to the mixture of metalline or mineral ſubſtances. And this may ſuffice for our firſt argument.

Arg. II. THE next ſhall be taken from the conſideration of ſome phænomena, (relating to medicinal ſtones) which agree very well with our hypotheſis, and will ſcarcely be very well explicated without it.

AND, firſt, as to tranſparent gems themſelves, I have learned by inquiry of travellers, that have viſited thoſe parts of the *Eaſt Indies*, where they grow, that ſometimes one ſort of gems, ſometimes another, and ſometimes alſo diamonds themſelves, are found included in the rocks where they are digged for, or in the midſt of hard looſe ſtones, which muſt be broken in pieces, to take out the diamond, or other incloſed gem: which phænomenon will be hard to be accounted for, unleſs by our hypotheſis; according to which it may rationally be ſuppoſed, that the gem was firſt formed either in earth, or ſome other ſoft and eaſily permeable ſubſtance, which being afterwards pervaded by ſome petrific juice or ſpirit, was turned into rock or looſe ſtones, according as the earth, and other ambient matter, chanced to be an intire and coherent maſs, or divided into clods and other portions. And I remember, that the governor of an American colony, having ſent me among other rarities, digged up in his country, an odd kind of mineral, that ſeemed more ponderous than at firſt ſight it promiſed, I had the curioſity to break it, and found in it, here and there, ſeveral gems, which by their figurarion and ſome other circumſtances, were concluded to have been formed there, before the ambient mineral had obtained the nature it then appeared to be of. And in opacous ſtones, it may hence happen, that a great lump of medicinal earth may be invaded and petrified after the newly mentioned manner; ſo that it may not be thought incredible, that ſome of theſe medicinal ſtones ſhould be very large in compariſon of others: As I remember, that an ingenious phyſician told me of a ſpleen-ſtone, as they call them, in the hands of an acquaintance of his (where I might have ſeen it, if my occaſions had permitted,) amounting to about fourſcore pound weight. And on this occaſion, I alſo remember, that even in a medicinal ſtone, much harder and heavier than marble, and whereof I have ſeen lumps far greater than I could lift; I remember, I ſay, that having had the curioſity to cauſe a pretty big piece, violently broken off from the maſs, whereto it belonged, to be ſawn aſunder, that I might conſider the internal textures, as far as it was viſible; I found ſeveral empty cavities of differing ſizes and figures in the ſolid ſubſtance of the ſtone, (which I think I have not yet loſt:) which ſeems to argue, that this compact and ponderous body was made of a ſtony nature, by the ſupervening of ſome petreſcent liquor or ſpirit upon porous earth, or ſome other conſiſtent ſubſtance. For if it had been a mere liquor, wherein thoſe cavities muſt have been ſo many aerial bubbles; it is not like, that ſome of them ſhould have ſuch irregular ſhapes, and that all ſhould have continued, without emerging to the top.

Secondly, our hypotheſis will alſo help to render a reaſon of what ſeems exceeding difficult to be explicated; namely, how ſome gems, that ſeem to be intire ſtones, are in part of one colour, and in that, which is contiguous to it, of a quite differing: of which ſort we have the ſardonyx, and ſome other opacous gems. And I have obſerved the like, though very rarely, in diaphanous ones. For, according to our hypotheſis, it may be ſaid, that a portion of matter, imbued with one of the tinctures of the parti-coloured gem, was firſt formed, and afterwards, ſome petreſcent juice, endowed with another colour, came to ſettle contiguouſly to it, and ſo by accretion made up one ſtone with it. I might illuſtrate this by telling you, that though fire do make a far greater agitation of bodies melted by it, than need be ſuppoſed in cold petreſcent liquors, yet I have found in making artificial gems, that by ſome miſchance or error in the operation, the mineral pigment has richly tinged one part of the tranſparent maſs, without at all imparting that colour to the very next part to it; ſo that if I ſhould ſhew one of thoſe I have yet by me, you would judge it to conſiſt of two differing gems ſubtlely glewed or faſtened together, unleſs you ſhould in vain try, as others have done, to diſcover by the eye or otherwiſe, ſome naked commiſſure, which may keep thoſe ſo differingly coloured bodies from making up one intire maſs.

But let us leave theſe artificial gems, and add to what I was ſaying about our natural ones, that the union of parts in theſe reſulting ſtones (if I may ſo call them) I was ſpeaking of before, might be the more perfect, if the ſupervening matter found not the firſt formed ſtone to have attained to its full induration: though, for aught I know, even in this caſe, the appoſition may be ſo cloſe, and the two matters ſo near of kin, that both may paſs for one ſtone, and be poliſhed both together, without any blemiſhing diſcontinuity of ſurface at thoſe parts, where one would expect commiſſures. For I have by me a lump, wherein there plainly appear ſtones of colours very different from each other, that were once diſtinct and incoherent; but by ſome petreſcent liquor have had all their intervals ſo exquiſitely filled up, that neither the touch, nor the artificers tool, the lump being now ſawn aſunder, diſcovered any commiſſures; but the whole maſs bears an uniform poliſh, and is harder than divers gems, that are worn in rings, readily enough ſtriking fire with a ſteel. And to confirm this the more, I, ſhall add, that in a place, where a prying perſon of my acquaintance lighted on this portion of petrified matter, he found not only other lumps, but divers looſe ſtones, that ſeemed altogether of the ſame nature with thoſe, that by the ſupervention of the petreſcent liquor were united into ſtony maſſes. I have alſo had a curious agat ſo formed, that it ſeemed highly probable, that the opacous parts of its matter had been ſome thin, but not altogether contiguous, beds of fine clay, or earth, lying almoſt parallel to each other (but not to the horizon) which by ſome petreſcent liquor, that chanced to ſettle there, was reduced to coagulate with it into a partly opacous and partly diaphanous ſtone. And of ſuch clays or mineral earths, I have ſometimes with pleaſure obſerved more than one or two, which, though diſtinct, and perhaps of differing colours, were ſo very thin, that the thickneſs of them all did ſcarce exceed an inch, nor did they always lie flat or horizontally, but in differing poſtures, both in reference to the horizon, and one another, and now and then the exterior ones did ſucceſſively almoſt ſurround the interior: and of theſe thin couches or layers of earth, I remember, I have obſerved a conſiderable number within a very ſmall compaſs of ground. I muſt not in this place ſtay to ſhew, how probable it is, that much after the ſame way may be explicated the production of divers other gems beſides agats, as chalcedonians and jaſpers, which are for the moſt part opacous, but oftentimes have ſome parts, that are not ſo. But I am content, before I go further, to mind you, on this occaſion,

ſion, of what I elſewhere deliver, that by purpoſely calcining, without breaking, ſome of theſe ſtones, whoſe greater part was diaphanous, I found, that the tranſparent parts turned white; and that ſome of the thin layers, or couches of mineral earth, had retained their colour, as well as poſition, and had it much heightened; ſo that one of theſe layers, after calcination, was of a very rich and permanent red. And this difference of colours I obſerved not only in layers, but in the ſpecks, and irregularly ſhaped clouds, if I may ſo call them, of other colours, as greeniſh, blueiſh, &c. I might here add, that I have found ſhining marchaſites, not only in other ſolid ſtones, but in marbles; as alſo flints themſelves, incloſed in great maſſes of marble, and likewiſe wood; in ſtrong ſtones imployed to build a wall, and ſhells, at leaſt as was judged by their ſhapes and ſizes; in a great maſs of ſtone, that I met with almoſt on the top of a hill remote from the ſea, together with divers other ſuch phænomena, which I think may probably be accounted for by our hypotheſis, and ſcarce without it. But being willing to diſpatch this diſcourſe, and unwilling to intrench upon the diſcourſe of the effects of the petreſcent juice, to which the conſideration of theſe and divers other phænomena, to be met with about the generation of ſtones and petrified bodies, eſpecially in wombs or molds, more properly belongs; I ſhall in this place only point back to one obſervation, and anſwer one objection; becauſe both of them are pertinent to our preſent diſcourſe.

THE obſervation is this: that even in tranſparent gems, and which is more, of the ſelfſame ſpecies, I have ſometimes taken notice of ſuch an aggeneration, or accretion of ſtones, to one another, as argues their having been produced at ſeveral times. For proof of this, I need no more than refer you to what I have not long ſince related about thoſe Corniſh diamonds, wherein, ſometimes, a leſſer ſtone, though geometrically ſhaped, was found in good part incloſed in a greater, as well as in part alſo extant above it. Whence I argued, that the production of this aggregate of two cryſtalline bodies was not made all at once, but ſucceſſively, and that the leſſer was firſt formed, which I ſhall now confirm by this conſideration. That if the greater ſtone had been firſt hardened, the matter of the leſſer muſt only have exteriourly ſtuck to it, and been, as it were, imboſſed upon it; but could not have made itſelf, in the ſubſtance of the greater, a bed or mold, eſpecially of ſuch a geometrical figure as itſelf had not yet received.

AND though this ſucceſſive generation of the parts, of ſeemingly entire gems, may appear to you ſomewhat new and ſtrange; yet, that its fitneſs and requiſiteneſs to explain the foregoing phænomena, and others, to be hereafter mentioned, may the more recommend it to you; I ſhall add, that, perhaps, you may be aſſiſted to conceive, if not invited to admit it, by a mechanical illuſtration. For we ſee, in divers chymical ſolutions, as of ſalts, and other bodies, that there are certain ſtages, or periods, of coagulation; ſo that, when ſuch a quantity of the ſuperfluous moiſture is exhaled, eſpecially upon any conſiderable refrigeration, or other favourable circumſtance, thoſe particles, that are moſt diſpoſed to coagulation, will convene, and ſhoot into cryſtals; after which, no more will do ſo, till a farther and more conſiderable evaporation of the water, or other menſtruum, be made; upon which will enſue a new cryſtallization of the parts. And I can ſhew you the productions of a metalline, but uncommon ſolution, that I ſo made in an appropriated liquor; that the firſt ſhooting afforded me a layer, or bed of curiouſly figured cryſtals, and the following another layer of fine cryſtalline bodies, that have faſtened themſelves to the former, but differ notably from them, both in ſhape and poſture. And in this experiment, the diſſolved body was but one, as the menſtruum but one; but if there be a diverſity of nature in the liquors, that make up a menſtruum, or in the bodies, that are diſſolved in it, ſome of the corpuſcles may convene either a part with thoſe of the ſame nature, or mingled with thoſe of and differing nature; but yet at the ſame time, and ſo make up cryſtals of a compounded nature; and ſome of them may convene with homogeneous particles, but at differing times; and ſo miſs of ſuch uniformity as might elſe appear in their concretions. Which may be illuſtrated by what I have elſewhere related concerning the cryſtallizations of ſalt-petre, and ſea-ſalt, diſſolved together in ordinary water; where, moſt commonly, grains of ſalt of reſulting figures are produced; and alſo a conſiderable part of the ſea-ſalt coagulates in the form of imperfect cubes, about the bottom, before the nitrous corpuſcles ſhoot into cryſtals of their own almoſt priſmatical ſhape. And I might further add, that it matters not, whether the ſuperfluous water be waſted by exhalation, or by being drained by a body fit to ſoak it up; as we have had occaſion to obſerve in accelerating the cryſtallization of ſome bodies, where I was not willing to employ the heat of the fire, by placing, underneath the ſolution, dried earth, or ſome other porous and ſoaking body.

WITH ſome analogy to ſuch inſtances as theſe, we may conceive, that where there are petreſcent liquors, mingled with common water, there may, by divers accidents, and particularly an hot ſummer, a ſufficient diſcharge be made of the ſuperfluous moiſture, to make the more diſpoſed parts of the petreſcent liquor to coagulate; and afterwards the coagulation may be ſuſpended, either by the ſupervening of a colder ſeaſon, as winter; or even in ſummer itſelf, by a plentiful rain, or the effect of it, a land flood, which might check the progreſs of coalitions, by overmuch diluting the liquor, that might elſe have turned into ſtone. Not to mention, that trial hath aſſured me, that there are bodies, and thoſe of very differing kinds, which will, in tract of time, eſpecially if their coalition be furthered by cold weather, coagulate, after they have long remained in a fluid form, though the water, or other menſtruum, by being incloſed in ſtopped glaſſes, be kept from waſting. And ſince the earth harbours differing kinds of theſe liquors, as I have elſewhere ſhewn, and divers of them may be copiouſly impregnated, ſome of them with one ſort of mineral, and ſome with another; we may conceive, that they may have diſtinct periods for their reſpective coalitions, and yet may ſtick cloſe to one another; in regard that, though in our chemical cryſtallizations the artiſts are wont to take out of the veſſel, what ſhoots the firſt time, before they make a freſh exhalation of the water for a new cryſtallization, and by this means, have the coagulated bodies, that they obtain at one time, more uniformly ſhaped; yet in the hollow receptacles, that the earth affords to petreſcent liquors, the veſſels continuing the ſame from firſt to laſt, the uniformity of the bodies produced by coalitions, made at ſeveral times, muſt be leſs regular; and the manifeſt accretions or aggregates of coaleſcent bodies muſt, in all likelihood, be more frequent. And accordingly having ſuffered the exhaling of ſome liquors to be continued in the ſame veſſel, I had coalitions of very differing bodies at the bottom.

What I was not long ſince ſaying, makes me remember, that, in order to a ſatisfaction, which the event gave me, of the conjectures I had about the ſucceſſive concretions of ſome ſolid fire-ſtones, that were not ſuſpected to be other than intire and uniform maſſes, I cauſed two or three, that I thought likely, and of very different ſizes and ſhapes, and brought from diſtant places, to be warily broken: which trial gave me the pleaſure of obſerving, that the internal texture of the leaſt of theſe minerals, which was almoſt ſpherical, was very differing from that of the more internal part of the ſubſtance of the ſtone; and that in the other, and greateſt mineral, there was a little globulous ſtone, that manifeſtly was not of the ſame piece with the invironing maſs, differing from it not only in texture, but here and there by a diſcernable commiſſure: though in moſt places, their adheſion was ſo ſtrict, that we could not make any ſeparation of the two minerals by the help of this commiſſure. The greateſt part of this double fire-ſtone I keep by me, and ſhall ſay nothing of what I further obſerved in it, having mentioned what I ſaid already but upon the by.

I might add, that in ſome circumſtances, even in cloſe veſſels, and therefore without any manifeſt exhalation of the water, or other menſtruum, and, ſometimes, where the diſſolved body was homogeneous, I have, in proceſs of time, had coagulations, where the laſt formed cryſtals ſeemed plainly to have been generated by way of accretion to the firſt.

Difficulty. Having now done with my obſervation, I ſhall endeavour to clear a grand difficulty, which, I foreſee, may be objected againſt our hypotheſis; namely, that theſe aggenerations, if I may ſo call them, of medicinal, and other ſtones, are ſometimes found in places, where there are no petrifying ſprings, and perhaps, no ſprings, or other waters at all; nay, little or nothing but quarries, or other maſſes of ſtone.

But to this I anſwer, firſt, that if we admit of the relations, that I elſewhere mention out of approved authors, concerning men and beaſts turned into ſtone by a petrifying ſpirit, that ſuddenly invaded them, it will not be abſolutely neceſſary, that there ſhould be any petreſcent ſprings, or other-like water, to produce ſuch minerals, as we are now diſcourſing of.

Secondly, for aught has yet been ſhewn to the contrary, we may ſuppoſe, that rainwater does ſometimes bring along with it ſuch petrifying particles, as may ſerve our turn. In confirmation whereof, I ſhall add, that having, of a learned and judicious perſon, enquired after divers particulars relating to a famous bath, by him viſited in *Hungary*, whoſe water abounds very much with petreſcent particles, over which there is very high building erected, I learned by his anſwers, among other remarkable things, that to the roof, or upper part of this tall ſtructure, there were faſtened many long ſtony concretions, (like thoſe wont to be employed to adorn grotto's) which he affirmed to be, from time to time, generated there; not, as I at firſt ſuſpected, by the daſhing up of any drops of water; (which he averred, could not reach any thing near ſo nigh) but by the copious petrific ſteams, that being there checked in their aſcent, did, according to their natural propenſity, coagulate into ſtone. Whether this relation may warrant me to gueſs, that, in ſome places, ſtones may be generated, without the help either of rain, or ſprings, by the aſcent of petrific particles, in the form of exhalations, from ſome lower parts of the earth; which exhalations, ſuffering the lighter ſteams, that accompanied them to exhale, may operate upon ſome diſpoſed materials, that they find in their way, and turn them into ſtone: whether, I ſay, this narrative may well ſuggeſt this conjecture, I ſhall not now ſtay to examine, though the earthy, and ſometimes ſulphureous ſediments, that have been obſerved at the bottom of rain-waters, ſuffered to ſettle in clean veſſels, may ſeem to favour it; and though alſo I might illuſtrate it by what I obſerved in a bottle of diſtilled liquor, whereof no part would naturally aſcend in a dry form; for having kept this vial well ſtopped in a ſafe and quiet place for a year or two, I obſerved, that the aſcending ſteams had quite pervaded the cork, and had formed, at the top of it, numerous whitiſh ſtiriæ, ſlender, but of a length, that ſurprized me.

Thirdly, there is no neceſſity, that in all ſoils, where petrific waters are to be met with, there ſhould be petrifying ſprings, at leaſt above ground. For I have cauſed to be digged ſtore of figured and tranſparent ſtones in a certain earth, that lay upon the upper part of a rock, and ſeemed to be a very dry ſoil: perhaps you will allow me to tell you, that I have, by pouring a ſolution of ſtony ſtiriæ, made with ſpirit of verdigreaſe, on a convenient quantity of bolus Armenus, and ſuffering the ſoft mixture to remain in a glaſs in the open air, till the ſuperfluous moiſture was exhaled; I have, I ſay, by this means, imitated in a little, what I have been now relating, and found ſmall, but untinged and figured cryſtals diſperſed through the little cavities of the red earth. But it will be more conſiderable to our preſent purpoſe to add, that the faireſt and hardeſt petrifying wood, that I ever had, or tried, was taken up by an ingenious perſon I employed in a plot of ſandy ground, where he could not find any petrifying, or ſo much as any other ſpring. To which I know not whether I ſhould add, that ſuppoſing the ground to have been once moiſtened with a lapideſcent liquor, whether brought thither by ſprings, or any other way; one may, in our hypotheſis, well enough account for this difficult phænomenon, that now and then, not only in the ſurface of the ground, and perhaps upon rocks themſelves, there are found aggregates of figured ſtones, that ſeem to grow upwards, as it were, from a root; which much puzzle men to know how they came there, and may incline them to their opinion, who aſcribe vegetations to ſtones. But to this may be anſwered, that many of the concretions, we are ſpeaking of, may have been formed in wombs, that lay, though not deep, yet under ground, or in ſhallow cavities in the ſurface of it; and that after their formation, the looſer earth, that ſurrounded them, may have been waſhed off by rains, blown off by winds, or otherwiſe removed, leaving behind them theſe ſtones that adhered firmly to a ſolid body. Beſides, if I had time, I think it were poſſible for me to ſhew, that ſtony concretions might be produced by the mechanical action of the air, upon the ſtony particles that ſucceſſively apply themſelves to the matter, that firſt begins to coagulate, when they are ready to be forſaken by the moiſture, that accompanied thoſe particles, and was neceſſary to their due application to the caſual rudiments (which paſs for roots) in imitation whereof, I have, more than once, obtained both from ſaline and ſtony ſolutions, dry tufts of prettily figured, and diaphanous or white, but very ſlender, ſtiriæ, if I may ſo call them, that ſeemed to grow out of the ſolid glaſs, and made men wonder how they came thither, no water, or other liquor, appearing near them.

FOURTHLY, It may very well happen, that the petreſcent liquor may be ſo mingled and diluted with ordinary water, as not to be diſtinguiſhed from it by the generality of men, nor to be capable of diſcloſing it ſelf by its effects, till either by the copious exhalation of the common water, or by ſome peculiar advantages, it has to operate upon bodies, it has opportunity to diſcover it ſelf. On which occaſion I ſhall add, that there is a lake in the North of *Ireland*, wherein I could never hear but that fiſhes lived as well as in other lakes, and yet there are ſome rocks near the bottom of it, to which there faſten themſelves divers maſſes and other pieces of a finely figured ſubſtance, and tranſparent as cryſtal; of which an eminent perſon, the chief owner of the lake, preſented me with ſome, and promiſed me more. Now, if we ſuppoſe, that either by ſprings of petreſcent water, or by rains, or by ſubterraneal ſteams, or otherwiſe, waters, reſting in any hollow place, though upon the top of rocks and mountains, ſhall be ſufficiently impregnated with petrifick particles; and that afterwards, in proceſs of time, the merely aqueous parts ſhall be, by degrees, by the heat of the ſun, the ſoaking of the grounds, the winds, or the continual action of the air, brought to exhale away in the form of vapours, the petrifick particles, which are not ſo volatile, will turn the ſoil beneath them, and on the ſides of them, as far as the ſphere of there activity reaches, into ſtone harder or ſofter, of this or that kind, according to the particular nature of the petreſcent liquors, and the ſtructure and other diſpoſitions of the ſoil they invade: in which ſoil, if there chance to be lodged bodies heterogeneous to it, whether vegetable ſubſtances, as roots, pieces of wood, gums, &c. or the whole bodies of animals, as toads, frogs, ſerpents, fiſhes, &c. or their parts, as ſhells, bones, &c. or minerals of an open texture, as boles, unripe ores; or elſe gems or ſtones of another kind already formed; any of theſe things, or any other, that ſhall chance to be lodged there, muſt be found either petrified, or encloſed in ſtone, when this changed and hardened ſhall ſoil come to be broken up. Nor is it at all neceſſary, that this petrefaction of the extraneous bodies, and of the ſoil or bed, be made at once; for it may well be made ſucceſſively at ſeveral times, according as ſome parts of the petreſcent juice happen to be more copious and penetrant, and conſequently more fit to be ſoaked in further than other. For, as the porouſneſs happens to be greater in one part of the ſoil, than in another; or, as the texture and diſpoſition of particular bodies, lodged in the earth, gives advantage to the petrifick particles to work on ſome of them, ſooner, or in a differing manner, than in others; ſo the induration of the pervaded matters may be very unequally made in point of time, as well as in other circumſtance. So that (to omit many other things explicable by it) we may, from what hath been already delivered, conceive, how it may happen, that medical ſtones of very differing colours, conſiſtencies, and operations (of which I have ſeveral by me, that I had from the ſame mineral maſs,) may be generated, and ſeem entire bodies, though (as in ſome, that I found) the difference is great, that ſo one part of the medical ſtone is dark, heavy, and opacous, and the other much lighter, tranſparent, and quite otherwiſe coloured. And upon the ſame principle may be explained, what I lately mentioned to you about the finding of diamonds encloſed in looſe ſtones and even in rocks; of which we have credible teſtimony: which ſeems not more ſtrange to me than a ſtone, which I have by me, which being a kind of pebble, contains in it a perfectly ſhaped ſerpent, coiled up, but without a head, which appears to have been formed before the ſtone, in regard, that in the upper and lower parts of the ſolid ſtone there are cavities left, which together make up one cavity, juſt of the ſize and ſhape of the contained body; to which, as it was eaſy for the matter of the ſtone, whilſt it was yet a ſoft body, to accommodate it ſelf exactly; ſo it is ſcarce conceivable, how, if the pebble had been firſt formed, the incloſed animal, if it were one, or the matter whereof the ſeeming animal afterwards was formed, ſhould not only get in, but find a cavity ſo curiouſly ſhaped, and ſo fitted to its bulk. And that this variety was produced at ſeveral times, might be further argued from this, that the ſeeming ſerpent is plainly of another and clearer kind of ſtone, than that of the mold, that encompaſſes it; and of the mold itſelf, one part, contiguous to the included body, is whitiſh, and abounds in ſhining grains or flakes; in both which, it differs from the other and far greater part. And now it will be time to haſten to the

Fifth conſideration, which is, that, for aught we know, in thoſe very places, where now there is nothing to be ſeen but looſe ſtones, and perhaps beds of ſtones themſelves, that in thoſe very places, I ſay, there may in times paſt have been petreſcent liquors, whether ſtagnant or running. For, I elſewhere * ſhew, (to another purpoſe) that earth-quakes, inundations of ſeas and rivers, ſinkings of ground, encroachments of the land on the water, fiery eruptions and other ſuch accidents, (ſome related by authentick authors, and others happening in our own times, in places, ſome of which I had the curioſity to ſee) have, among other odd effects, been able to dry or choke up pools and lakes, and to ſtop and quite divert the courſe, not only of ſprings, but of rivers, ſo as to leave no footſteps of them, where they plentifully flowed before. Upon the ſcore of which tranſpoſitions of notable quantities of terreſtrial matter, and other great changes of the ſtructure and diſpoſition of the ſoil in divers places, it may well be ſuſpected, that the ſtony wombs or molds, wherein the above-mentioned bodies were found, were heretofore, at ſome time or other, of a muddy or earthy nature, and were receptacles of petreſcent liquors, which, at ſeveral times, turned the whole maſs of the ſoil into ſtone, before the ſprings, or other waters, containing the petrific liquors or ſpirits, were quite conſumed, or had their courſe altogether diverted. But though I could ſay much more to confirm and apply this, and the preceding conſiderations, yet having ſpent ſo much of my time already, I ſhall not only leave all that unſaid, but, to make ſome amends for having ſtaid ſo long in clearing this difficulty, I ſhall do little more than name the two remaining arguments.

Arg. III. It agrees very well with what we were formerly ſaying (in the firſt argument) about the great ſpecific gravity of ſuch, as the newly-mentioned ſtones, in compariſon of that of white marble, or tranſparent pebbles, that it ſhould be poſſible, out of thoſe minerals, to extract ſome of that ſubſtance, whether metalline, or of kin to it; upon whoſe account, I told you, I ſuppoſed them to be ſo ponderous. And accordingly we have, by appropriated menſtruums, obtained from the forementioned bodies, and not from thoſe only, ſolutions or tinctures, which, beſides that, by their colour or taſte, they diſcover themſelves, did, upon their being dropped upon a ſolution of galls, or ſome other convenient liquor, or upon their being examined by other proper ways, produce ſuch changes of colour, or ſuch determinate phænomena, as argued them to abound with metalline, or mineral particles, which, for the moſt part of them, I obſerved to be of a vitriolate nature; ſo I found, that the ſolution of a blood-ſtone, which taſted very rough upon the tongue, would, with the infuſion of galls, make an inky mixture; and the like would alſo be made with load-ſtone, emery, marchaſites, &c. opened with corroſive menſtruums. But the ſolution of lapis calaminaris, which was of a golden colour, did not operate like the reſt on the infuſion of galls; but yet by its taſte, as well as colour, ſufficiently diſcovered itſelf to have copiouſly impregnated the menſtruum. And now the mention of lapis calaminaris minds me, to take thence an inſtance of what I lately intimated, that there may be other ways, beſides that of diſſolutions in proper menſtruums, to ſhew, that ſome medicinal ſtones participate of metalline and mineral ſubſtances. For it is by melting lapis calaminaris with copper, and keeping them together for a competent while in fuſion, that braſs is made; wherein the red colour of the copper is changed into a golden one, and the abſolute weight (for I ſpeak not of the ſpecific gravity) conſiderably increaſed. Nor is this the only mineral ſtone, from which I have, by a way quite differing from thoſe I have yet mentioned, namely, with running mercury, obtained a metalline ſubſtance. And though native cinabar, uſed by eminent phyſicians both inwardly and outwardly, be looked upon by the vulgar as only a red ſtone; yet it is known, in the quick-ſilver mines of *Friuli*, and ſome other places where it abounds, that it is a mercurial ore, whence, by vehement fires, they diſtil running mercury, which we, by moderate ones, have ſometimes done.

But here perhaps it may not be improper to tell you, that though before any admonition given men of the expediency of examining ſtones hydroſtatically, I could not receive from others, yet I made againſt myſelf the following objection, that there are ſome ſtones, to which uſeful qualities are aſcribed, which are either not at all heavier in ſpecie, than is requiſite for a ſtone, as ſuch, to be; or ſo little heavier, that it is no way likely, that metals, or any ſuch ponderous minerals, ſhould contribute either to their productions, or their virtues.

In anſwer whereunto, I thought it may be ſaid in the firſt place, that our hypotheſis does no way oblige us to deny, that there may be ſuch ſtones. For though it aſcribes the virtues of moſt gems, and metalline ſtones, to the metalline and ponderous mineral ſubſtances they partake of, yet the conceſſion agrees very well with our doctrine; which (as will in the fourth argument be more manifeſted) ſpeaks in general, when it teaches, that the virtues of ſtones may, in many caſes, depend upon their conſiſting not of a pure petreſcent ſubſtance, but a ſubſtance impregnated with other minerals, which, though moſt commonly they prove ſpecifically heavier than the petreſcent matter, as ſuch, without being the leſs, but rather, in ſome caſes, the more operative and communicative of their virtues; yet, in divers ſtony concretions, the adventitious ingredients may be ſpecifically lighter than the genuine matter of the ſtone; as may be eaſily gathered from ſome paſſages of the foregoing diſcourſe. For, not here to urge, that divers bodies, that paſs for ſtones, do abound in particles of ſalt, which may be much leſs heavy than pure ſtone of the like bulk, I have obſerved, that ſome other hard foſſils abound with a kind of bitumen, which, when by diſtillation brought to an oil, is much leſs heavy than a ſtone of the ſame bulk. And, as I remember, I have had ſome portions of ſuch oil, that would ſwim even upon common water; and, leſt this ſhould be aſcribed to the ſubtilization, the bitumen received from the fire, I will add, that having hydroſtatically weighed a piece of good aſphaltum, we found it to be to water of the ſame bulk, but as 1, and ſomewhat leſs than $\frac{4}{10}$ to 1. Which was within a tenth of the proportion to water of a ſtony, though a bituminous foſſil, commonly called in *England* Scots-coal. And becauſe ſulphur, as well as bitumen, is very apt, (and indeed, more apt, than before trial I expected) by even a moderate heat, or attrition to diffuſe its ſteams, (uſually rank-ſcented enough) I ſhall add, that there are variety of hard ſtones, which abound in ſulphur: (witneſs, that in ſome places they obtain their common brimſtone by ſublimation thence) and yet having weighed a roll of brimſtone in air and water, I found it to be but a fraction ſcarce worth mentioning above double its weight to the liquor; which ſhews it to be much lighter in ſpecie than cryſtal itſelf.

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* In an examen of an experiment urged for the magnetiſm of the earth.

AN improvement of this firſt anſwer may furniſh me with the ſecond. For hence we may argue, that it is not impoſſible, that the principal virtue of a light medical ſtone, ſhould be due to ſome mixture of a metalline, or the like ponderous ſubſtance; ſince, if ſome of the ingredients, that are plentifully mixed with the true ſtony matter, be of the lighter ſort, though there be alſo ſome metalline, or other heavy mineral particles mingled with the ſame matter, yet the ſpecific levity of the one, in compariſon of this matter, may compenſate the ſpecific gravity of the other; and they may all compoſe a ſtone, either leſs, or not more ponderous than white marble. On which occaſion, I remember, not only that I found a blackiſh Eaſt-Indian flint, and likewiſe a black Engliſh one, to have to water not full the proportion of $2\frac{6}{10}$ to one, but that one of the firſt pieces of black marble, that I examined hydroſtatically, was found, notwithſtanding the darkneſs of its colour, to be to water of the ſame bulk, ſcarce any thing more than $2\frac{7}{10}$ to 1, which, you may remember, was the proportion I found between white marble and water, unleſs we ſhould ſay, that this blackneſs of colour proceeded, not ſo much from any groſs bituminous matter, imbodied with that of the ſtone, but from ſome mineral ſmoke that had pervaded it. And this puts me in mind of ſpeaking ſomething in this place about what might properly enough have been diſcourſed of long ago.

WHEREFORE I ſhall ſubjoin, in the third place, that it ſeems not impoſſible, that the matter, which medical ſtones are made of, may, before it comes to be hardened, derive various colours, and be imbued with virtues by ſubterraneal exhalations, and other ſteams. This, I fear, you will think ſomewhat ſtrange, and therefore I ſhall briefly endeavour to confirm it by the mention of two or three particulars.

THAT then many places of the lower part of the earth emit copious exhalations into the upper, and even into the air itſelf, I preſume you will grant, and I have elſewhere proved it. That alſo ſuch ſubterraneal ſteams will eaſily mingle with liquors, and imbue them with their own qualities, may be inferred from the experiment of mixing the gas (as the Helmontians call it) or the ſcarce coagulable fumes of kindled and extinguiſhed brimſtone, with wine, which is thereby long preſerved. And I have elſewhere mentioned, how I have incorporated this ſmoke with other liquors, wherein I obſerved its operations to be notable.

THAT beneath the ſurface of the earth there may be ſulphureous, and other ſteams, that may be plentifully mixed with water, and there, in likelihood, with lapideſcent liquors, I have alſo manifeſted in another * diſcourſe.

THAT quick-ſilver may be in part reſolved into fumes by leſs fires than many of thoſe that burn under ground, will be readily acknowledged by chemiſts and gilders, and is obvious in the fumigations employed in the cure of the lues venerea. And that mercury may in the bowels of the earth be ſo diſguiſed, and well mixed with ſtony matter, as to ſuffer the whole concretion to paſs for ſtone, may be obſerved in ſome kind of native cinaber.

THAT ſal armoniac, of which, in ſome places, there is to be dug up ſtore, will, with a moderate fire, be made to aſcend in form of exhalations, is vulgarly known, as to the factitious ſalt of that name; and I have found it to hold in the native. That common ſal armoniac, ſulphur, mercury, and tin, will be ſublimed into a gold-like ſubſtance, that participates of moſt, if not of all the ingredients, may appear by the account I have elſewhere given of the way, I uſed, in making aurum muſicum: and that even gold itſelf, the heavieſt and fixedeſt of the bodies we know, may, by no great proportion of additament, and that with but a moderate fire, be made to aſcend in the form of fumes, or even of flame, I have ſeveral times tried, by ways elſewhere delivered. And that mineral exhalations may be met with in the bowels of the earth, is witneſſed by the relations of divers credible perſons, converſant about minerals, that affirm themſelves to teſtify what they write upon their own obſervation, to which ſome things, that I had ſeen myſelf, did the more incline me to give credit. And this copious aſcenſion of mineral fumes, and even of metalline ones, may be much confirmed, not only by what is written by profeſſed chemiſts, but by the learned and curious *Johannes Kentmannus*, who, in the uſeful catalogue of the *Miſnian* foſſils he had collected, amongſt the pyritæ or fire-ſtones, reckons one, whoſe title is, *Pumicoſus, & ab exhalatione ardenti nigro colore tinctus*; and another, whoſe inſcription is, *Coloris argenti, qui ab exhalatione viroſa colore cinereo eſt tinctus*. The ſame may be further confirmed by what I have ſomewhere met with, as related *in terminis* by the learned *Cabæus*, that he found in the territory of *Modena*.

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* Of ſubterraneous ſteams.

To bring this home to our purpoſe, ſince there are mineral exhalations of very differing kinds, diſperſed in divers places under ground, and ſince there are ſeveral volatile minerals, as arſenic, orpiment, ſandarach, &c. that are very actively hurtful; there may be others endowed with medical qualities, and the exhalations of ſuch minerals, either alone, or mixed with petreſcent liquors, pervading duly-diſpoſed earths, and boluſſes, and other fluid, ſoft, or open ſubſtances, before their induration, may endow them with medicinal and other qualities.

NAY, when I recall to mind the old phænomena, that I have partly obſerved, and partly received from credible teſtimony, about the coalitions, mixtures, tinctures, and the emanations, as it were of thoſe tinctures, in metalline, ſtony, and other foſſile concretions; I dare not peremptorily deny, but that, even after ſubterraneal bodies have obtained a conſiderable degree of induration, and perhaps great enough to make them paſs for ſtony ones, there may be ſubterraneal ſteams ſubtle enough to penetrate, tinge, and otherwiſe impregnate them. Which you would think the leſs impoſſible, if you reflect upon what I juſt now related out of *Kentman*; and eſpecially, if I had time to add here, what I remember I elſewhere delivered about my trials to tinge native cryſtal with differing colours, by the fumes of volatile minerals. And that a very ſmall proportion of a metalline ſubſtance, reſolved into minute particles, may ſuffice to impart a tincture to a greater quantity of other matter duly diſpoſed, may appear, by thoſe factitious gems, wherein, with three or four grains of a ſkilfully calcined metal, or ſome ſuch mineral pigment, we may give the colour of a natural gem to a whole ounce, or more, of vitrified matter. And I remember, that in ſubtiler fluids, I have made the inſtance by vaſt odds more conſpicuous, having tinged with one grain, or leſs, of a prepared metal, as gold or copper, as much ſucceſſively generatied phlegm, as, if it could have been all preſerved, would have amounted to a bulky lump of deeply-coloured matter.

BUT your allowing the heſitancy I have expreſſed in this laſt paragraph is not neceſſary to my preſent purpoſe; wherefore I ſhall not borrow any thing to countenance it from another paper, but paſs on to what remains.

Arg. IV. THE laſt thing, that I ſhall repreſent to ſhew, that the virtues of opacous gems, and medicinal ſtones, may be more eaſily, than thoſe of tranſparent ones, accounted for in our hypotheſis, is this, that the main ingredients, whereof many ſuch opacous ſtones conſiſt, were complete mineral bodies before they became ſtones; ſome of them having been medicinal boluſſes, or the like earths; ſome earths abounding with metalline or mineral juices; ſome, ores of metals, or minerals of kin to metals; and ſome, in fine bodies of other ſorts, or natures, differing from theſe and one another. For all theſe ſeveral kinds of foſſils may, by the ſupervening and pervaſion of petrific ſpirits, be turned into ſtone; and conſequently retain many of the virtues, they were indowed with by the mineral corpuſcles, that had copiouſly, either under the form of liquors, or exhalations, impregnated them, whilſt they were yet earths, or other bodies of a more open or penetrable texture.

I might illuſtrate this by the way I elſewhere mention, whereby I made ſuch mixtures, even of ſtony and metalline ingredients, that, notwithſtanding their coalition, were tranſparent; though you will grant that to be more difficult, than to compound ſuch concretions when one is allowed to make them opacous.

BUT here I muſt obviate an objection, which I foreſee may be made againſt our preſent fourth argument, unto which, even what I have been now ſaying, may afford a riſe. For ſince it ſeems by our doctrine, that gems may be but magiſteries, and conſequently but ſuch compoſitions, as, though made in the bowels of the earth, might be made or imitated by human ſkill, it may ſeem very improbable to many, that bodies ſo near of kin to artificial ones ſhould be endowed with ſuch peculiar, and, ſome of them, with ſuch ſtrange virtues, as are aſcribed to divers gems, and are thought to be capable of flowing only from certain ſubſtantial forms, and thoſe very noble ones too.

To this I might reply, that I admit not any ſuch imaginary beings as the Peripatetic forms, which, I fear, they will never be able to demonſtrate. But to avoid unneceſſary diſputes, I will rather anſwer in ſhort, that ſuch compoſitions, as are called artificial, may, for all that, be endowed with great virtues, and ſuch as are called ſpecific; witneſs the virtues of many chemical preparations, even of thoſe, that are uſed by phyſicians of all ſorts. And leſt you ſhould think, I need to fly to chemiſtry, of which ſome learned men are pleaſed to have a great diſtaſte, I will name a couple of inſtances out of *Galen* himſelf; the one is the aſhes of crayfiſh, to which, notwithſtanding the deſtruction, that has been made of the priſtine body by fire, he gives a greater commendation againſt the as ſtrange, as fatal poiſon infuſed by the biting of a mad dog, than he does either to the fiſh itſelf unburned, or to any medicine of nature's own providing; and I hope, you will grant a virtue of that kind and degree, to be ſpecific enough. My other inſtance ſhall be taken from treacle, which, though allowedly a factitious body, and conſiſting of I know not how many ingredients ſhuffled together, was yet, in the days of *Galen*, to whom a book is attributed about it, and ever ſince has been, the famouſeſt antidote, in theſe parts of the world, and has been celebrated, not only for its alexipharmical virtues, which alone are ſufficient to intitle it to ſpecific ones, but for divers others, which are generally aſcribed to it, ſome indeed upon the ſcore of manifeſt, but others alſo upon that of occult qualities.

THE objection being thus diſpatched, we may return to our medicinal ſtones, about which I ſhall venture to add, that, according to our way of explicating the production of them, a not impoſſible ſolution may be offered of this difficult phænomenon; that ſometimes ſtones, that are thought, without ſcruple, to be of the ſame kind (as hath been particularly obſerved by learned men of the lapis nephriticus) are of ſuch different qualifications, that ſome of them prove very conſiderable remedies in caſes, where others prove almoſt utterly ineffectual. And I have obſerved alſo, though very rarely, that a medical ſtone may have virtues, that are taught to be the properties of ſtones of another kind. For, according to our hypotheſis, when the ſtony matter is impregnated, as it ought to be, with thoſe minerals, that in the ordinary courſe of nature belong to that ſpecies, its virtue will be ſuch, as it ſhould be for kind, but for degree may be very various, anſwerable to the plenty, purity, ſubtlety, &c. of the mineral, that impregnates it. But if the ſtony matter chance to be inbued with ſome other ſubſtance of a contrary nature, though, perhaps, the proportion of it may be ſo ſmall, and the colour of it ſuch, as not to make an alteration in the ſtone obvious to ſenſe, and great enough to make it judged to be of another ſpecies; yet it may ſo vitiate the matter, wherein its expected quality reſides, or check and infringe its operations, as not to leave the ſtone any conſiderable degree of virtue. And on the other ſide, if it happen, that the mineral corpuſcles, that are wont to impart a certain virtue to the ſtony matter of one gem, ſhould, by ſome lucky hit, be ſo united with that of another ſort of gems (of which caſe I formerly gave an inſtance in green diamonds) though the quantity of this unuſual ingredient may be but very ſmall, yet, if its efficacy be great, it may innoble the ſtone with a notable degree of ſome ſuch virtue, as is ſuppoſed not to belong to that ſpecies, but to another.

See *Unzerus* de nephritide.

AND on this occaſion I ſhall add, that I know a gentleman, a profeſſed ſcholar, who to the eye ſeems to be of a complexion extraordinarily ſanguine: this perſon was for a long time ſo troubled with exceſſive bleedings at the noſe, that, notwithſtanding all the remedies he could procure in an academy of phyſick, where he lived, he was divers times brought to death's door; till at length his caſe growing very famous, there was ſent him by an ancient gentlewoman a blood-ſtone, about the bigneſs of a pigeon's egg, with an aſſurance, that it had done ſcarce credible cures in his diſeaſe, by being worn about the patient's neck. Upon the uſe of this ſtone he quickly recovered his health, and had long enjoyed it, when I converſed with him, but yet ſo, that when he left it off any conſiderable time, his diſtemper would return. And when I ſeemed to ſuſpect, that imagination might have an intereſt in the efficacy of this remedy, he anſwered, that he was very well ſatisfied of the negative; and particularly upon this trial, that he had, by the hands of a third perſon, that lived not far off, and whom he named to me, ſtopped a hæmorrhage in a neighbouring gentlewoman, whom the violence of the diſtemper kept from knowing, that any thing had been applied to her, till a pretty while after the blood was ſtanched. I ſhall not here mention other inſtances, though very remarkable, of the efficacy of this ſtone, which I had, both from the gentleman himſelf, and an intimate friend of his, who is a very learned man and a phyſician; becauſe I have ſaid enough to make it ſeaſonable for me to tell you, that notwithſtanding all the odd operations of this ſtone, when I came to look upon it, it was ſo differing in colour and texture from what I expected, that I ſhould have taken it much rather for a gem of ſome other ſpecies, than a blood-ſtone.

TO confirm ſome of the particulars comprized in this our fourth argument, and ſhew the variety, and ſometimes great plenty of mineral and other ſubterraneal matters, that may concur to the compoſition of bodies, that paſs for ſtones; I ſhall obſerve, that the ſubtilty and penetrancy of ſome liquors, if duly conſidered, may evince it to be poſſible, that ſuch bodies ſhould be petrified by them, and with them, as may in part conſiſt of animal and vegetable ſubſtances, as in petrified ſkulls, bones, and pieces of wood: and we ſee, that ſoft ſtone, which is plentifully found near *Naples*, and commonly called the lapis lyncurius, being rubbed a little and moiſtened with water, and then expoſed to the ſun in a due ſeaſon of the year, will, in a very ſhort time, (as eye-witneſſes have aſſured me,) produce muſhrooms fit to be eaten; as if even the ſeminal principles and rudiments of vegetables may be ſo preſerved in a petrified earth, as to be able to diſcloſe themſelves, when they find an opportunity. To which agrees well what an eminent perſon, maſter of ſome of theſe ſtones, informs me, that they now and then find them of a vaſt bigneſs, as if whole maſſes of earth, pregnant with the prolific principles of muſhrooms, were, by ſome supervening, but not very potently hardening petreſcent liquor, turned into ſtone.

AND not only there may be boluſes, ſealed earths, and ſuch like foſſils, that are commonly known to be medicinal, hardened into ſtone,

ſtone by petrifying agents; but alſo other earths, ſubject to be petrified, may have medicinal and ſubtle particles of ſuch a kind in them, as ſcarce any body would expect. But to omit inſtances, belonging to another paper, I have viſited a certain clay-pit in a waſte piece of ground, in which, at a conſiderable depth from the ſurface of the earth there lay a bed of clay, which by diſtillation yielded ſome acquaintances of mine a ſalt ſo volatile and ſtrong, and ſo differing from other ſubterraneal ſalts, that my examens did not diſcover the manifeſt qualities of it without ſome wonder; and the owners of it (perſons curious and rich) did themſelves uſe it as well as give it in phyſick, and cryed it up for an excellent cordial, and a great opening and diaphoretic medicine.

That ſublimable ſalts, ſulphurs, bitumens, (bodies, that communicate enough of their virtues,) may be met with in the bowels of the earth, I have elſewhere ſhewn: and that ſuch ſubſtances may be found in bodies that paſs for ſtones, I have been induced to think by the chemical examen, that I purpoſely made of ſome ſuch concretions, particularly of that ſolid and heavy one, that is commonly called ſcotch-coal, from whence I obtained by diſtillation, (wherein I ſomewhat wondered, other mens curioſity did not, as far as I knew, prevent me,) a good proportion of oil or liquid bitumen, and no ſmall number of ſaline particles, that ſeemed to be of an uncommon nature.

That metalline particles may concur to make up a body, that paſſes for a medicinal ſtone, may appear by native ſulphur, which is it ſelf a compounded body, beſides a good proportion of mineral earth.

I had thoughts not to make an end of this diſcourſe, without mentioning to you ſome attempts, that I partly deſigned, and partly made, to illuſtrate ſome paſſages of it by purpoſely contrived experiments, whereof ſome were unproſperouſly, and others not altogether unſucceſsfully tried. But not having the minutes of them by me, and not daring to truſt my ſingle memory in experiments ſo nice, and ſo long ſince made, as thoſe were, I ſhall here put an end to your trouble; eſpecially ſince at length I perceive, that the forgetfulneſs of my firſt intended brevity has miſled me ſo far beyond the bounds of it into excurſions, whereinto the unforeſeen connexion of things unawares engaged me, that I ſtand in need both of your pardon and my own; of yours, for having exerciſed your patience with a prolix diſcourſe; and of my own, for having receded from my cuſtom, by contributing to that prolixity, and by expatiating upon conjectures; to which the more I conform to my own practice, the leſs I am indulgent: though theſe may be the more pardonable, becauſe I have propoſed them but as gueſſes, not peremptory aſſertions, much leſs phyſical demonſtrations. And if *Ariſtotle* himſelf, where he gives an account of phænomena appearing above the ſurface of the earth, ſcrupled not to think he had done enough, if he had ſhewn, how ſuch things may be produced; I hope it may be tolerable in me, who treat of things, that nature does privately in her dark and ſubterraneal receſſes, to have offered accounts, that are poſſible, if not probable. And yet I ſhould have ſpent much leſs of my diſcourſe upon conjectures, if I had not ſeen, that they gave me riſes to bring in more of natural hiſtory, than I could elſe decently do. But after all this I confeſs to you, (though you may think it a paradox) that one of the main cauſes of the prolixity of theſe papers was my haſte, and that experience hath taught me, on this occaſion (as well as on ſome others) that there may be more truth, than there is likelihood, in the genteel conceipt of a French ſecretary, that ſaid, he had written his friend a long letter, becauſe he had not leiſure to write him a ſhort one.

TRACTS.

CONTAINING,

NEW EXPERIMENTS, touching the Relation betwixt FLAME and AIR. And about EXPLOSIONS.

An HYDROSTATICAL DISCOURSE, occaſioned by ſome Objections of Dr. HENRY MORE againſt ſome Explications of NEW EXPERIMENTS made by the Author of theſe TRACTS:

TO WHICH IS ANNEXED

An HYDROSTATICAL LETTER, dilucidating an Experiment about a way of weighing Water in Water.

NEW EXPERIMENTS,

Of the POSITIVE or RELATIVE LEVITY of Bodies under Water,

Of the AIR'S SPRING on Bodies under Water,

About the DIFFERING PRESSURE of heavy SOLIDS and FLUIDS.

The PUBLISHER *to the* READER.

IT will, it is preſumed, be altogether needleſs to preface any thing by way of commendation to the following Tracts; they will certainly commend themſelves by their own worth to the intelligent and attentive reader, who might have ſeen them ſooner, if the preſs had not detained them longer than was expected; ſince, to the publiſher's knowledge, they were ready in the year 1671, except the hydroſtatical diſcourſe, and the explication of the author's experiment of weighing water in water, the former of which was finiſhed in the beginning of this year 1672; though the latter could not be ſo till near the end of the ſame year, viz. the month of February, Engliſh ſtile, becauſe the book of Mr. *George Sinclair*'s Hydroſtaticks, in which it is excepted againſt, came not, I think, before that time to *London*; I am ſure, not to the view of the honourable Author. Farewel.

NEW EXPERIMENTS

Touching the RELATION betwixt

FLAME AND AIR,

Sent in a LETTER

To the Learned Publiſher of the PHILOSOPHICAL TRANSACTIONS.

SIR,

YOU may have obſerved, as well as I, that ſince the publiſhing of the experiments I ſent you touching reſpiration, divers of our learned men have ſpent both thoughts and diſcourſes in inquiring and diſputing, whether there reſides in the heart of animals ſuch a fine and kindled, but mild ſubſtance, as they call a vital flame, to whoſe preſervation, as to that of other flames, the air, (eſpecially as it is taken in, and expelled again by reſpiration) is neceſſary. This among other conſiderations, makes me think it ſeaſonable (though many avocations make it inconvenient) to complete the performance of the promiſe I made you, by adding to the experiments about reſpiration, which your commands have already obtained of me, thoſe ſcattered notes, that I have been able to pick up about the relation betwixt flame and air. And though, I confeſs, they are very much inferior in number to the trials about reſpiration: and, that in making them it was not ſo much my deſign to complete an entire and diſtinct tract, though but a ſmall one, of ſuch experiments, as to gratify my own curioſity in the examining of a paradox or two, I had been writing about flame; yet the nobleneſs of the queſtion now under debate, and their pertinency to it, will poſſibly keep them, as few as they are, from being uſeleſs. And that alſo they may be the better kept from being unwelcome, I have choſen to make my ſelf a relator of matters of fact, without engaging with either of the litigant parties in a controverſy, wherein I am the leſs tempted to be partial, becauſe I have not formerly declared my opinion about it, and at preſent, I ſee, on either ſide, perſons, for whom I have no ſmall reſpect and kindneſs.

AND now, Sir, that you may not expect in the following papers ſuch a number and variety of experiments, as I might perhaps be able to preſent you with, on ſome more tractable ſubject; I ſhall briefly mention to you ſome of the chief difficulties I met with in the making of theſe; which I do the rather, that, if you, and your ingenious friends have a mind to proſecute ſuch trials, you may not be ſurpriſed with the difficulties I have met with; but provide at leaſt againſt thoſe foreſeen ones, by which you will ſcarce fail to be encountered.

I ſhall then inform you, that the enſuing experiments were rendered uneaſy and troubleſome to me by this; that ſome of them could not be conveniently done at all ſeaſons of the year, nor in any ſeaſon in all weathers, but muſt be made not only in the day time, but in ſunſhine days. You will eaſily gueſs, that I ſpeak of thoſe experiments, that are to be made by the help of a burning-glaſs, caſting the reflected or refracted beams of the ſun upon the combuſtible matter placed in the exhauſted receiver: for, by reaſon of the interpoſition of ſo thick a glaſs, whereby many of the incident beams of light are reflected, and others inconveniently refracted, there is ordinarily requiſite a clear day, and a competent height of the ſun above the horizon, and ſometimes alſo a convenient time of the year, to bring ſuch experiments, as we were ſpeaking of, to a fair trial. Not to take notice, that in ſuch attempts there uſually intervene circumſtantial difficulties, not ſo eaſy to be foreſeen; and it not being ſummer, when I had occaſion to make the following experiments, I could make but very few with the ſun-beams; beſides that there are divers others, which are not that way to be made ſo conveniently, if at all, as by the help of the fire.

BUT though the trials of this ſecond ſort had their conveniences, in regard they might be made in any weather, and as well by night as day; yet they were not unattended with peculiar inconveniencies; ſome of which you will eaſily diſcern by the mention of them, that was neceſſary to be made in ſome of the relations themſelves. And, beſides more particular and emergent difficulties, there was this in general, that rendered theſe experiments troubleſome; that, whether I made them in larger receivers, or in ſmall, or in middle-ſized ones, each of theſe caſes had its inconveniencies: for very large receivers, beſides that it was very toilſome and tedious to empty them of air, required ſo much time for the exhauſtion, that too frequently, by that time the operator had done pumping, the included, or other heated body was grown too cold to perform the deſired effect: and if the receiver were not conſiderably large, than the red-hot iron, or other included body, that was to burn the combuſtible matter, would much endanger the breaking of the over-heated glaſs, and not afford room enough for ſome phænomena to be fairly exhibited in; and, beſides, create another difficulty, to which we found middle-ſized receivers alſo obnoxious: for, ſeveral times, when the experiment required an intenſe heat within the receiver, then (eſpecially if ſome caſual obſtacle hindered the quick exhauſtion) the heat of the ignited iron, or ſome ſuch other included body, would ſo melt or ſoften the cement, that faſtened the receiver to the engine, that, when the glaſs was brought to be well exhauſted, and ſometimes alſo before, the external air would, by its preſſure and fluidity, ſqueeze or thruſt in ſomewhere or other the yielding cement, and thereby cauſe in the inſtrument a leak, that would much incommodate us, if not reduce us to begin the experiment again, inſomuch, that, for ſome trials, we were fain to provide a cement on purpoſe; the leaſt fuſible, that we uſed on other occaſions, being yet found too fuſible on theſe.

Nor were thoſe, I have already mentioned, the only difficulties and impediments I met with in making experiments about flame and air; but I ſhall not here trouble you with them in this place, where it may ſuffice for me to have mentioned thoſe, that are of a more general nature, and are like the moſt frequently to occur.

But though I declined to name any other to you, than the foregoing difficulties in making the following experiments; yet I muſt not omit to take notice of one, that may occur to you about judging of them. For, in thoſe trials, that require to have an ignited iron or any ſuch thing included in the receiver, it would uſually happen, that ſo much heat would rarefy the air ſhut up in the mercurial gage, and conſequently inable it to depreſs the mercury, that lies under it, far beneath the mark it would have ſtaid at, upon the meer account of ſo much ambient air pumped out: this would happen, I ſay, before the heated receiver was well exhauſted; ſo that, if one be not aware of this, it will be obvious, by looking on the gage, to conclude the receiver to be well emptied, before it really is ſo. And therefore the ſafeſt way in theſe caſes is, to continue to pump (without truſting to the ordinary marks) till you ſee, that the mercury will be no further depreſſed in the ſealed leg of the gage; though otherwiſe, by concurring ſigns, one that is verſed in thoſe trials, may well enough judge, when he needs to pump no longer.

But perhaps you will here demand, whether, by our engine, we can competently withdraw the air out of a receiver; or whether, at leaſt, that may not be much better done by the help of quick-ſilver, after the manner of the Torricellian experiment, in regard that ponderous liquor frees the glaſs, it deſerts, from all the air at once, and exactly hinders the regreſs of it.

In anſwer whereunto, I hope you do not expect, that I ſhould contend for a favourabler judgment of the engine I employ, than the virtuoſi (as well foreign as Engliſh) have been pleaſed to paſs on it already: and therefore, to tell you freely my thoughts about the main part of the propoſed queſtion, I ſhall readily avow to you, that I think, there may be experiments (ſuch as ſome of thoſe, where the included body need be but ſmall, and where the being ſuddenly produced is chiefly deſired in the effect) wherein, by the help of the quick-ſilver, the exhauſtion of the air may be diſpatched with greater celerity, and conſequently make the effect be more conſpicuous, than, by our ordinary way of trying, it would be in our engine; ſince the fall of the mercury does, as the objection intimates, produce a vacuum (in our ſenſe of that word) very nimbly, whereby the expanſion of the air is preſently effected, and the aereal particles, harboured in the pores of any body placed in this deſerted cavity, will thereby have opportunity more ſuddenly to expand themſelves. But, on the other ſide, I might anſwer in general, that when I have particular occaſions to diſpatch the exhauſtion of the air, I can very much haſten it, by barely leſſening, as I have ſeveral times done, the capacity of the receiver; inſomuch, that I have ſometimes employed ſo ſmall an one, that in half a minute, or much leſs, after it was fitted on, we could conſiderably exhauſt it, and thereby produce phænomena exceeding conſpicuous. And as to the experiments of this little tract in particular, it may be ſaid, that not to mention the troubleſomeneſs, and other inconveniencies of needing to employ ſuch an unwieldy weight of mercury, you will eaſily find, by the phænomena of divers of the enſuing trials, that moſt of them cannot be with any conveniency, and ſome of them not at all, made in the Torricellian tubes. As for the ground of the objection, that the air cannot be ſo well drawn out by our way, as by the ſubſiding of the mercury; though you may think that very clear, yet one, that were very jealous of the reputation of the inſtrument I employ, may perhaps reaſonably enough queſtion it. For the vacuum, that is produced in the Torricellian experiment, as it is made all at once, ſo it is made once for all; and therefore, if there were any aereal particles lurking in the mercury, as there will be pretty ſtore, if the quantity of that liquor be great enough to make a conſiderable vacuum, which if it be not, it will be too ſmall for very many of our trials; they will remain in the deſerted cavity at the top of the glaſs, and, by their expanſion there, much hinder the full operation of an ambient vacuum upon the bodies placed in it. Beſides that almoſt all ſuch bodies, if they be dry, will be ſo incongruous to mercury (which ſcarce ſticks to any conſiſtent bodies but metals,) that probably there will be no ſmall number of aereal corpuſcles intercepted between the mercury and thoſe ſurfaces, to which it does not cloſely adhere: which aery corpuſcles, when the ſubſiding mercury deſerts them, will be left to encreaſe the number of thoſe, that, as we were ſaying, will emerge from the mercury; from which, as alſo from the pores of the included bodies, will perhaps ariſe divers new ones, from time to time, for a pretty while after. And in caſe the vacuum be made by a cylinder of two or three and thirty foot of water, as for ſome experiments, that have been tried in *France* and *Italy*, hath been done, the emerſion of bubbles may laſt a long time, as may be gathered from ſome obſervations of mine, elſewhere related.

ON the contrary, in our engine, though when the receivers are not very ſmall, they are more ſlowly emptied; yet in recompence, we may continue the pumping out of the air as long, and renew it in the ſame experiment as often as we think fit: ſo that, if we perceive, that, after the firſt exhauſtion of the glaſs, there happen any aereal particles to extricate themſelves ſucceſſively out of the included body, we can, by reſuming the pump from time to time, whenever need requires, free the vacuum from theſe alſo; which, in ſome caſes, I have found to be longer and more copiouſly emitted by the included bodies, than any thing but jealous trials could have convinced me of. And to confirm what I have been ſaying by ſomething hiſtorical, I ſhall add; that though the excellent Florentine academians are thought to have proſecuted the experiments about the vacuum made with mercury the furtheſt of any, yet ſome eminent members of that illuſtrious ſociety were pleaſed to confeſs to me, that they never were able, by the help of mercury, to bring a glaſs-bubble, ſealed up with air in it, to burſt of itſelf by the withdrawing of the external air; which yet I have often done with the engine I employ, and convinced them, that I could do ſo, by doing it in their preſence.

YOU will, perhaps, think it ſomewhat ſtrange, to find, that I ſet down ſome of the following narratives in ſuch a way, as does not expreſs me ſollicitous to aſcribe and vindicate to the air ſo abſolute and equal a neceſſity to the production and conſervation of all flames, as divers learned men have concluded from my former experiments. But I, that am content to be kind to the air, but not partial, ſhall not ſcruple to declare to you, that, as much as ſome may think me beholden to the air for any diſcoveries of itſelf, it may have vouchſafed me; yet, I think, a natural, as well as a civil hiſtorian, does, in his accounts of matters of fact, owe more to truth, than to gratitude itſelf. And though, whereever the air can challenge a clear, or, at leaſt, a probable intereſt in a phænomenon, I am not only diſpoſed, but glad to do it right; yet I would not eaſily aſſert to it a larger juriſdiction than I find nature to have aſſigned it; eſpecially ſince, without partiality, that, I preſume, may be ſhewn to be very large and conſiderable, and perhaps to reach to many things, wherewith men ſeem not to have yet taken notice, that it hath any thing to do at all.

WHAT hath been hitherto ſaid, will not, I hope, ſeem impertinent or uſeleſs, whenever you ſhall fall upon the actual making of ſuch experiments as you are about to read. But I fear, that to add any thing more, (which were not difficult for me to do to the preliminary part of this ſmall tract) would make it too diſproportionate to the hiſtorical; from which I ſhall therefore no longer detain you.

The FIRST TITLE.

Of the difficulty of producing FLAME without AIR.

EXPERIMENT I.

A way of kindling brimſtone in vacuo Boyliano, *unſucceſsfully tried.*

WE took a ſmall earthen melting pot, of an almoſt cylindrical figure, and well glazed (when it was firſt baked) by the heat; and into this we put a ſmall cylinder of iron, of about an inch in thickneſs, and half as much more in diameter, made red hot in the fire; and having haſtily pumped out the air, to prevent the breaking of the glaſs; when this veſſel ſeemed to be well emptied, we let down, by a turning key, a piece of paper, wherein was put a convenient quantity of flower of brimſtone, under which the iron had been carefully placed; ſo that being let down, it might fall upon the heated metal; which as ſoon as it came to do, that vehement heat did, as we expected, preſently deſtroy the contiguous paper; whence the included ſulphur fell immediately upon the iron, whoſe upper part was a little concave, that it might contain the flowers when melted. But all the heat of the iron, though it made the paper and ſulphur ſmoke, would not actually kindle either of them, that we could perceive.

EXPERIMENT II.

An ineffectual attempt to kindle ſulphur in our vacuum another way.

ANOTHER way I thought of to examine the inflammability of ſulphur without air; which, though it may prove ſomewhat hazardous to put it in practice, I reſolved to try, and did ſo after the following manner:

Into a glaſs-bubble of a convenient ſize, and furniſhed with a neck fit for our purpoſe, we put a little flower of brimſtone (as likely to be more pure and inflammable than common ſulphur;) and having exhauſted the glaſs, and ſecured it againſt the return of the air, we laid it upon burning coals, where it did not take fire, but riſe all to the oppoſite part of the glaſs, in the form of a fine powder; and that part being turned downward and laid on coals, the brimſtone, without kindling, roſe again in the form of an expanded ſubſtance, which (being removed from the fire) was, for the moſt part, tranſparent, not unlike a yellow varniſh.

ADVERTISEMENT.

THOUGH theſe unſucceſsful attempts to kindle ſulphur in our exhauſted receivers were made more diſcouraging by ſome more, that were made another way; yet judging that laſt way to be rational enough, we perſiſted ſomewhat obſtinately in our endeavours, and conjecturing, that there might be ſome unperceived difference between minerals, that do all of them paſs, and are ſold for common ſulphur, I made trial, according to the way hereafter to be mentioned, with another parcel of brimſtone, which differed not ſo much from the former, as to make it worth while to ſet down a deſcription of it, that probably would not be uſeful.

But in this place, it may ſuffice to have given a general intimation of the poſſibility of the thing. The proof of it you will meet with under the third Title, when I come to tell you what uſe I endeavoured to make of our ſulphurous flames.

EXPERIMENT III.

Shewing the efficacy of air in the production of flame, without any actually flaming or burning body.

HAVING hitherto examined by the preſence of the air, what intereſt it has in kindling of flame; it will not be impertinent to add an experiment or two, that we tried to ſhew the ſame intereſt of the air, by the effects of its admiſſion into our vacuum. For I thought it might reaſonably be ſuppoſed, that if ſuch diſpoſitions were introduced into a body, as that there ſhould not appear any thing wanting to turn it into flame but the preſence of the air, an actual accenſion of that body might be produced by the admitted air, without the intervention of any actual flame, or fire, or even heated ſubſtance; the warrantableneſs of which ſuppoſition may be judged by the two following experiments.

When we had made the experiment, ere long to be related in its due place, (viz. Title II. Experiment the 2d) to examine the preſumption we had, that even when the iron was not hot enough to keep the melted brimſtone in ſuch a heat, as was requiſite to make it burn without air, or with very little, it would yet be hot enough to kindle the ſulphur, if the air had acceſs to it: to examine this, I ſay, we made two or three ſeveral trials, and found by them, that if ſome little while after the flame was extinguiſhed, the receiver were removed, the ſulphur would preſently take fire again, and flame as vigorouſly as before. But I thought it might without abſurdity be doubted, whether or no the agency of the air in the production of the flame might not be ſomewhat leſs, than theſe trials would perſuade; becauſe, that by taking off the receiver, the ſulphur was not only expoſed to freſh air, but alſo advantaged with a free ſcope for the avolation of thoſe fumes, which in a cloſe veſſel might be preſumed to have been unfriendly to the flame.

How far this doubt may, and how far it ſhould, be admitted, we may be aſſiſted to diſcern by the ſubjoined experiment, though made in great part for another purpoſe; which you will perceive by the beginning of the memorial I made of it, that runs thus;

EXPERIMENT IV.

A differing experiment to the ſame purpoſe with the former.

HAVING a mind to try, at how great a degree of rarefaction of the air, it was poſſible to make ſulphur flame by the aſſiſtance of an adventitious heat, we cauſed ſuch an experiment as the above-mentioned to be reiterated, and the pumping to be continued for ſome time after the flame of the melted flowers of brimſtone appeared to be quite extinguiſhed, and the receiver was judged by thoſe, that managed the pump (and that upon probable ſigns) to be very well exhauſted. Then, without ſtirring the receiver, we let in at the ſtop-cock very warily a little air, upon which, we could perceive, though not a conſtant flame, yet divers little flaſhes, as it were, which diſcloſed themſelves by their blue colour to be ſulphurous flames; and yet the air, that had ſufficed to re-kindle the ſulphur, was ſo little, that two exſuctions more drew it out again, and quite deprived us of the mentioned flaſhes. And when a little air was cautiouſly let in again at the ſtop-cock, the like flaſhes began again to appear, which, upon two exſuctions more, did again quite vaniſh, though, upon the letting in a little freſh air the third time, they did once more re-appear.

Whether, and how far ſuch experiments as theſe may conduce to explicate what is related of fires, ſuddenly appearing in long undiſcloſed vaults or caves to thoſe, that firſt broke into them, I may perchance elſewhere conſider, but ſhall not here enquire, eſpecially being not yet fully ſatisfied of the truth of the matter of fact.

EXPERIMENT V.

About an endeavour to fire gun-powder in vacuo *with the ſun-beams.*

WHATEVER hath been hitherto delivered, will not, I preſume, make it unreaſonable to enquire, whether, what intereſt ſoever the air appears to have in the production,

tion of thoſe flames, that are to laſt for ſome time, there may not eaſily be produced a momentary flame or flaſh, without any aſſiſtance from the air. Wherefore I employed ſome endeavours to diſcover, whether there were the ſame need of air to the going off of gun-powder, as to the inflammation of other bodies. And though my firſt attempt of this nature being unproſperous, it was concluded by the learned of the by-ſtanders, that I ſhould never be able to make a ſucceſsful one to kindle gun-powder in an exhauſted receiver; yet this did not hinder me from proſecuting a deſign, for whoſe feaſibility I conſidered, that it might be alledged *a priori* (as they uſe to ſpeak) that brimſtone, which is one of the ingredients of gun-powder, appears by ſeveral trials, to be ſometimes capable of accenſion in our vacuum, and therefore probably may kindle the reſt. But how far the firing of powder, without the help of air, is poſſible, will be beſt judged by the experiments you will meet with under the third title: and how far it is more difficult to be kindled in our exhauſted receivers, than in the open air (which is an inquiry proper for this place) may be gueſſed by the ſubjoined trial; which, though it were made many years ſince (in the year 1660) before we had deviſed the mercurial gage, to examine how well the receiver was exhauſted, I ſhall yet afford it a room in this place, becauſe it was made in ſummer by the help of a burning-glaſs, which I could not employ to purpoſe in the winter-ſeaſon, wherein the two following trials were made.

To give you then ſome account of that part of the experiment, which concerns our preſent inquiry, I will ſubjoin a tranſcript of what I find regiſtered about it; which is to this purpoſe, and almoſt in theſe words: that, having conveniently placed three or four grains of gun-powder in the cavity of our receiver, and having carefully drawn out the air, we caſt the ſun-beams, united by a good burning-glaſs, upon the powder, and kept them there a pretty while to little purpoſe; till, at length, the powder, inſtead of taking fire, ſmoaking only, and melting like a metal, thoſe ſpectators, that were of another opinion, than I was yet convinced of, would have me leave off. The further event of ſuch trials more fully proſecuted you will find under the third title; all that will be pertinent to be here added being, that the newly recited experiment was not the ſingle one, we made about that time, that diſcovered a great indiſpoſition even in gunpowder to be fired in our vacuum.

EXPERIMENT VI.

An attempt to fire gun-powder in vacuo, *by means of a hot iron.*

WE took, by weight, what we judged a convenient quantity of gun-powder, that was extraordinarily ſtrong and well made; and having in our receiver, that was capable of holding about ſixteen pounds of water, placed the formerly mentioned iron firſt heated red-hot, when the air appeared by the mercurial gage to have been diligently pumped out, we let down, by help of the turning key, a ſmall piece of thin paper, wherein the powder had been put, till we ſaw it reached the plate, by whoſe heat we hoped the paper would be deſtroyed, and the powder made to go off. But though both the one and the other had been purpoſely well dried near the fire, before they were put into the receiver; the deſired exploſion of the powder did not enſue. Yet there appeared upon the iron plate a pretty broad blue flame, like that of brimſtone (whence it was judged the ſulphureous ingredient of the gun-powder, that was kindled) which laſted ſo very long, as we could not but wonder at it. But, at length, the powder not going off and the ſtill decaying heat of the iron forbidding us to wait any longer, we thought fit to take off the receiver, and found (as we expected) that the paper contiguous to the iron was, in part, deſtroyed by its heat; but moſt of the grains of the powder ſeemed not altered, and were found diſpoſed enough to be fired, notwithſtanding the conſumption of the brimſtone, that had burned away.

APPENDIX.

TO confirm the foregoing experiment, by ſhewing how great a diſpoſition to take fire there may be in gun-powder, that yet would not do ſo without air, I ſhall ſubjoin this obſervation,

HAVING reiterated the newly mentioned experiment after the like manner, and with the ſame receiver, and iron-plate, as formerly, we did not find any exploſion to be made for ſo long a time, that thinking it in vain to wait any farther, we let in the air, which might perhaps, by the help of the remaining heat of the iron, procure the operation we at firſt deſired. The event was, that after nothing had enſued for a good while, and we ſcarce thought, that ſuch a thing would happen; the powder ſuddenly went off with a great flaſh, and ſo ſhook the receiver, that was yet ſtanding on the engine, as to endanger the throwing of it down. Which circumſtance I mention, to give you a caution, that may prove uſeful, in caſe you try in cloſe veſſels experiments with gun-powder; ſince if they be not warily managed, they may ſometimes (as I have had occaſion to obſerve) prove dangerous enough; which will be the better diſcerned, if I add, that the powder, that had this operation on a receiver (large enough to contain two gallons of liquor) was weighed before it was put in, and amounted but to one grain, (though a greater quantity might perhaps have been well enough ventured upon, if it had been but common gun-powder.)

EXPERIMENT VII.

Reciting another way, whereby the firing of gun-powder in vacuo Boyliano *was attempted.*

TO diverſify our ways of examining the indiſpoſedneſs of gun-powder to be fired in our vacuum, we thought fit to add to the foregoing trials that, which followeth.

INTO a pretty large and ſtrong glaſs-bubble we put a few ſmall corns of gunpowder, and having carefully exhauſted it, and ſecured it againſt the return of the air, we put it upon a pretty quantity of live coals ſuperficially covered with aſhes; by whoſe heat the ſulphureous ingredient of the powder was in part kindled, and burned blue for a pretty while, and with a flame conſiderably great (in proportion to the powder;) upon whoſe ceaſing, the powder, which, when all was done, did not take fire, appeared to have ſent up, beſides the flame, a pretty deal of ſulphureous ſublimate, that ſtuck to the upper part of the glaſs, and being held againſt a candle we cauſed to be brought in, (for the experiment had been purpoſely made in a dark place) it exhibited divers vivid colours like thoſe of the rain-bow.

EXPERIMENT VIII.

About a trial made to fire gunpowder in our vacuum by the help of ſparks.

THOUGH in the fourteenth of the long ſince publiſhed phyſico-mechanical experiments there is recited a trial made about kindling of gunpowder with a piſtol; yet I ſhall not forbear to ſubjoin the enſuing account, partly, becauſe the receiver, we then employed, being about four times, if I miſ-remember not, as big as that we laſt made uſe of, it was very difficult to exhauſt the one ſo well as the other; and partly, becauſe we wanted ſome accommodations, with which we ſince furniſhed ourſelves, and (having not then deviſed the mercurial gage we employed in the making this laſt experiment) we could not then judge ſo well, as we ſince could, of the degrees, to which the receiver was emptied. And, therefore, when in the relation of that fourteenth trial, there is mention made of one attempt, that did ſucceed, among divers, that did not; there is towards the cloſe an intimation given, that in ſpite of the great rarefaction, that had been made in the air, there might yet be ſome little portion of it remaining in the receiver. I proceed then to the promiſed relation, which I find thus ſet down:

To proſecute the deſign of the foregoing experiment by a way ſomewhat differing from thoſe hitherto mentioned, we made, though not without difficulty, the enſuing trial; one of whoſe ſcopes you will find intimated at the cloſe of the relation.

WE took a ſmall and very ſhort piſtol, and having well faſtened it with ſtrings to a great weight, that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then over both the weight and piſtol we whelmed a receiver, capable of containing two gallons of liquor, and having carefully cemented it on, we cauſed the air to be diligently pumped out; having before put in a mercurial gage, to help us to diſcern when it was exhauſted. Laſtly, ordering the pump to be plied in the mean while, for fear ſome air ſhould ſteal in, before the trial was compleated, we did, by the motion of the turning key, ſhorten a ſtring, that was tied both to it and the trigger of the piſtol, by which means we did as much as we could towards the firing of the powder in the pan; but though the pan were made to fly open, yet the powder did not go off: whereupon letting in the air, and cocking the piſtol again, without taking it off the weight it was tied to before, we drew out a little air, to be ſure, that the receiver was cloſely cemented on, (which care we took in reference to another experiment;) and then letting in the air at the top of the receiver, and ſtopping it in with the turning key, we did, by the help of that key, draw aſide the trigger again; whereupon, though there had been no new powder put into the pan, nor any left in it, but only ſome little, that remained after the late trial, yet that little readily took fire and flaſhed in the pan; which made it the more probable, that, in the former trial, ſparks of fire had been ſtruck out by the colliſion of the flint and ſteel: which was the more credible, becauſe in another trial, made the ſame hour in the ſame exhauſted receiver, two of the aſſiſtants plainly ſaw a ſpark or two fly out upon the falling of the cock, though I, that chanced to ſtand in an inconvenient place, did not then perceive it. But afterwards, having cauſed the experiment for my fuller ſatisfaction to be repeated, I freed my ſelf from need of truſting others eyes: ſo that it appears, that notwithſtanding the great indiſpoſition of gunpowder itſelf to be reduced into flame in our vacuum, yet even ſolid matter is not uncapable of being ignited there, if it be put into a motion ſufficiently vehement.

IF this experiment had not been ſo very troubleſome to make, I ſhould have been invited to reiterate it, becauſe a not contemptible ſcruple may be prevented, if the trial can be made to ſucceed, in regard, that the going off of the whole gunpowder, by the falling of a ſpark or two only upon two or three of its grains, would argue, that the accenſion of the reſt was made by the propagation of flame from the kindled grains to the reſt; ſo ſmall a portion of ignited and ſuddenly vaniſhing matter, as is to be found in a ſpark or two, being not likely to be able in ſo very ſhort a time to impart a vehement, or ſo much as a ſenſible heat, to the whole aggregate of grains, or at leaſt a great part of them, as the focus of a burning-glaſs, held long enough upon them to make them melt, may well be ſuppoſed to do.

EXPERIMENT IX.

Two ways of making aurum fulminans go off in our exhauſted receiver.

BECAUSE it is wont to be ſuppoſed, how juſtly I here diſpute not, that aurum fulminans, as the chemiſts call it, is much of the nature of gunpowder, though by vaſt odds ſtronger than it; I thought it not unfit to make trial, whether it could be made to go off in our exhauſted receiver; and accordingly, about the time, that the other experiment of firing gunpowder by the ſun-beams was made, we alſo made trial of this; and that, as I remember, in the ſame receiver, and with the ſame burning-glaſs. The event was, that, though the air had been pumped out, the concentrated beams of the ſun made the aurum fulminans go off, and violently ſcatter about the cavity of the receiver a yellowiſh duſt or powder, which other trials in the free air made us look upon as particles of the gold, that was the main ingredient of this odd compoſition.

This experiment we reiterated a good while after in another place, and with other veſſels, and yet with the like ſuceeſs.

But in regard theſe trials being made by the united ſun-beams, it was unavoidable, that our eyes would be before-hand affected with the vivid impreſſions of ſo glaring a light; it ſeemed not ſafe to determine, by the bare going off, or ſhattering of the aurum fulminans, whether or no it afforded any flame or light upon its exploſion: for, as we could not be ſure of the affirmative, becauſe our eyes could not diſcern any momentany flame or flaſh; ſo it ſeemed not ſafe to conclude the negative; ſince, though there had been ſuch a flame, yet, if it had not been ſtrong, it would not have been ſenſible to our eyes, whilſt pre-affected by a powerful light. Wherefore we reſolved to make this trial in the night with an iron heated, but not candent, (that its light might not eclipſe that, which the powder might afford;) and having, after the manner already often recited, exhauſted a pretty large receiver, and let down by a ſtring half a quarter of a grain (by weight) of good aurum fulminans, of our own preparing, looſely tied in a little piece of thin paper, (which paper, former trials to another purpoſe kept us from fearing, that no hotter an iron, than ours then was, would kindle) we found, as we expected, that after the powder had lain long enough upon the iron to be thoroughly heated, it went off all together, and, as the by-ſtanders affirmed, with a flaſh: but my face being accidentally turned to remove a light, that I feared might diſturb us, I could not ſee the flaſh myſelf, and therefore cauſed the experiment to be made once more, to ground my narrative upon my own obſervation; which quickly aſſured me, that the luminous flaſh, produced upon the exploſion, was not only ſenſible, but conſiderable.

The SECOND TITLE.

Of the difficulty of preſerving FLAME without AIR.

SINCE it is generally, and in moſt caſes juſtly, eſteemed to be more eaſy to preſerve flame in a body, that is already actually kindled, than to produce it there at firſt; we thought fit to try, whether, at leaſt, bodies already burning might not be kept in that ſtate without the concurrence of air. And though in ſome of our formerly publiſhed phyſico-mechanical experiments it happened, that actual flame would ſcarce laſt a minute or two in our large pneumatical receiver; yet, becauſe it ſeemed not improbable, that mineral bodies once kindled might afford a vigorous, and very durable flame, we thought fit to deviſe, and make the following trials: whence probably we might receive ſome new informations about the diverſities, and ſome other phænomena of flame, and the various degrees, wherein the air is neceſſary, or helpful to them.

EXPERIMENT I.

Reciting an attempt to preſerve the flame of brimſtone without air.

WE put upon a thick metalline place a convenient quantity of flowers of ſulphur; and having kindled them in the air, we nimbly conveyed them into a receiver, and made haſte to pump out ſome of the included air, partly for other reaſons, and partly that the cavity of the receiver might be the ſooner freed from ſmoak, which would, if plentiful, both injure the flame, and hinder our ſight. As ſoon as the pump began to be plied, or preſently after, the flame appeared to be ſenſibly decayed, and continued to be leſſened at every exſuction of the air; and in effect it expired, before the air was quite drawn out. Nor did it, upon the early removal of the receiver, do any more than afford, for a very little while, ſomewhat more of ſmoak in the open air, than it appeared to do before.

The reiteration of this experiment preſently after afforded us nothing new, worth mentioning in this place.

EXPERIMENT II.

Relating a trial about the duration of the flame of ſulphur in vacuo Boyliano.

TO vary a little the foregoing experiment, and try to ſave ſome moments of time, which on theſe occaſions is to be huſbanded with the utmoſt care; having provided a cylinder of iron, larger than the former, that it might by its bulk, being once heated, both contribute to the accenſion of the ſulphur, and to the laſting of its flame, we made a trial, that I find regiſtred to this effect:

We took a pretty big lump of brimſtone, and tied it to the turning-key; and having got what elſe was neceſſary in a readineſs, we cauſed the iron-plate to be haſtily brought red-hot from the fire, and put upon a pedeſtal, that the flame might be the more conſpicuous; and having nimbly cemented on the receiver, we ſpeedily let down the ſuſpended brimſtone, till it reſted upon the red-hot iron, by which being kindled, it ſent up a great flame with copious fumes, which hindered us not from plying the pump, till we had, as we conjectured, emptied the receiver; which we could not do without withdrawing together with the air much ſulphureous ſmoke, that was offenſive enough both to the eyes and noſtrils. But notwithſtanding this pumping out of the air, though the flame did ſeem gradually to be ſomewhat impaired, yet it manifeſtly continued burning much longer, than by the ſhort duration of other flames in our receivers, when diligence is uſed to withdraw the air from them, one could have expected. And eſpecially one time, (for the experiment was made more than once,) the flame laſted, till the receiver was judged to be well exhauſted; and ſome thought it did ſo ſurvive the exhauſtion, that it went not out ſo much for want of air, as fuel; the brimſtone appearing, when we took off the receiver, either to have been conſumed by the fire, that fed on it, or to have caſually run off from the iron, whoſe heat had kept it conſtantly melted.

☞ IN caſe you ſhould have a mind to proſecute experiments of the nature of this and the precedent, it may not prove uſeleſs, if I intimate to you the following advertiſements.

1. For the red-hot iron above mentioned, we thought it not amiſs to provide, inſtead of the melting-pot employed in the firſt experiment, a pedeſtal, if I may ſo call it, made of a lump of dryed tobacco-pipe-clay, that the vehement heat of the iron might neither fill the receiver with the ſmoak of what it leaned on, nor injure the engine, if it ſhould reſt immediately upon that; and this pedeſtal ſhould be ſo placed, that the iron may be as far as you can from the ſides of the receiver, which elſe the exceſſive heat would endanger.

2. To the above-mentioned concave iron, that was to receive the brimſtone, we did for ſome occaſions cauſe to be fitted a thick convex piece of iron, ſhaped almoſt like a flattiſh button; which was not to be uſed conſtantly, but upon occaſion, that, being laid red-hot over the melted brimſtone, it might increaſe the heat, and keep the flame from having ſo broad a ſuperficies, whereby it would conſume its fuel too faſt.

3. WE ſometimes thought it expedient, for the clearer diſcerning of what ſhould happen in the receiver, to make the experiment by night, and remove the candles, when we were juſt about to pump, preſuming, that the flame would be conſpicuous enough by its own light; as indeed we found it to be, though its light were but dim, conſidering the greatneſs of the flame; whoſe colour, though it did not quite loſe its wonted blueiſhneſs, ſeemed yet to have received a great and ſomewhat odd alteration.

THERE is one great inconvenience, ſcarce avoidable in this experiment, *viz.* that the fumes aſcending very copiouſly do quickly much darken the receiver, and if the trial be long continued, line it with a kind of flower of brimſtone, which obſcures it much more, and therefore ought to be carefully wiped away, whenſoever the receiver is taken off; upon which account you will not, I preſume, wonder, if you ſhall find the phænomena of theſe experiments not always to be the very ſame with what you meet with in this paper; ſince, as it is very poſſible, that we may not have been able to obſerve things ſo accurately by reaſon of the newly mentioned fumes and flowers; ſo it is not impoſſible, that the difference, if there ſhall be any, of other men's obſervations from ours ſhould proceed from the ſame cauſe.

BEFORE we paſs from this ſecond experiment, it will not be amiſs to take notice, that though the flames of brimſtone may be allowed to be ſomewhat more durable than the flames of vegetables are wont to be; yet it is not ſafe to conclude, that it was meerly upon the account of their native vigour, that the flames above mentioned laſted ſo long in our receiver.

FOR we ſeemed to obſerve, that there was requiſite a very intenſe heat of the iron to make the ſulphur capable of flaming on it, when any conſiderable proportion of air was withdrawn. For which reaſon it ſeems expedient, according to what I lately intimated, that the iron, that is to keep it melted, be of a good thickneſs, that it may the longer retain a competent heat; and we thought it contributed to the ſucceſsfulleſt trials we made, that in them we uſed, beſides the concave iron, the convex one mentioned in the ſecond note.

EXPERIMENT III.

Of the laſting of the flame of a metalline ſubſtance in the ſame vacuum.

THOSE ſulphurs, that chemiſts call metalline, being ſuppoſed by many to be of a much more fixed nature than common ſulphur, and it being indeed probable enough, that in them good ſtore of very minute particles are crowded together, I thought fit to try, whether a body, wherein a vulgar chemiſt would think the ſulphur of a metal to be the main ingredient, would afford in our vacuum a more vigorous or laſting flame, than that of common ſulphur. And, though I will not here trouble you with my particular ſcruples about the chemiſts doctrine concerning metalline ſulphurs, nor with the grounds, on which I deviſed the following inflammable ſolution of Mars, (for I do not now give it a more determinate name) which ſome chemiſts will not perhaps diſlike; I ſhall here annex the enſuing tranſcript of the trial itſelf.

HAVING provided a ſaline ſpirit, which by an uncommon way of preparation was made exceeding ſharp and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of filings of ſteel, which were not ſuch, as are commonly ſold in ſhops to chemiſts and apothecaries, (thoſe being uſually not free enough from ruſt) but ſuch as I had a while before cauſed to be purpoſely filed off from a piece of good ſteel. This metalline powder being moiſtened in the vial with a little of the menſtruum, was afterwards drenched with more; whereupon the mixture grew very hot, and belched up copious and ſtinking fumes; which, whether they conſiſted altogether of the volatile ſulphur of the Mars, or of metalline ſteams participating of a ſulphureous nature, and joined with the ſaline exhalations of the menſtruum, is not neceſſary to be here diſcuſſed. But whenceſoever this ſtinking ſmoke proceeded, ſo inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a blueiſh, and ſomewhat greeniſh flame, at the mouth of the vial, for a good while together; and that, though with little light, yet with more ſtrength, than one would eaſily ſuſpect.

This flaming vial therefore we conveyed into a receiver, which he, who uſed to manage the pump, affirmed, that about ſix exſuctions would exhauſt. And the receiver being well cemented on, upon the firſt ſuck the flame ſuddenly appeared four or five times as great as before; which I aſcribed to this, that upon the withdrawing of the air, and conſequently the weakning of its preſſure, great ſtore of bubbles were produced in the menſtruum, which breaking could not but ſupply the neck of the vial with ſtore of inflammable ſteams, which, as we thought, took not fire without ſome noiſe: upon the ſecond exſuction of the air, the flame blazed out as before, and ſo it likewiſe did upon the third exſuction, but after that it went out; nor could we re-kindle any fire by haſtily removing the receiver; only we found, that there remained ſuch a diſpoſition in the ſmoke to inflammability, that holding a lighted candle to it, a flame was quickly rekindled.

EXPERIMENT IV.

Of the duration of the flame of ſpirit of wine impregnated with a metal in the exhauſted receiver.

BECAUSE it may, upon grounds not improbable, be thought, that well-dephlegmed ſpirit of wine being a pure æthereal liquor, which does not, like combuſtible ſulphurs (whether vulgar or metalline) emit any viſible ſmoak to ſtifle the flame (into which it may, in the free air, be totally reſolved;) if this ſpirituous, and thus qualified liquor, could be duely aſſociated with a metalline body, the reſulting flame might be more than ordinarily vigorous and durable; I reſolved to make an experiment of this ſort, and having by a way, that I delivered in another paper [in a Paradox about the fuel of Flames] ſo united highly rectified ſpirit of wine with a prepared metal, that they would both afford a conſpicuouſly tincted flame; we put this mixture into a ſmall glaſs-lamp, made on purpoſe, and furniſhed with a very ſlender wick, which the mixture would not burn, whilſt there was liquor enough to imbibe it well; and putting this lighted lamp into a convenient place of a receiver, that was not ſmall, ſince it was able to contain about two gallons, or ſixteen pounds of water, we made haſte to cement on the glaſs to the engine, and yet found not in two or three ſeveral trials, that after the pump began to be moved, ſo little a quantity of tincted flame in that capacious glaſs laſted much, if at all, more than half a minute of an hour, eſtimated by a minute watch.

And becauſe the receiver, we then made uſe of, ſeemed to me, by reaſon of its ſize, and ſome accommodations, that belong to it, proper enough to be employed about other trials, concerning the relation between flame and air; I thought fit to try, with the ſame ſmall lamp and liquor, what other phænomena of that kind would be afforded by letting air in and out, according to the various exigencies of my particular aims.

But not having then, nor in ſome time after, the leiſure and opportunity of ſetting down things circumſtantially, I contented myſelf to take thoſe ſhort notes of the principal things, whereof I now ſubjoin the tranſcript.

When the flame began to decay, the turning key being now and then drawn almoſt out, the tincted flame laſted once a minute and a half, and another time longer.

The turning key being taken out in the beginning, the flame laſted two minutes or better.

A pipe bedded in the cement at the bottom of the glaſs, and having at each end an open orifice almoſt of the bigneſs of that filled by the turning key, which key was then removed from the top; the tincted ſpirit ſeemed to burn very conveniently, as if the flame would have burned very long, if we would have permitted it ſo to do.

The orifice at the top being ſtopped with the turning key, though the pipe were left open at the bottom, it plainly, in a ſhort time, ſeemed much to decay, and ready to expire; whereupon I cauſed one to blow conſtantly, yet but very gently, in at the pipe with a pair of bellows, and by this means, though we did not keep the flame vigorous, yet we kept it alive for above four minutes; and then obſerving it to be manifeſtly ſtronger, than it was, when we began to refreſh it with the bellows, we ceaſed from blowing, and found, that though the glaſs-pipe was ſtill left open, yet within about one minute the flame was quite extinguiſhed.

EXPERIMENT V.

Of the conſervation of flame under water.

THE better to examine the neceſſity of air to flame, I thought fit not only to make the ſeveral trials mentioned in this paper, whether it would live in a medium much thinner than air; but alſo to try, whether it would be able to continue in a medium many hundred times thicker than air, namely in water.

I doubted, not but many would think this both an eaſy and a needleſs inquiry, ſince eminent writers, both ancient and modern, tell us without ſcruple, that Naptha and Camphire will burn under water; but I had never the good fortune to be able to make them do ſo; and may be allowed to doubt, whether theſe writers, notwithſtanding their confidence, deliver what they affirm, upon experience, not bare tradition. And though in celebrated authors I have met with divers receipts of making compoſitions, that will not only burn under water, but be kindled by it; yet I have found thoſe, I had occaſion to conſider, to be ſo lamely, or ſo darkly (and ſome of them I fear ſo falſely) ſet down, that by the following compoſition, how ſlight ſoever it may ſeem, I have been able to do more, than with things they ſpeak very promiſingly of; ſince, though it will not be kindled by water, yet being once kindled, it will continue to burn under water.

AND that there might be no ſuſpicion, that whilſt the mixture continued under water, it did only, as it were, vehemently ferment, or ſuffer a violent agitation of its parts without having them kindled, till in their aſcending they were actually fired by the contact of the air, incumbent on the ſurface of the water; to obviate this ſuſpicion (I ſay) we were careful to try the experiment, not only in other veſſels, but in a large glaſs, the tranſparency of whoſe ſides, as well as that of the contained water, would permit us to ſee, for a while, the burning of our compoſition, which was ſometimes with a weight detained, and ſometimes with a forceps held, till it was conſumed, a good way under the ſurface of the water.

THE way of making the experiment is this: we took of gunpowder three ounces, of well burned charcoal one drachm, of good ſulphur or flower of brimſtone a little leſs than half a drachm, of choice ſalt-petre near a drachm and a half: which ingredients being well reduced to powder, and diligently mingled without any liquor, either a large gooſe-quil, whoſe feathery part was cut off, or a piece of a tobacco-pipe, of two or three inches long, and well ſtopped at one end, had its cavity well filled with this mixture, (inſtead of which, beaten gunpowder alone might ſerve, if it did not operate too violently, or waſte too ſoon:) for the kindling whereof, the open orifice of the quil or pipe was carefully ſtopped with a convenient quantity of the ſame mixture, made up with as little chemical oil or water, as would bring it to a fit conſiſtence. This wild-fire was kindled in the air, and the quill or pipe, together with a weight, to which it was tied to keep it from aſcending, was ſlowly let down to a convenient depth under water, where it would continue to burn, as appeared by the great ſmoak it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the quill or pipe kept the dry mixture from being acceſſible to the water (that would have diſordered and ſpoiled it) at any other part than the upper orifice; and there the ſtream of kindled matter iſſued out with ſuch violence, as did inceſſantly beat off the neighbouring water, and kept it from entering into the cavity, that contained the mixture, which therefore would continue burning, till it was conſumed.

It is probable, that moſt men will conclude from this experiment, that air is not ſo abſolutely neceſſary to the duration of flame, as ſome other of our trials ſeem to argue; and that there ought to be a difference made between ordinary flames, and thoſe, that burn with an extraordinary vehemency. But my deſign being, as I long ſince intimated, rather to relate trials, than debate hypotheſes, I ſhall only add, that it may be pretended on the behalf of the opinion, that this experiment ſeems to diſprove, that, not to mention the air, that may lurk in the pores of the water, or that, which may be intercepted between the little grains of powder, whereof the mixture conſiſts, the ſalt-petre itſelf may be ſuppoſed to be of ſuch a texture, that in its very formation the corpuſcles, that compoſe it, may intercept ſtore of little aereal particles between the very minute ſolid ones, which thoſe corpuſcles are made up of. And this inexiſtence of the air in nitre may be probably argued from the great windineſs of the flame, that is produced upon the deflagration of nitre. According to this ſurmiſe, though our mixture burns under water, yet it does not burn without air, being ſupplied with enough to ſerve the turn by the numerous eruptions of the aereal particles of the diſſipated nitre itſelf.

ON this occaſion I remember, that in another paper I relate, that for divers purpoſes, and among them to remove this ſuſpicion, I ſucceſsfully tried to reproduce nitre in *Vacuo Boyliano*, that there might not be any air, or at leaſt any quantity worth heeding, intercepted between the convening particles, that by their coalitions made up the nitrous corpuſcles, which, in favour of the neceſſity of air to flame, may be pretended to be but ſo many little empty bubbles cloſe ſtopped, whoſe moiſter parts may, by the fire, that kindles the nitre, be exceedingly rarified, and in that eſtate emulate air, and violently burſt their little priſons, and throw about the fragments of them with force, and in numbers enough to make their aggregate appear ſuch a flame, as is wont to be be made by unctuous and truly combuſtible bodies; and yet this rarified ſubſtance, that thus ſhatters the nitrous particles, may really be no true and laſting air, but only vehemently agitated vapours, which preſently, upon the ceſſation of the heat, return to liquor; as we ſee, that the vapours of an Æolipile, that iſſue out after the aereal particles have been expelled, though they make a great noiſe and a temporary wind near the hole they ſtream out at, and would perhaps, if that hole were cloſe ſtopped, break the Æolipile; yet are not true and permanent air, but at a ſmall diſtance off the inſtrument return into water.

BUT though I could ſuggeſt other ſuſpicions and conjectures about the incluſion of air between the particles of ſalt-petre, yet I forbear to mention them in a writing deſigned to be chiefly hiſtorical.

EXPERIMENT VI.

Relating an odd phænomenon about the flame of a metal in our vacuum.

TO the foregoing experiments made on purpoſe I ſhall add a phænomenon afforded us by chance, and yet not unworthy to accompany the reſt.

WHILST we were trying to kindle ſomething in our exhauſted receiver, it happened by ſome accident or other, that the combuſtible ſubſtance, that was to be kindled, fell beſides the iron, whereby our intended trial was defeated. But whilſt we were conſidering what was to be done on this occaſion, and had not yet let in the air, that had been pumped out, the lights alſo continuing yet removed; we were ſurpriſed to ſee ſomething burn, like a pale blueiſh flame almoſt in the midſt of the cavity of the receiver, and at firſt ſuſpected it to be ſome illuſion of the eyes; but all the by-ſtanders perceiving it alike, and obſerving, that it grew very broad, we looked at it with great attention, and found it to laſt much longer, than I remember I have ſeen any flame do in an exhauſted receiver. I ſhould have ſuſpected, it had proceeded from ſome brimſtone, ſticking, without our heeding it, to ſome part of the iron, which we had formerly employed to kindle ſulphur in our receiver, had it not been, that, beſides other things, I remembered, that we had juſt before kept it red-hot in the fire, and conſequently muſt have burned away any little brimſtone, if there were any, that adhered to it: but though we much wondered, whence this our flame proceeded, I would not let any thing be done, that might haſten its extinction; and at length, when it expired of its ſelf, we let in the air, which had been till then kept out, and perceived upon the concave part of the iron (which we judged to be the place, where the flame had appeared) a piece of melted metal, which we concluded had been faſtened to the ſtring, that the fuel we deſigned to kindle had been tied to, in order to the letting it down the more eaſily: and this made us conceive, that the ſtring happening to be burned by the exceſſive heat of the iron, the piece of metal fell into the cavity of it, and, by the ſame heat, the more combuſtible part, which the chemiſts call the ſulphur, was melted and kept on fire, and continued burning ſo long as we have related. The piece of metal was judged to be lead, but having not formerly obſerved ſuch a diſpoſition in that metal to be inflamed, I conſidered it attentively, and perceived, that it was ſome fragment, that the operator had chanced to light on, of a mixture of lead and tin, that I had (a while before, for an experiment not at all belonging to our preſent ſubject) cauſed to be colliquated in a certain proportion. Upon whoſe account it ſeems, the mixture of the ingredients had acquired ſuch a new texture, as, whether by making the bodies open one another, or by what other means ſoever, fitted the maſs to afford us the phænomenon above recited. And though I made an unſucceſsful trial with a mixture of lead and tin, to produce ſuch a flame upon the heated iron in the open air; yet the newly related experiment may ſuffice to argue, that there may be flames of metalline ſulphurs (as the chemiſts call them) that will be, at leaſt, as eaſily produced without the concurrence of the air, as that of common ſulphur, and continue to burn in our vacuum longer than it.

THE THIRD TITLE.

Of the ſtrangely difficult Propagation of ACTUAL FLAME *in vacuo Boyliano*.

I Have more than once obſerved, that ſome bodies (whereof I make particular mention in another paper) though they will not be turned into flame by very intenſe heats, and thoſe of very differing kinds, are yet very readily kindled by an actual flame. So that the propagation of flame to contiguous bodies, that, according to the hitherto obſerved, and unqueſtioned courſe of things, muſt thereby in a moment, as it were, be actually inflamed, ſeems to be not only very eaſy, but almoſt infallible: and yet, that this propagation is not eaſy, or is perhaps ſcarce poſſible to be performed without the aſſiſting preſence of the air, may be gathered from the next following experiments; at whoſe titles though you will probably be ſurprized, in regard, that by the two firſt experiments of the firſt title of this tract, it will ſcarce be expected, that ſulphur ſhould be kindled in our vacuum; yet I preſume your wonder will ceaſe, when I put you in mind, that I formerly took notice to you of my having ſometimes met with ſuch ſulphur as would be kindled there; and it was whilſt that well-diſpoſed parcel of ſulphur laſted, that I took the opportunity of making with the flame of it the trials, to which I now proceed.

EXPERIMENT I.

An ineffectual attempt to make flame kindle ſpunk in an exhauſted receiver.

HAVING placed the often mention cylindrical plate of iron, firſt brought to be red-hot, in a receiver, capable of containing two gallons of water; and having alſo diligently pumped out the air, we kindled a little ſulphur upon the heated plate, and then a piece of dried ſpunk, tied to a ſtring, was, by the help of a turning key, let down to the flame; and when the experiment was finiſhed, and the ſpunk was taken out, we found it in divers places, not manifeſtly altered ſo much as in colour; and in thoſe parts, that had been moſt expoſed to the flame, it was turned to a ſubſtance very differing from aſhes, being black and brittle as tinder, and, like it, exceedingly diſpoſed to be kindled upon the touch of the fire.

EXPERIMENT II.

An unproſperous attempt to make flame kindle camphire without the help of air.

AS a farther confirmation of the difficulty of propagating flame in our vacuum, we may annex the following trials.

INTO the lately mentioned receiver we conveyed the cylindrical plate of iron, made uſe of in the former experiment; and when the air had been diligently pumped out, we did, by the help of the turning key, let down upon the hot iron a piece of ſuch brimſtone, as would, in ſpite of ſo diſadvantageous a place, be kindled with that heat. A little above this ſulphur we had tied to the ſame ſtring a piece of camphire, that being a body exceedingly apt to take fire, if not, as it were to draw it, at the flame of lighted brimſtone. But our ſulphur melting with the heat of the iron cylinder, dropt unluckily from the ſtring it was faſtened to before, and for the moſt part fell off. And as ſoon as it came to the ground, where it was diſtant from the vehement heat of the metal, the flame expired, and that part of the ſulphur, that happened to ſtick to the ſide of the iron, was inflamed by it. And I, that chanced to be then in an inconvenient poſture for ſeeing the camphire, could not, becauſe of the ſmoke of the extinguiſhed brimſtone, well diſcern what became of it. But my amanuenſis, that happened to be on the beſt ſide of the receiver, affirmed, he plainly ſaw the flame of the brimſtone reached the camphire, without being able to make it flame. Which ſeemed the leſs to be doubted of, becauſe the camphire was by help of the turning key let down low enough, and if it had afforded a flame, the difference of colours betwixt that and the blue flame of ſulphur would have made it very eaſy for me to have diſtinguiſhed them.

ANOTHER trial I would have thoroughly made to kindle one piece of ſulphur in our vacuum by the flame of another, tied a little lower in the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire: but, though we could find nothing, that was viſibly amiſs in the kind of ſulphur we then uſed, yet we were not able, even by a reiterated trial, to make it take fire upon the iron, where nevertheleſs it melted and ſeemed a little to boil.

A third trial was not ſo unſucceſsful; for having in the well-exhauſted receiver let down upon the very hot iron a match, made of a piece of card dipped in brimſtone, the lower extream of it was kindled by the contact of the hot iron. But though the ſulphurated part of the match thus flamed away, yet the remaining part, which was a meer piece of card, was not thereby turned into flame, nor in moſt places ſo much as ſenſibly ſcorched or blacked; though, as I remember, the match had been purpoſely dryed before-hand to facilitate its inflammation.

EXPERIMENT III.

A ſtrange experiment upon gunpowder, ſhewing, that though it were fired itſelf, yet it would not fire the contiguous grains in vacuo Boyliano.

THE preceding trials may ſuffice to manifeſt the difficulty of communicating flame, without the help of air, from one body to another, even when the bodies to be kindled are of a very inflammable nature. But becauſe there is no propagation of flame made in any bodies, that we converſe with here below, with any thing near ſuch celerity, as in the contiguous grains of gunpowder; a great heap whereof will, almoſt in the twinkling of an eye, be turned into flame by propagation from any one ſmall kindled grain; nothing ſeemed fitter to manifeſt, how much flame is beholden to air, than if ſuch an experiment could be made, as might ſhew, that, even amongſt the contiguous grains of kindled gunpowder, flame would not be propagated without the help of air. How far a trial of this nature may be made in our engine, the following narratives will beſt declare.

WE took ſome paper, and laying it upon ſome convenient part of the plate of the engine, we made upon it a train of dry powder, as long as the glaſs would well cover; then, carefully faſtening on the receiver with good cement, we ſolicitouſly pumped out the air; which done, we took a good burning-glaſs, and about noon caſt the ſun-beams through it upon the train of ſome gunpowder: where, though the indiſpoſition to accenſion was ſo great, that the powder did not only ſmoke, but melt without going off, and the operator, though verſed in ſuch experiments would not allow, that it would ſignify any thing to continue the trial any longer; yet, upon my being obſtinate to proſecute it, he, being willing to follow the experiment, rationally conſidered, that the receiver, we had been hitherto fain to uſe, was ſo opacous, as to reſiſt the entrance of many of the beams, that ſhould have their operation upon the powder: whereupon taking a finer glaſs, that was lately come in, we laid by the former, and employed that, which, by reaſon of its tranſparency, ſo little weakned the beams of the ſun, that being according to my direction held obſtinately upon the ſame parts of the train, they were able to fire ſeveral of them one after another. But though the ſun could thus kindle the powder, yet it could not make the flame propagate, but only thoſe parts, that were melted, did at length kindle and fly away, leaving the reſt unaltered, as I curiouſly obſerved, finding ſeveral little maſſes of colliquated matter in ſeveral places of the train, with the powder unchanged in all the other parts of the ſame train, that lay in a direct line; beſides that ſome of the little colliquated maſſes were contiguous to the reſt of the powder, which appeared unchanged, and kindled readily, and flaſhed all away, as ſoon as I cauſed the burning-glaſs to be applied to it in the open air.

EXPERIMENT IV.

Reciting another attempt to confirm the former.

FOR further confirmation of ſo odd an experiment, I ſhall alſo add a ſhort account of another made with gunpowder in our vacuum.

To try on an occaſion, that need not here be diſcourſed of, whether, by the help of one of thoſe little inſtruments, that are now uſed at *London*, to examine the ſtrength of powder, we could find any difference made by the abſence and preſence of the air, in the reſiſtance of the inſtrument, or the effects of the powder on it; we faſtened it to a competently heavy and commodiouſly ſhaped weight of lead; and when it was carefully filled and primed with powder, we placed it in a receiver of a convenient bigneſs, whence we pumped out the air after the uſual manner, and perhaps with more than uſual diligence. But though at length, after the powder had long reſiſted the beams of the ſun, concentrated on it by a good double convex burning-glaſs, it did, as I expected, take fire at the touch-hole, and fill the receiver with ſmoke; yet this kindled powder could not propagate the flame to that, which was in the box, how contiguous ſoever the two parcels were to one another. And when the inſtrument was taken out into the air, (by which it appeared how free the touch-hole was) as ſoon as ever new-priming, with the ſame ſort of powder, was put to it, the whole very readily went off: and when, for further ſatisfaction, we cauſed the inſtrument to be new charged, and upon its taking fire only at the touch-hole in the exhauſted receiver, we ordered new priming to be added, without ſo much as taking the inſtrument out of the receiver, though afterwards the receiver was cloſed again, but without being exhauſted of air; the powder, though cloſely ſhut up in the glaſs, did readily go off, as well that, which was in the box or cavity of the powder-trier, as that, which lay on the outward part of the inſtrument. And this trial, for the main, was repeated with the like ſucceſs.

EXPERIMENT V.

Briefly mentioning two differing trials, with two differing events, to kindle gunpowder in our vacuum.

YOU will eaſily believe, that the event of the foregoing trials ſeemed ſtrange enough to the ingenious perſons, that I had deſired to be preſent at them; and perhaps, the attentive conſideration of it may well enough ſuggeſt ſuch odd ſuſpicions and conjectures, as I have neither the leiſure, nor the boldneſs to diſcourſe of in this place.

BUT here I ſhall not diſſemble my having, by a ſomewhat differing way, made a couple of trials, whereof, though the firſt may confirm the great indiſpoſition of gunpowder to be kindled in our vacuum, yet the ſecond ſeems to look another way.

THE firſt is ſummarily ſet down in my notes to this purpoſe. [A few ſmall corns of gunpowder being included in a very ſmall bubble freed from its air, and ſecured againſt the return of it, or any other, and then applied warily to coals covered with aſhes, did not go off, nor burn, but afforded a little yellow powder, that ſeemed to be ſulphur, and ſublimed to the upper part of the glaſs.]

THE latter's event I found in the ſame paper to have been thus regiſtered. [But two larger bubbles, though ſtrong, whereof one had the air but in part, and the other carefully emptied, being provided, each of them, with a greater quantity of powder (though ſcarce enough to promiſe ſuch an effect) a while after they were put upon quick coals, each of them was blown in pieces, with a report almoſt like that of a muſket; but though this was done in a dark place, yet we did not perceive, whether or no there were any real flame produced.]

THE event of this trial ſeems at firſt ſight to contradict the inference, that probably you have drawn from the foregoing experiments. But yet it may not be unworthy of our inquiry, whether this way of trial be as proper to give ſatisfaction to the curious, as that, made with the ſun-beams, was. And I leave it to be conſidered, whether or no it may not be doubted, whether the going off of the gunpowder was cauſed by a ſucceſſive, though extremely ſwift, propagation of real flame, from the firſt kindled grains to the reſt; or did not proceed from this, that the coals acting ſtrongly at the ſame time on the whole area, or extent of the powder, that was next to them, and this in the abſence of the air, each grain was in that caſe, as it were, a little granado, and the heap of them, being uniformly enough acted on by the fire, they were made to go off, as to ſenſe, all at once, as if there had been but a contemporary exploſion made of them all together by the action of the external fire, rather than any true accenſion made by the flaming grains of the unkindled ones. As I remember I have tried, that even in the open air one may, with a burning-glaſs dexterouſly employed, make ſome part of a little parcel of aurum fulminans go off, whilſt the neighbouring parts of the ſame parcel, to which the focus does not extend with heat enough, will not be made to do ſo.

NEW EXPERIMENTS

About the RELATION betwixt

AIR and the FLAMMA VITALIS of ANIMALS.

(Sent to the ſame Perſon, to whom the former Papers were addreſſed.)

THE twenty experiments hitherto ſet down under the three foregoing titles, by ſhewing the relation betwixt air and flame in general, may be ſerviceable to the inquirers into the nature of the vital flame in particular. But yet having had occaſion to make ſome trials, that more directly regard the requiſiteneſs of air to the flamma vitalis or vital principle of animals; I ſhall now preſent you by themſelves, as many as I could light on, without being ſolicitous, that they ſhould be quite differing from each other; becauſe in ſo new and nice a ſubject, the affinity, that may be found between ſome, either in regard of the ſubjects expoſed to trial, or in the manner of making it, may be uſeful, if not neceſſary, to confirm things by the reſemblance of events, or make us proceed cautiouſly and diſtinctly in pronouncing upon caſes, where the ſucceſs was not uniform.

EXPERIMENT I.

Wherein the durations of the life of an animal, and of the flame of ſpirit of wine, included together in a cloſe veſſel, were compared.

WE took ſome highly rectified ſpirit of wine, and put about a ſpoonful of it into a ſmall glaſs-lamp, conveniently ſhaped and purpoſely blown with a very ſmall orifice, at which we put in a little cotton-wick, which was but very ſlender.

WE alſo provided a tall glaſs-receiver, which was in length eighteen inches, and contained above twenty pints of water. This receiver, which was open at both ends, was at the upper orifice (which was not wide) covered with a braſs plate, faſtened on very cloſe with good cement, for uſes, whoſe mention belongeth not to this place; and for the lower orifice, which was far the wideſt, we had provided a braſs-plate furniſhed with a competent quantity of the cement we employed to keep the air out of the pneumatical engine; by means of which plate and cement, we could ſufficiently cloſe the lower orifice (though a wide one) of our receiver, and hinder the air from getting in at it.

THESE things being thus prepared, we took the ſmall glaſs lamp above-mentioned, and having lighted it, we placed both it, and a ſmall bird, (which was a green-finch) upon the braſs plate, and in a trice faſtned it to the lower orifice of the receiver, and then watched the event; which was, that within two minutes (as near as we could eſtimate by a good minute-watch) the flame, after having ſeveral times almoſt quite diſappeared, was utterly extinguiſhed; but the bird, though for a while he ſeemed to cloſe his eyes, as though he were ſick, appeared lively enough at the end of the third minute; at which time, being unwilling to wait any longer by reaſon of ſome avocations, I cauſed him to be taken out.

AFTER he had for a pretty while, by being kept in the free air, recovered and refreſhed himſelf, the former trial was repeated again, and at the end of the ſecond minute, the flame of the lamp went out; but the bird ſeemed not to be endangered by being kept there a while longer.

AFTER this, we put in, together with the ſame bird, two lighted lamps at once, viz. the former and another like it, whoſe flames, according to expectation, laſted not one whole minute, before they went out together. But the bird appeared not to have been harmed, after having been kept five or ſix times as long before we took off the receiver.

In the tall receiver above mentioned we included a mouſe, with a lighted lamp filled with the ſpirit of wine; but before the experiment was near finiſhed, the mouſe, being at liberty within the glaſs, made a ſhift to blow out the flame; which being revived without taking out either the lamp or the animal, the ſpirit of wine burned about a minute longer, during which time the mouſe appeared not to be grown ſick, no more than it did afterwards, when, for ſome minutes after the extinction of the flame, he had been kept in the ſame cloſe and infected air.

AFTERWARDS we placed the ſame mouſe in another receiver, which ſeemed to be by a third part leſs capacious than the former, and in it we alſo fixed a piece of ſlender wax-candle, ſuch as is wont to be made up in rolls, and employed to light tobacco. This candle continued burning in this new receiver but for one minute, during which time it emitted ſtore of ſmoke; but this not hindering the animal to appear lively enough, even after we had kept him much longer in that infected air, the ſame candle, without being taken out, was lighted again, but burned not ſo long as before; yet it ſufficed to darken the receiver, and therefore probably much to clog the included air, in which nevertheleſs the mouſe being kept, by our gueſs, eight or ten minutes longer, he appeared, neither when he was taken out, nor a while before, to have received any conſiderable harm by his detention there.

EXPERIMENT II.

Of the duration of the life of a bird, compared with the laſting of a burning candle and coal in our vacuum.

WE took a green-finch and a piece of candle of twelve to the pound, and included them in a great capped receiver, capable of containing about two gallons, or ſixteen pound of water, which was very carefully cemented on to the pump, that no air might get in or out. In this glaſs we ſuffered the candle to burn till the flame expired, (which it did, in more than one trial, within two minutes or ſomewhat leſs;) at which time the bird ſeemed to be in no danger of ſudden death; and, though kept a while longer in that clogged and ſmoky air, appeared to be well enough, when the receiver was removed. Afterwards, we put the ſame bird into the receiver with a piece of a ſmall wax taper, whoſe flame, though it laſted longer than the other, yet the bird outlived it; and it was judged he would have done ſo, though the flame had been much more durable. After this, we included the ſame bird with the firſt-mentioned candle in the receiver, which we had cauſed to be often blown into with a pair of bellows, to drive out the ſmoke and infected air; and then beginning to pump out the air, we found, that the flame began more quickly to decay, and the bird to be much more diſcompoſed, than in the former experiments; but ſtill the animal outlived the flame, though not without convulſive motions. The experiment we repeated with a piece of the fore-mentioned taper, and the ſame bird; which, though caſt into threatening ſymptoms upon the gradual withdrawing of the air, outlived, not only the flame, but the ſmoke too, that iſſued from the kindled wick, which circumſtance was alſo obſerved in the preceding trial.

LASTLY, having freed the receiver from ſmoke, and ſupplied it with freſh air, we put in with the ſame bird a piece of charcoal of about two inches in length, and half an inch in breadth, which had been, juſt before it was put in, well blown with a pair of bellows, that it might be freed from aſhes, and thoroughly kindled; and made haſte to pump out the air. This diligence was continued not only till none of the fire could be diſcerned by any of the by-ſtanders, but till, in our eſtimation, (which the event juſtified) it was irrecoverable by the admiſſion of the outward air; which at its coming in found the bird very ſick indeed, but yet capable of a very quick recovery. And this experiment was, with the ſame animal and coal re-kindled, tried over again with the ſame ſucceſs.

WHETHER this ſurvival of animals, not only to a flame, that emits ſtore of fuliginous ſteams, as in this trial, but to that, which is made of ſo pure a fuel as ſpirit of wine, that affords not ſuch ſteams, as in the former experiment; whether, I ſay, this ſurvival proceed from this, that the common flame and the vital flame are maintained by diſtinct ſubſtances or parts of the air; or, that common flame making a great waſte of the aerial ſubſtance, they both need to keep them alive, cannot ſo eaſily as the other find matter to prey upon, and ſo expires, whilſt there yet remains enough to keep alive the more temperate vital flame; or, that both theſe cauſes, and perhaps ſome other, concur to the phænomenon, I leave to be conſidered.

EXPERIMENT III.

Of what happened to the light of glow-worms in the exhauſted receiver.

FOR the ſake of thoſe learned men, that have thought the light of glow-worms and other ſhining inſects to be a kind of effulſion of the biolychnium, or vital flame, that nature has made more luminous in theſe little animals than in others; and which a very eminent phyſician of the college of *London* affirms to have felt in a warm climate more than ſenſibly hot; I ſhall ſubjoin on this occaſion ſome trials made on glow-worms, which elſe ſhould be referred to thoſe experiments of mine about the relation betwixt air and light, that you were formerly pleaſed to publiſh.

WE took two glow-worms, that ſhone vividly enough, eſpecially one of them, whoſe light appeared ſtrong and tincted, as if it had been tranſmitted through a blue glaſs: theſe we laid upon a little plate, which we included in a ſmall receiver of finer glaſs than ordinary, that we might the better ſee what would happen: and having for the ſame purpoſe removed the candles, that no other light might obſcure that of the inſects, we waited in the dark, till that was conſpicuous, and then ordered the air to be begun to be pumped out; and, as we expected, upon the very firſt exſuction there began to be a very manifeſt diminution of the light, which grew dimmer and dimmer, as the air was more and more withdrawn, till at length it quite diſappeared, though there were young eyes among the aſſiſtants. This darkneſs having been ſuffered to continue a long while in the receiver, we let in the air again, whoſe preſence, as we looked for, reſtored at leaſt as much light as its abſence had deprived us of. This experiment was repeated with one more of thoſe inſects; and the event was, that they all three gradually loſt their light by the exhauſtion of the receiver, and regained it, with ſome increaſe, as was judged) by the return of the air. And in this experiment we let in the air by degrees, and with an interval or two, to obſerve, as we did, that, as the diminution of light was greater and greater, when the air was more and more withdrawn, ſo the returning ſplendor was gradually increaſed, as we pleaſed to let in more and more air upon the worms.

EXPERIMENT IV.

Containing a variation and improvement of the foregoing trial.

BUT here I foreſaw, it might be ſuſpected, that the diſappearing of the light in our exhauſted receiver did not ſo much proceed from any real, though but temporary, extinction or eclipſe of it, as from this, that the glow-worms having, as I have often obſerved, a power of drawing the luminous part into the opacous part of their body, they might, finding themſelves prejudiced by the withdrawing of the air, hide their light from our eyes, without loſing it, till being again refreſhed by the return of the air, they might be invited to protrude it again into the tranſparent part of their tails. This ſcruple ſeeming grounded upon the nature of the thing, I thought it worth while to remove it by the help of another obſervation, that I long ſince made, and have mentioned elſewhere about glow-worms. Which is this, that, if they be killed whilſt they are ſhining, their luminous matter may continue to ſhine for a good while after it is taken out of their bodies; and accordingly having put ſome of that, we took out of the forementioned inſects, upon a little paper, and included it in the receiver we employed, the candles being removed, we perceived it to ſhine vividly enough before the pump was ſet on work, and afterwards to grow dimmer and dimmer, as the air was more and more drawn out, till at length it quite vaniſhed; and it re-appeared immediately upon the air's return. This experiment was reiterated twice more with the ſame ſucceſs for the main. But we took notice, that the luminous matter, after the air was let in, ſeemed to us not only to have regained its former degree of light, but ſenſibly increaſed it, (as it once happened alſo in the experiment made on the living worms) which, whether it was cauſed by any real change made by the receſs and acceſs of the air in the matter itſelf, or by the greater accuſtomance of our eyes to the darkneſs of the place, I diſpute not; and ſhall only add this phænomenon of one of our trials, that having a mind to ſee, whether a very little proportion of returning air would not ſuffice to reſtore ſome little light to the diſappearing matter, it was ſomewhat ſtrange to obſerve, that ſo very ſmall a quantity of air, as was let in before the light was revived, was enough to make it become plainly viſible, though but dim; in which ſtate it continued, till we thought fit to let in more air upon it. Farther trials I could not make with theſe glow-worms, having received them but that night out of the country, and being the next morning to begin a journey.

EXPERIMENT V.

Wherein the former inquiry is farther proſecuted.

AFTER the lately mentioned trials we made with the glow-worms, having procured two or three other of thoſe inſects, whereof one was judged to be as large as three ordinary ones, we found, when we had brought them out of the country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a conſiderable degree of luminouſneſs in the under part of his tail, we put him into the ſmall receiver formerly mentioned, to try, whether, after the death of the animal, the ſhining matter would retain its former properties; but at the firſt time the air was pumped out after the uſual manner, the light was not only not aboliſhed, but continued vivid enough, and ſo it did, when the air being let in, and again withdrawn, the trial was made a ſecond time. But being unwilling to abandon the experiment till we tried it yet further, I cauſed the receiver to be exhauſted yet once or twice more, and at length I perceived, that the light began to diminiſh, as the air was withdrawn; and laſt of all, it ſo diſappeared, that the by-ſtanders could not ſee it, whereas upon the readmiſſion of the air, the light ſhone vividly as before, if not more bright. This experiment was reiterated with the like ſucceſs, and in both theſe times the like happened to the light of the dead one, and of a living one, that we included with it, to be able to compare them together; though there were this diſparity betwixt them, that the luminous part of the dead worm was much larger than that of the living, and the light of the latter appeared of a very greeniſh blue, whereas that of the former ſeemed to be of a white yellow.

EXPERIMENT VI.

Made to examine, whether animals be heavier dead than alive.

IT is a received tradition, that bodies, when dead, are much heavier than the ſame were when alive: the matter of fact being taken for granted, ſome will perhaps aſcribe the change to the utter inability of a dead body any way to aſſiſt thoſe, that endeavour to remove it. But, according to the general opinion, this difference proceeds from the total extinction or receſs of the ſpirits vital and animal, which being ſuppoſed to be not only agil but light, leſſened the weight of the body they enlivened; and flame being conceived to be the lighteſt among bodies here below, it is not improbable, that ſome will aſcribe the phænomenon to the levity of the flame, which by being diffuſed through the body of an animal, and vivifying it, deſerves the name of vital. But I would not adviſe any to rely on this conceit, till they are duly ſatisfied of the truth of the matter of fact; which becauſe I have not yet found, that any has endeavoured to try, I ſhall on this occaſion give you the following tranſcript of one of my notes about ſtatical experiments.

A mouſe, weighing about three drachms and a half, being put in one of the ſcales of a very nice balance, was counterpoiſed together with a ſtring, that was tied about his neck like a nooſe, and after a while, by drawing the ends of it, was there ſtrangled. As ſoon as we judged him quite dead, we weighed him again, and though nothing was ſeen to fall from him;

yet, contrary to the received tradition, that bodies are much heavier dead than alive, we found the weight to have loſt about $\frac{1}{6}$ of a grain; which probably proceeded from the avolation of divers ſubtile particles upon his violent and convulſive ſtruglings with death. But this was no more than an experiment of this kind, made ſome years ago, induced me to expect and foretel.

Afterwards in a larger balance, but a very good one, purpoſely made for nice experiments, we took a very young catlin, of between ten and eleven ounces in weight, and cauſed him to be ſtrangled on the ſame ſcale, wherein he had been put. But he could not be diſpatched ſo ſoon as an ordinary full grown animal; ſo that by that time he was quite dead, we found him not only not to be grown heavier, but lighter by four grains; which did not much ſurprize us, having elſewhere noted the life of ſo very young creatures of that kind not to be eaſily deſtroyed for want of reſpiration. And I remember, that, for trial's ſake, another catlin of the ſame litter with this I have mentioned, being included in a receiver, wherein another animal of that ſize might probably have been diſpatched in two or three minutes by the pumping out of air, was kept there ſomewhat above a quarter of an hour before he appeared to be quite dead.

ADVERTISEMENT.

These two following attempts falling into the hands of the author after the preceding experiments were printed, it was thought fit to annex them here for the affinity of the ſubject.

An ATTEMPT to produce LIVING CREATURES in *Vacuo Boyliano*.

In reference to the opinion of thoſe naturaliſts, that hold the ſeeds of living creatures to be animated, and eſpecially to the hypotheſis of thoſe learned men, that aſſert the flamma vitalis lately mentioned; it may be an enquiry of moment, whether or no in the ſeminal principles, or rudiments of animals, the manifeſt operations of life may be excited without the concurrence of the air, whoſe intereſt in the production and conſervation of flame may be gathered from the foregoing experiments. For, it ſeems likely to prove no inconſiderable diſcovery in reference to the lately mentioned hypotheſis, if it be found, that the principle of life in ſeminal rudiments needs, as well as other flames, the concurrence of the air to actuate it.

I thought fit, therefore, notwithſtanding the great and almoſt inſuperable difficulties, which it was eaſy enough for me to foreſee I ſhould meet with, to attempt the hatching of eggs in our vacuum: but though I made ſome unſucceſsful trials of this kind, in order to a diſcovery about reſpiration, (not here to ſpeak of the attempts I made about the animation of putrid matter,) yet leaving the mention of them to its proper place, I ſhall only take notice in this, what directly concerns the preſent inquiry. Conſidering then, that pregnant females cannot be made to live and bring forth young in our exhauſted receiver, and that the eggs of birds, and ſuch greater animals, do, in this colder climate of ours, require to be hatched by the incubation of the females, or other birds; I thought the fitteſt ſubjects I could both make choice of, and procure for the deſigned experiments, would be the eggs of ſilk-worms. For, having many years ſince tried ſeveral things about thoſe inſects, and among others found, that their eggs would be hatched, not only by the heat of one's body, (though that be the uſual way,) bur by the warmth of the ſun even here in *England*, if they be kept till the ſpring be far enough advanced: remembering this, I ſay, I got a good number of ſilk-worms eggs; and having cauſed three conveniently ſhaped, but very ſmall receivers, to be purpoſely made, that differed very little (and that accidentally) either in ſize or figure, we conveyed into each of them, together with a ſmall ſtock of mulberry-leaves, ſuch a number of eggs, as we thought ſufficient to make one morally ſecure, that at leaſt ſome of them were prolifick: this done, we carefully exhauſted one of them, and ſecured it againſt the return of the air; the two others we left full of air: but having left in one a little hole for the air to come in and get out at, we ſtopped the other ſo cloſe, as to hinder all intercourſe between the included air and the external. All things being thus prepared, we expoſed the receivers to a ſouth-window, where they might lie quiet, and where I either came, or ſent to look on them from time to time; the ſpring being then ſo far advanced, that I ſuppoſed the heat of the ſun would be of itſelf ſufficient to hatch them in no long time.

As to the ſucceſs of this trial, my not being able to find any regiſter of the particular phænomena, that occurred, keeps me from venturing to relate it very circumſtantially; but this I remember in general, that both I and others took notice, that in the unexhauſted receivers there were divers eggs hatched into little inſects, that perforated their ſhells, and crept out of them; though afterwards, for want of change of food, or air, or both, few or none of them proved long-lived. But though the eggs in theſe receivers began to afford us little animals in a few days; yet the eggs in the exhauſted receiver did not, in many more, afford us any. And though I will not venture to ſay, how long preciſely we kept them in the ſame window, after ſome of the above-mentioned eggs were hatched; yet (if I much miſtake not) it was, from firſt to laſt, about three or four times as long; and I remember, we kept them till it was thought to no purpoſe to wait any longer, and agreed in imputing the not hatching of the eggs by the ſo long continued action of the ſun to the abſence of the air.

WHAT other phænomena occured to us in making this experiment, and another not unproſperous one upon the eggs of flies, you may expect, when I can light on my notes about them, or have my memory refreſhed by thoſe, that aſſiſted at the making of them.

(*Annexed, by way of* APPENDIX, *to the former* PAPERS.)

An ATTEMPT made upon GNATS in our Vacuum.

I Elſewhere mention, that it has been obſerved by a couple of our virtuoſi (whom I there name) and ſeveral times by me, that here in *England* multitudes of gnats are generated of little animals, that live, for a part of the ſummer, like fiſhes in the water; and conſidering, that by theſe a very unuſual paſſage is made from ſwimming to flying animals, I thought them very fit ſubjects, whereon to make the following experiment.

[PARTLY to try, whether at leaſt an animal already living and moving in our vacuum may be able to attain the perfection due to it, according to the courſe of nature; and partly to examine, whether, in caſe he ſhould attain it, at leaſt the lighter ſort of winged inſects may be able to fly in that place; and partly to diſcover, whether an animal, that had long lived in our vacuum, would, when turned to a fly, be able to continue alive without reſpiration, he had never been accuſtomed to, in its priſtine form or ſtate; we took divers of thoſe little ſwimming creatures, which in autumn, eſpecially towards the end of it, are wont to be turned into gnats, and having put a convenient number of them together in a fit quantity of rain-water, wherein they had been found and kept, into a ſmall receiver, the air was pumped out, and the veſſel ſecured againſt its return, and then ſet aſide in a place, where I could obſerve, that the day after ſome of theſe little animals were yet alive and ſwimming to and fro, not without minute bubbles adhering to them; but at the end of a day or two after that, I could not perceive any of them to ſurvive their dead companions, nor did any of them recover, when freſh air was let in upon them. But though this experiment were the beſt I was then able to make, yet I reſolved, if God ſhould vouchſafe me life and health, to repeat it the enſuing autumn; that, wherein it was made, proving ſo cold and unſeaſonable, that a number of theſe little creatures, put up with water into another ſmall receiver, died all within a few days, though none of the air was exhauſted; and ſeveral, that I kept in an ordinary glaſs, that was divers times unſtopped to give them freſh air, did yet periſh at no ordinary rate. And I confeſs (as unkind as this trouble of mine may ſeem to the air,) that the failing of this and ſome other experiments of producing animals in our exhauſted receivers was the more unwelcome to me, becauſe I had, and have ſtill a great deſire to ſee, if it be poſſible, what would happen to animals, which had been produced in a place free from the preſſure of the atmoſphere, as if they had been born in *Epicurus*'s imaginary intermundane ſpaces, upon their coming to be ſuddenly ſurrounded with our heavy air, and having their tenderly framed bodies expoſed to its immediate preſſure.]

NEW EXPERIMENTS
ABOUT
EXPLOSIONS

(Annexed, by way of APPENDIX, to the former PAPERS.)

FORASMUCH as ſome of the learned men, that are the grand aſſertors of the flamma vitalis (whoſe opinion occaſioned my preſenting you the foregoing experiments) do alſo, with the juſtly famous Dr. *Willis*, explicate many of the motions of animals, eſpecially thoſe performed in the muſcles, by the exploſions made of certain juices or fluid ſubſtances of the body, when they come to mingle with each other: and foraſmuch alſo as I do not remember, I have heard the maintainers of this hypotheſis inſiſt on other inſtances in favour of it, than the going off of gunpowder; which being not a liquor, but a conſiſtent and brittle body, and requiring for its exploſion either actual fire, or a far intenſer heat, than can be ſuppoſed natural in men, and other animals; I was induced to ſuſpect, they were not yet provided with better examples; and therefore I preſume, it will be looked upon, as a thing neither uſeleſs, nor altogether impertinent, if, without offering to determine any thing about the truth of the opinion, I ſupply the embracers of it with two or three examples of exploſions made by the bare mingling of liquors; which I ſhall borrow from the elſewhere-mentioned notes, that I drew up ſome years ago, in order to the improvement of ſome parts of phyſick.

EXPERIMENT I.

Of an exploſion made with the ſpirits of nitre and wine.

WE took ſpirit of nitre, ſo ſtrong, that the fumes made the upper part of the glaſs it was kept in, always reddiſh; and having put but one ounce of it into a bolt-head, with a long neck, capable to contain, as we gueſſed, twelve or ſixteen times as much, we cauſed an equal weight of alcohol, or highly rectified ſpirit of wine, to be taken, and a little of it being put to the ſpirit of nitre, it preſently made ſo ſtrong and quick an expanſion or exploſion, that ſome of it flew out of the glaſs, and hit againſt the cieling of the room, (where I ſaw the mark of it) and falling upon his face, that held the glaſs, made him think (as he told me) that fire had fallen upon it, and made him run down the ſtairs like a mad-man, to quench the heat at the pump. Wherefore, bidding the laborant proceed more warily, I ordered him to put into the bolt-head but part of a ſpoonful of ſpirit of wine at a time; and yet, at each of a pretty many affuſions, that I ſtayed to ſee the effect of, there would be a great noiſe, as of an ebullition, though no ſtore of froth produced, and accompanied with ſo great a heat, that I could not hold the glaſs in my hand; and immediately there would iſſue out a copious and red ſmoke; to which when I cauſed a little candle to be held, though at near half a foot diſtance from the top of the bolt-head, it would preſently take fire, and burn at the top of the bolt-head, like a flame at the upper end of a candle, till I cauſed it to be blown out, that freſh ſpirit of wine might be poured in; which, when it was all mingled with the other liquor, the heat and conflict ceaſed.

DIVERS other phænomena relating to this experiment, (by which I intended to make out more things than one) belong not to our preſent ſubject, and are already ſet down in other papers. But yet it will be pertinent to ſhew in this place, that the noiſe and ebullition produced in this mixture is not unaccompanied with a briſkly expanſive, or an exploſive motion. To make then an experiment to this purpoſe, and yet avoid the danger, whereto the making of it unwarily might expoſe both the veſſels and us; we put an ounce of ſuch ſtrong ſpirit of nitre, as is abovementioned, into a moderately large bolt-head furniſhed with a proportionable ſtem, over the orifice of which we ſtrongly tied the neck of a thin bladder, out of which moſt part of the air had been expreſſed, and into which we had conveyed a ſmall vial, with a little highly rectified ſpirit of wine: then this vial, that before was cloſed with a cork, being unſtopped, without untying or taking off the bladder, a ſmall quantity, by gueſs not a quarter of a ſpoonful, of the alcohol of wine, was made to run down into the ſpirit of nitre, where it preſently produced a great heat and commotion, and blew up the bladder, as far as it would well ſtretch, filling alſo the ſtem and cavity of the glaſs with very red fumes, which preſently after forced their way into the open air, in which they continued, for a good while, to aſcend in the form of an orange-coloured ſmoke.

EXPERIMENT II.

Of an exploſion made with oil of vitriol and oil of turpentine.

IF I had at hand the papers, you have divers times heard me ſpeak of, about heat, I could give you the particulars of ſome trials about exploſion, that perhaps you would think more pertinent than deſpicable: but, for want of thoſe papers, I muſt content myſelf to tell you in general, that I remember, that I have, more than once, taken ſtrong oil of vitriol, and common oil of turpentine, and warily mixed them in a certain proportion, by ſhaking them very well together; and that thereupon enſued (what I had reaſon to look for) ſo furious an agitation of the minute parts of the mixture, and ſo vehement or ſudden expanſion or exploſion, as did not only ſeem ſtrange to the ſpectators, but would have proved dangerous too, if I had not taken care before-hand, that the trials ſhould be made in a place, where there was room enough; and that even the operator, that ſhook the veſſel, ſhould ſtand at a convenient diſtance from the mixture.

EXPERIMENT III.

About an exploſion made by two bodies actually cold.

I Remember not, that I found the aſſertors of exploſions in animals to have taken notice of a difficulty, which to me ſeems not uneaſy to be obſerved, and yet very worthy to be cleared. For it is known, that fiſhes, and thoſe eſpecially of the vaſter ſort, can move and act in the waters with a ſtupendious force; and yet it is affirmed by thoſe, that pretend to know it, that the blood of moſt fiſhes is ſtill actually cold: and I remember, I found the blood even of thoſe I diſſected alive to be ſo. From whence moſt men would argue, that even in the vaſt ſea-monſters there can be made no exploſions, theſe being ſtill effected by or accompanied with an intenſe degree of heat.

It were incongruous to my deſign, to examine this difficulty, as it directly regards the exploſions, ſaid to be made in animals: but ſpeaking of exploſions in general, perhaps I might do the favourers of vital ones (if I may ſo term them) no unacceptable piece of ſervice, by experimentally ſhewing, that it is not impoſſible, though it ſeem very unlikely, that exploſions ſhould be made upon the mixture of bodies, which, whilſt they ſeem to put one another into a ſtate of efferveſcence, are really cold, nay, colder than before their being mingled. Of theſe odd kind of mixtures, I remember I have in another * paper ſet down ſome trials, that I made to other purpoſes, as well with two liquors, as with a liquor and a ſolid body; which later ſort I there mention my having made by an improvement of an experiment of the excellent Florentine virtuoſi. And among thoſe trials I find one, whoſe pertinency to the matter in hand invites me to annex as much of it, as is proper in this place.

There were put two ounces of powdered ſal armoniac into a pretty large glaſs-tube, hermetically ſealed at one end; into the ſame a ſlender glaſs-pipe, furniſhed with two ounces of oil of vitriol, was ſo put, that, when we pleaſed, we could make the liquor run out into the larger tube, which, after theſe things were done, was cloſed exactly, ſo that nothing might get in or out. My deſign was, that this inſtrument ſhould be ſo warily inverted, that the operator might get out of the way, and the oil of vitriol, falling ſlowly upon the ſal armoniac, ſhould, without producing any heat, produce an exploſion not dangerous to the by-ſtanders. But whilſt I was withdrawn to a neighbouring place to write a letter, the operator not ſtaying for particular directions, raſhly inverted the inſtrument, without taking care to get away: whence it happened, that as ſoon as ever the contained liquor, being too plentifully poured out, came to work on the ſal armoniac, wherewith it is wont to produce cold, there was ſo ſurpriſing and vehement an expanſion or exploſion made, that with a great noiſe, (which, as the laborant affirmed, much exceeded the report of a piſtol,) the glaſſes were broken into a multitude of pieces, many of which I ſaw preſently after, and a pretty deal of the mixture was thrown up with violence againſt the operator's doublet and his hat, which it ſtruck off, and his face; eſpecially about his eyes, where immediately were produced extreamly painful tumors, which might alſo have been very dangerous, had I not come timely in, and (to add that upon the by) made him forthwith diſſolve ſome ſaccharum Saturni in fair water, and with a ſoft ſpunge keep it conſtantly moiſtened by very frequently renewed applications of the liquor: by God's bleſſing upon which means, within an hour or two, the pain, that had been ſo raging, was taken away, and the fretting oil of vitriol was kept from ſo much as breaking the ſkin of the tumors, that it had made.

The firſt part of the relation of this trial might have been omitted, or at leaſt ſhortened, unleſs I had deſigned to communicate unto you a way of doing what I do not know to have been attempted by others, namely to put bodies together, when and by what degrees one pleaſes, after the glaſs, that contains them, has been hermetically ſealed up; which mechanical contrivance, eſpecially as it may be varied, may be, as I have tried, uſefully applied to more purpoſes, than it were proper here to take notice of.

But to conclude with a word or two touching the foregoing experiment; I ſhall only add, that another time we made a like trial a ſafer way, by tying a bladder ſo to the top of a bolt-head, into which we had before-hand put the ſal armoniac, that, by warily moving the bladder, whence the air had been expreſſed, we could make ſome of the ſal armoniac, we had lodged in its folds, to fall upon the liquor, with which it preſently made an exploſive mixture, that quickly blew up the bladder.

But theſe, Sir, are bare conjectures, left to be, after a farther diſcuſſion, (if you think them worthy of it) determined by you, to whom as theſe papers are addreſſed, ſo they are alſo ſubmitted by the writer of them.

I am,

SIR,

Yours, &c.

AN

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* About the production or extrication of air.

AN
HYDROSTATICAL DISCOURSE,

OCCASIONED BY

The OBJECTIONS of the Learned Dr. *HENRY MORE*,

AGAINST SOME

EXPLICATIONS of NEW EXPERIMENTS made by Mr. *BOYLE*;

AND NOW

Publiſhed by way of PREFACE to the three enſuing TRACTS.

To the READER.

WHEN I determined to write this polemical diſcourſe, I did not forget, that when I firſt ventured ſome of my trifles abroad into the world, my friends obtained from me a promiſe, that after I ſhould have anſwered the two firſt, that ſhould expreſly write againſt me (which happened to be the learned *Linus* and Mr. *Hobbes*,) to ſhew, that I was not altogether unacquainted with a way of defending truths, I would afterwards write no book in anſwer to any, that ſhould come forth againſt mine; for, not only my friends, but I, thought it enough for a perſon, that never was a gown-man, to communicate freely his thoughts and experiments to the curious, without deſpairing, that thoſe things, that ſhould be evidently true, would be able to make their own way, and ſuch as were very probable, would meet with patrons and defenders, in ſo inquiſitive an age as ours. And indeed I do not find, that either upon the account of my writings, or ingenious men's opinion of them, I have had much cauſe to repent the keeping of my promiſe, notwithſtanding the writings, that have impugned ſome of mine, but without much prejudice, that I know of, either to the propoſed truths, or the propoſer of them. And therefore I ſhould not at all have entered upon a defence of what is attacked of mine by the learned Dr. *More*, if I had not ſuppoſed, that it would not require a book, but might be diſpatched in a preface: for having by me ſome little tracts, that ſhould, though the doctor had never engaged me, have been imparted to the publick, and obſerving, that the new experiments contained in one or other of them would by an eaſy application be brought to confirm my formerly delivered explications of other phænomena, and enervate the doctor's objections againſt them; I thought I might, without long troubling the reader, or my ſelf, defend what I looked upon as truth, by anſwering ſome incidental paſſages of the doctor's diſcourſe, and referring the reader, for the main points in controverſy between us, to thoſe experiments of the following tracts, which clearly contain the grounds of deciding them. But yet this conſideration would not perhaps have engaged me to write the following preface, if the objections I was to anſwer had not been, by a perſon of ſo much fame, propoſed with ſo much confidence; and though with very great civility to me, yet with ſuch endeavours to make my opinions appear not only untrue, but irrational and abſurd, that I feared his diſcourſe, if unanſwered, might paſs for unanſwerable, eſpecially among thoſe learned men, who, not being verſed in hydroſtaticks, would be apt to take his authority and his confidence for cogent arguments; and who (not obſerving how liberal ſome men are of titles to the arguments, that pleaſe them) would make a ſcruple of thinking, that what is with great ſolemnity delivered for a demonſtration in a book of metaphyſicks, can be other than a metaphyſical demonſtration. The care therefore, that what I judge to be true, ſhould not be made to paſs for abſurd, which is a degree beyond what is merely erroneous, by being ſo ſeverely handled by a perſon of Dr. *More*'s fame and learning, induced me to begin the following papers; which ſhould have been ſhorter than now it is, but that I was perſuaded to lengthen it beyond what was either neceſſary or deſigned, that I might, by the addition of ſome few thoughts and experiments on the occaſions, that were ſuggeſted to me, endeavour to clear up and confirm ſome hydroſtatical truths, that, I fear, are but by very few either aſſented to, or perhaps ſo much as underſtood, and ſo might make the reader amends for the trouble I was forced to give him in a diſpute, which I apprehended he might otherwiſe think himſelf but little concerned in. And he will, I hope, eaſily diſcern, that I have no mind to burthen him in my preface with things not pertinent to the ſcope of it, if he take notice, that both for his ſake and the learned doctor's, (whoſe civility I would not leave unanſwered) I have reſtrained my ſelf to the defenſive part, forbearing to attack any thing in his Enchiridium Metaphyſicum, ſave the two chapters, wherein I was particularly invaded.

But though I have declined the delivering my opinion of the doctor's book, yet I dare not forbear owning my not being ſatisfied with that part of his preface, which falls foul upon Monſieur *des Cartes*, and his philoſophy. For though I have often wiſhed, that learned gentleman had aſcribed to the divine author of nature a more particular and immediate efficiency and guidance, in contriving the parts of the univerſal matter into that great engine we call the world; and though I am ſtill of opinion, that he might have aſcribed more than he has to the ſupreme cauſe, in the firſt origin and production of things corporeal, without the leaſt injury to truth, and without much, if any, prejudice to his own philoſophy; and though not confining myſelf to any ſect, I do not profeſs myſelf to be of the Carteſian: yet I cannot but have too much value for ſo great a wit as the founder of it, and too good an opinion of his ſincerity in aſſerting the exiſtence of a deity, to approve ſo ſevere a cenſure, as the doctor is pleaſed to give of him. For I have long thought, that in tenets about religion, though it be very juſt to charge the ill conſequences of men's opinions upon the opinions themſelves; yet it is not juſt, or at leaſt not charitable, to charge ſuch conſequences upon the perſons, if we have no pregnant cauſe to think they diſcern them, though they diſclaim them. And ſince men have uſually the fondneſs of fathers for the offſpring of their own brains, I ſee not, why *Carteſius* himſelf may not have overlooked the bad inferences, that may be drawn from his principles, (if indeed they afford any ſuch) ſince divers learned, and not a few pious perſons, and profeſſed divines of differing churches, have ſo little perceived, that the things objected are conſequent to ſuch principles, that they not only abſolve them as harmleſs, but extol them as friendly and advantageous to natural religion. And I ſee not, why ſo great and radiant a truth, as that of the exiſtence of a God, that has been acknowledged by ſo many meer philoſophers, might not as well impreſs itſelf on ſo capable an intellect, as that of Monſieur *des Cartes*; or that ſo piercing a wit may not really believe he had found out new mediums to demonſtrate it by. And ſince the learned *Gaſſendus*, though an eccleſiaſtick, had been able, as well ſafely, as largely to publiſh the irreligious philoſophy of *Epicurus* himſelf; it ſeems not likely, that ſo dexterous a wit as that of Monſieur *des Cartes* could not have propoſed his notions about the mechanical philoſophy, without taking ſo mean a courſe to ſhelter himſelf from danger, as in the moſt important points, that can fall under man's conſideration, to labour with great ſkill and induſtry to deceive abundance of ingenious men, many of whom appeared to be lovers of truth, and divers of them lovers of him alſo. And I am the more averſe from ſo harſh an opinion of a gentleman, whoſe way of writing, even in his private letters, tempts me very little to it, becauſe I cannot think him an atheiſt, and an hypocrite, without thinking him (what Dr. *More* has too much celebrated him) to call him a weak head, and almoſt as bad a philoſopher, as a man. For, as far as I underſtand his principles, ſome of the moſt important points of his philoſophy (which, if it were needful, I could name) are interwoven with the truth of the exiſtence of a God, or do at leaſt ſuppoſe it, and are not demonſtrable without it. But I muſt not prevent the Carteſians, who, now he cannot do it for himſelf, I doubt not will apologize for their maſter; though looking upon him as a great benefactor to, though not the firſt founder of the mechanical philoſophy, I could not conſent, by a total ſilence upon ſuch an occaſion, to become any way acceſſary to the blemiſhing of his memory.

AN HYDROSTATICAL DISCOURSE, &c.

SIR,

UPON the advertiſement you gave me yeſternight, that I was particularly concerned in the learned Dr. *More*'s Enchiridium Metaphyſicum, I this day turned over the leaves of one, which I have freſhly received from the reverend author himſelf: and being aſſiſted by the ſeries of the titles, I quickly lighted on that part of the book, whoſe ſubject made me expect to find myſelf queſtioned there, as I preſently found I was. For though that civil adverſary is pleaſed to omit my name, and, the farther to diſguiſe it, employs, inſtead of it, a great and unmerited encomium; yet by the book he cites, and the experiments, againſt which he argues, it is very eaſily diſcoverable, that his objections are meant againſt me, who ſee yet no cauſe at all to be ſcrupulous to own my name, and the doctrine delivered in the paſſages heis pleaſed to oppoſe.

I doubt not but you will preſently deſire to know, what I think of this much expected work; but when I have told you, that I have gained time to peruſe only (and that but curſorily) the twelfth and thirteenth chapters, you will, I queſtion not, excuſe a perſon, that does exceedingly want health, and yet wants not almoſt continual avocations, if I now content myſelf to give you my thoughts of that part of the newly-mentioned chapters, which properly relates to me; I ſay, that part of the chapters, becauſe there are others, wherein I need not intereſt myſelf. For, to omit other paragraphs, the doctor has, in the former part of the twelfth chapter, thought fit to ſeparate from my explication of the phænomena in queſtion betwixt us that of the learned *Henricus Regius*; and the latter part of the ſame chapter he employs in an ingenious diſpute againſt thoſe, that would have the aerial particles act with perception and deſign, and (as he ſpeaks) *pro re nata*; which opinion you will Sect. 16, 17. eaſily believe I neither was of, nor am like to adopt.

IT remains then, that ſetting aſide thoſe diſcourſes of the twelfth chapter, wherein it is needleſs, that I ſhould make myſelf a party; I proceed to conſider thoſe paragraphs, which will be eaſily gueſſed to be levelled at my explications, and by which I muſt confeſs, I cannot at all be yet convinced of their being falſe ones. But in doing this, I ſhall not only, in compliance with my preſent haſte, but alſo to expreſs my reſpect to the learned doctor, forbear to ſay any more, than what I ſhall judge requiſite to anſwer the objections, that directly concern my own explications, without meddling, by way of retaliation, with his hypotheſes or opinions, or endeavouring to ſet any paſſages of his writings at variance among themſelves, or to take thoſe little advantages, which are uſually ſought for by diſputants.

I ſhall not trouble you, nor tire myſelf with any ſchemes, ſince the doctor has taken the pains to inſert thoſe, that are neceſſary for his purpoſe, in his book, and I have not my own at hand. Wherefore, not doubting, that you have by you thoſe books of mine he refers to, and ſuppoſing, that you will, whilſt you are reading, have alſo this book, with the inſerted ſchemes before your eyes, I ſhall not ſpend time on any further preamble, but immediately enter upon the conſideration of the objections I am to anſwer.

The FIRST SECTION.

CHAP. I.

THE firſt explication of mine, that the learned doctor animadverts upon in his twelfth chapter is that, which I give in the thirty-third of my phyſico-mechanical experiments, touching the ſpring and weight of the air; where I relate, that the ſucker in the air-pump of our engine, having been forcibly depreſſed to the lower part of the braſs cylinder, which yet was carefully cloſed at the top, ſo that the cavity of the cylinder was empty of air; this ſucker, I ſay, would, in this caſe, appear ſpontaneouſly to remount towards the top of the cylinder, though it were clogged with a hundred pound weight to hinder its aſcent. Which phænomenon I aſcribed to this, that the ſucker being, by the withdrawing of the air in the cylinder, freed from the wonted force of the ſpringy air, that endeavoured to depreſs the internal part of it, was not enabled, by the appendant weight, to reſiſt the preſſure of an atmoſpherical cylinder equal in diameter to it, which, preſſing againſt its lower or external ſurface, endeavoured to impel it up.

Now the doctor having, in the two firſt paragraphs, made a deſcription of my engine, (which I ſhall now paſs over) does in the third teach us, that the corporeal cauſe, if there be any, of the aſcent of the ſucker, muſt be either in the ſucker itſelf, or in the almoſt exhauſted cavity of the cylinder, or, laſtly, in the external air. Which premiſed, he does in the ſame third ſection, and in the fourth, endeavour to prove at large, that the cauſe is to be derived neither from the one, nor from the other of the two firſt. And therefore I, that maintain neither of the opinions he diſputes againſt, ſhall leave thoſe paragraphs of his untouched. Nor ſhall I meddle with the fifth, ſixth and ſeventh, where he argues againſt the explications of ſome, that would ſolve the phænomenon upon ſome Carteſian grounds, and as well amply, as particularly againſt the ſolution, that he ſuppoſes would be given of it, congruouſly to his own ſentiments by the learned *Regius*. Theſe diſcourſes, I ſay, of the doctor's I leave untouched; becauſe it is at length in the eighth paragraph, that he impugns that ſolution of the phænomenon, which he aſcribes to me, whoſe opinion he firſt delivers, though not juſt in the terms I would expreſs it myſelf; yet I dare ſay very ſincerely, and ſo near my ſenſe, that I ſhall forthwith paſs from the eighth ſection to the beginning of the ninth, where he begins to propoſe his objections, which he is pleaſed to uſher in with a complement to me, that I ſhould be very vain, if I looked upon as any thing more than a complement.

To his firſt objection, propoſed in theſe
Pag. 139. words, *Primò enim, ſi hæc ſolutio verè mechanica ſit, quæ tandem cauſa verè mechanica aſſignari poteſt gravitationis ſingularum particularum, totiúſque atmoſphæræ in ſuis locis? nam quod materiam ſubtilem attinet*, &c. I anſwer, that I did not in that book intend to write a whole ſyſtem, or ſo much as the elements of natural philoſophy; but having ſufficiently proved, that the air, we live in, is not devoid of weight, and is endowed with an elaſtical power or ſpringineſs, I endeavoured, by thoſe two principles, to explain the phænomena exhibited in our engine, and particularly that now under debate, without recourſe to a fuga vacui, or the anima mundi, or any ſuch unphyſical principle. And ſince ſuch kind of explications have been of late generally called mechanical, in reſpect of their being grounded upon the laws of the mechanicks; I, that do not uſe to contend about names, ſuffer them quietly to be ſo: and to entitle my now examined explication to be mechanical, as far as I pretend, and in the uſual ſenſe of that expreſſion, I am not obliged to treat of the cauſe of gravity in general; ſince many propoſitions of *Archimedes*, *Stevinus*, and thoſe others that have written of ſtaticks, are confeſſed to be mathematically or mechanically demonſtrated, though thoſe authors do not take upon them to aſſign the true cauſe of gravity, but take it for granted, as a thing univerſally acknowledged, that there is ſuch a quality in the bodies they treat of. And if in each of the ſcales of an ordinary and juſt balance a pound weight, for inſtance, be put; he, that ſhall ſay, that the ſcales hang ſtill in æquilibrium, becauſe the equal weights counterpoiſe one another; and in caſe an ounce be put into one of the ſcales, and not into the oppoſite, he that ſhall ſay, that the loaded ſcale is depreſſed, becauſe it is urged by a greater weight than the other; will be thought to have given a mechanical explication of the æquilibrium of the ſcales, and their loſing it, though he cannot give a true cauſe, why either of thoſe ſcales tends towards the center of the earth. Since then the aſſigning of the true cauſe of gravity is not required in the ſtaticks themſelves, though one of the principal and moſt known of the mechanical diſciplines; why may not other propoſitions and accounts, that ſuppoſe gravity in the air, (nay prove it, though not *à priori*) be looked on as mechanical?

CHAP. II.

THE next thing the doctor oppoſes to my explication, is a reſolute denial, that there is any ſuch gravitation, as I pretend, of bodies, or their particles, in their proper places. But becauſe, for the proof of his negation, he refers us to the next chapter, we ſhall hereafter have a fitter place than this to conſider it in.

THIRDLY, he tells us, we may juſtly doubt of the equal diffuſion of the ſpringy power, or the preſſure of the air every way. In what ſenſe, in ſome caſes, I admit of a ſmall inæquality between the preſſure of fluids againſt differing parts of a ſurrounded body, I have * elſewhere declared, and need not here diſcourſe of; ſince in the caſe before us, and in the like, that preſſure is inconſiderable enough to be ſafely neglected. And whereas
our author thus argues, *Semotâ vi elaſticâ,* Pag. 139.
particulæ tamen atmoſphæræ deorſum tenderent. Eſt igitur depreſſio quædam deorſum præter vim elaſticam ipſi ſuperaddita; ſurſum non item, ſed elaſtica ſola, eſtque ſuppar ratio in preſſionibus tranſverſis & obliquis: I preſume, he did not ſufficiently conſider our hypotheſis and the nature of the preſſure of fluid bodies, that have weight: for water, to which no ſpringineſs is aſcribed, as there is to air, but which acts by its weight and fluidity, is able, upon the ſcore of thoſe qualities, to buoy up great ſhips, that the ebbing tide often leaves upon the ſtrand.

AND whereas the learned examiner propoſes
a fourth objection in theſe terms, *Quibus omni-* Pag. 139.
bus addas, difficile eſſe intellectu, ſi unius cylindri atmoſphæræ pondus æqualis diametri cum embolo reflectione in fundum emboli derivetur, cur non quinque alii cylindri aeris, qui circumſtant embolum, in ejus fundum eodem modo ſimul agere poſſunt, ita ut vis ſurſum impellens embolum ſextuplo major ſit, quàm hactenus ab hujus opinionis fautoribus exiſtimata eſt. Quod ſi ſit, tunc certè, ſiquo artificio fieri poſſit ut

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* See the Hydroſtatical Paradoxes, eſpecially Parad. 7.

ut unius ſolius cylindri aerei in embolum admitteretur, reliquorum quinque excluſa, & pari tamen facilitate embolus aſcenderet; manifeſtum indicium eſſet, ut unum quidem cylindrum atmoſphæræ agere in fundum emboli, ſed totam hypotheſin ingenioſam tantummodo eſſe fictionem. I preſume, hydroſtaticians will think this might have been ſpared. For they will tell him, that there can no more of a fluid preſs directly upward againſt the cylindrical orifice of a body immerſed in that fluid, than a cylinder of that fluid of the ſame diameter with the orifice, the lateral preſſures bearing againſt the lateral parts of the cylinder. And therefore if you invert, for inſtance, a pipe open at both ends, and filled to a height with oil, with common water; the oil, that is kept up by the preſſure of the water upwards, will keep at the ſame height as to ſenſe, whether the veſſel, that contains the water, be broad or narrow, provided it be ſomewhat larger than the orifice of the pipe.

AND now, to invalidate yet further the precedent objections made by the doctor, I ſhall add, that it need not be thought incredible, that the atmoſphere by its weight, or the ſpring of the air compreſſed by that weight, ſhould be able to raiſe up four ſcore or an hundred pound, hanging at the ſucker; ſince I have * manifeſted two or three years ago, by a clear and cogent experiment, that a little air in a bladder will, by its mere ſpring, be able to heave up a weight of a hundred pound, and this without the help of any rarefaction by heat. By which experiment may be alſo confirmed, what I delivered a while ſince about the endeavour of the air, that is wont to be included in our braſs cylinder, by expanding it ſelf to thruſt away the ſucker (which, in regard of the ſtructure of the pump, it can do no otherwiſe than downwards,) with a depreſſing force, æquivalent to the preſſure upwards of the atmoſphere, againſt the external part of the ſame ſucker.

CHAP. III.

BUT I ſhall not inſiſt upon the foregoing objections, becauſe the learned doctor himſelf tells, that their attempts may ſeem to be but light ſkirmiſhes in compariſon of that, which follows. Whereunto I ſhall therefore apply my attention.

THIS grand objection our learned adverſary takes from the already often-mentioned aſcent of the ſucker clogged with a hundred pound weight; and recommends by this introduction. *Etenim ex ipſis phænomeni viſceribus robuſtiſſimum jam contra omnem mechanicam illius ſolutionem argumentum eruo, & quod non ſolum contra vim aeris elaſticam ſuprà dicto modo explicatam militat, ſed etiam contra Carteſianum illum aeris conatum nixumque, &c.* Which premiſed, the argument it ſelf is thus propoſed: *Eſt enim* (ſays he) *juxta hujus experimenti phænomenon, vis illa aeris elaſtica (nixuſque expanſorius) major multo, quàm quæ fieri poteſt a rerum naturâ, quámque quotidianis illis phænomenis congruit. Nam ſi nixus hic elaſticus tantam vim elaſticam haberet, ut plus centum pondo plumbum ſurſum poſſit propellere, omnes profectò rerum terreſtrium compages tantâ violentiâ comprimerentur, ut nullæ, niſi quæ admodum firmiter compactæ ſint, tantæ compreſſioni reſiſtere poſſent, quin refrigerentur, vel partium colliſione ita contererentur, ut brevi tempore perirent, &c.*

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THOUGH this objection be ſpecious enough, yet it preſents me with no difficulty, that I was not well aware of; as I preſume you will eaſily perceive by what you will meet with in the following papers, eſpecially that, which conſiſts of experiments and conſiderations about the differing preſſures of ſolids, weights, and ambient fluids. The nature of which preſſure and its æquality (as far as in our controverſy it is needful to be ſuppoſed) will, I hope, ſatisfy you of the invalidity of the propoſed objections; eſpecially ſince the doctrine it impugns, namely the weight and preſſure of the atmoſphere, is not a bare hypotheſis, but a truth made out by divers experiments, by which even profeſſed oppoſers of it have publickly acknowledged themſelves to be convinced.

CHAP. IV.

IN the next paragraph (which is the eleventh) the learned doctor adds a further objection, wherein he ſuppoſes, that there is laid upon a wooden ſcale, of the ſame diameter with the above-mentioned ſucker, a lump of butter of the ſame largeneſs with the ſcale. Whence he argues, that if our hypotheſis take place, the butter muſt be preſſed againſt by two cylinders of air, the one preſſing it upwards, the other downwards, and the preſſure of them both amounting to two hundred pounds. But, ſays he, the butter is not preſſed at all, as appears by this, that no ſerous humour is ſqueezed out of it towards the edges, not ſo much as in thoſe parts, that lie parallel to the horizon, whence the concluſion ſeems eaſy to be deduced.

BUT in the twelfth paragraph, the doctor himſelf propoſes a ſolution, which he might eaſily foreſee I would employ to invalidate his argument; namely, that the air preſſing, as well againſt the ſides of the butter, as againſt the top and bottom, hinders the maſs from horizontally extending itſelf. And whereas, by way of reply to this ſubterfuge, as it is called in the margent, he ſubjoines, *Cui reſpondeo, quòd tamen hoc nihil prohibet, quo minùs in omnes partes horizontales exprimatur humor ſeroſus & lacteus, ſi revera eſſet ulla hujuſmodi preſſura elaſtica, qualis fingitur:* the reply is eaſy, that the preſſure of the ambient air, which is a fluid more ſubtil than butter-milk, will as well hinder the ſtarting out of that liquor, as of the parts of the butter itſelf; as he will eaſily grant, that attentively conſiders the nature of the thing, and remembers how air keeps water from running out at the little holes of a gardener's watering-pot cloſed at the top. What the objector adds about

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* See Continuat. of New Exper. Phyſico-Mechan. Exper. 48.

about the extruſion of what he calls a ſubtiler element (ſuppoſed to be harboured in the butter) by the preſſure of the atmoſphere, in caſe it had any, I think it would not be difficult to anſwer, if we conſidered, that a great and undeniable preſſure, applied to water, does not ſenſibly condenſe it, or deprive it of its fluidity, becauſe of the groſsneſs and ſtrength of its parts. But the argument being but tranſiently mentioned by the author, and grounded upon a Carteſian ſuppoſition, that I never employed, I leave it to thoſe, that may think themſelves concerned (which I am not) to make a ſolemn anſwer to.

Pag. 143. AND whereas, our learned examiner ſuperadds, *Quod tametſi butyri maſſa in diſci lignei ſpeciem reducta, cujus margo centum vicibus areâ ſit minor, interque duas laminas ligneas ejuſdem formæ ac latitudinis poſita, filis ſuſpenderetur in aere tanquam in lance, ita ut preſſura aeris elaſtica, quâ ab infra, quâ deſuper, ducentis fere vicibus exceſſura ſit preſſionem in marginem butyri, butyrum tamen nihilo arctius comprimetur per vim aeris elaſticam, nec aliter hic afficietur quàm antea*: he ſeems not to have ſufficiently conſidered the laws of the hydroſtaticks, according to which, ſuppoſing the preſſure of the atmoſphere that he rejects, the butter ought not to be deprived of its ſhape. For the preſſure of the ambient air, being equal on all ſides, if we ſuppoſe the ſuperficies of the butter to be diſtinguiſhed into a multitude of little equal portions, each of theſe, whether they be ſituated horizontally, or on the edges, can be preſſed againſt but by an atmoſpherical pillar equal to its baſis; and the horizontal portions, if I may ſo call them, cannot be thruſt out of place, without there be at the ſame time ſqueezed out ſome of the lateral portions, which yet cannot be ſo diſplaced, becauſe they alſo are, with equal force, preſſed inwards by little aerial pillars, whoſe baſes are contiguous to them, and bear againſt them. Which anſwer, though of itſelf ſufficient, may be much confirmed by the inſtance, you will hereafter meet with, of a lump of butter, that kept its irregular ſhape, in ſpight of a great and manifeſt preſſure of the water, that ſurrounded it.

AND this anſwer may ſuffice to diſprove, what the doctor annexes in the beginning of the thirteenth paragraph, about the vaſt exceſs of preſſure, which the air exerciſes upon the flat and horizontal ſurfaces of the abovementioned lump of butter, in compariſon of the preſſure the marginal parts of its ſurface can be expoſed to. What he adds, and illuſtrates with a ſcheme, about the hand's being aſſiſted with the preſſure of the air, it concerns not me to anſwer. But whereas among the places, where the elaſtical power of the air is underſtood not to reach, he reckons a pail full of water, with a lump of butter put in it; he ſuppoſes that, which our hydroſtaticks will by no means allow, and which is diſproved by ſeveral, both of our former experiments, and by thoſe you will meet with in the following papers. By which it appears, that the preſſure of the atmoſphere is exerciſed, as indeed I do not ſee what ſhould hinder it from being, even upon bodies, that are quite immerſed under water; and by which, added to what has been hitherto diſcourſed in anſwer to the learned doctor's objections, you will eaſily judge, how deſervedly he ſhuts up the arguments, we have been examining, with this concluſion. *Adeo ut extra omnem controverſiam poſitum videatur, quòd nulla eſt ejuſmodi vis elaſtica in aere, qualem è doctis nonnulli ſupponunt, multoque minus tam fortis, ut centum librarum pondus ſuperet. Quod erat demonſtrandum.* Pag. 143.

CHAP. V.

BUT this is not all the doctor urges againſt me in this chapter; for in the fourteenth paragraph he ſeconds his former argument by another, drawn from this experiment of mine, that having taken two round marbles, whoſe ſurfaces, that were to be contiguous, were as well ground very flat, as carefully poliſhed; and having placed them one directly upon the other, they did in a horizontal poſture ſo firmly cohere, without the help of any glue, or viſcous body *, that the upper marble being pulled up, would take up the lower, though clogged with a weight of fourſcore and odd pounds.

THIS experiment, when I many years ago firſt publiſhed it, I referred to the action of the atmoſphere, which preſſing equally and ſtrongly againſt the ſurfaces of both the marbles, except where they were contiguous, the higher could not be drawn directly upwards from the lower (and conſequently muſt be followed by it) by a leſs force, than that, which was equivalent to the weight of as great a cylinder of the atmoſphere as leaned upon the upper marble.

THIS experiment thus explained, though it hath been judged a very favourable one to the hypotheſis, on whoſe behalf I alledged it, does yet to the juſtly famous doctor ſeem a very conſiderable argument againſt it, though for this judgment of his he urges only this reaſon, that if the force, with which the air preſſes the lower marble againſt the upper, be able to ſuſtain that marble, though clogged with the great weight above-mentioned, the ſame preſſure of air would much more eaſily ſupport a plate of wood brought to a true plain, and not loaded with any weight, if the wooden plate were ſubſtituted to the lower marble, and, inſtead of it, applied to the upper.

BUT ſince the experiment, as I propoſed it, did upon trial ſucceed very well, it had not been amiſs, if the learned examiner had conſidered it as it was really and ſucceſsfully made, and ſhewed, why the preſſure of the ambient air was not able to hinder the ſeparation of the marbles: and his needleſs ſubſtitution of a wooden plate, inſtead of the lower marble, eaſily ſuggeſts a ſuſpicion, that there may lie ſome fallacy, though not intended by him, in the variation he propoſes of the experiment. And he ſeems to have himſelf had thoughts of this kind, by taking notice, that it may be anſwered on our behalf, that a wooden plate cannot be ſo exactly applied to the upper marble, but that there will be a little air intercepted between it and the bottom of that ſtone. And though, having granted, that it may be ſo, he employs two pages to ſhew, that this intermediate air could not keep the preſſure of the atmoſphere from ſupporting the unclogged plate of wood, if it had been that preſſure, which, when there was no ſuch intermediate air, had ſuſtained the lower marble with all the appendant weight; yet I confeſs, his proofs ſeem not to me to be anſwerable to the aſſurance he uſes in ſpeaking of them. His examples taken from gunpowder and wind you will eaſily judge not to be very proper, where we are not conſidering a force, that acts by a ſudden and vaniſhing impetus, but a conſtant and equal preſſure. And as to his other inſtance, which is taken from five men, that thruſt againſt the ſixth (ſtanding with his back to a wall) who is but as ſtrong as any one of them; I anſwer, that neither is this example near enough of-kin to our caſe. For each of theſe five men is ſuppoſed to have an equal power of thruſting, proper to himſelf, and independent from all, or any of the other four. And the ſixth man is likewiſe ſuppoſed to reſiſt but by his own ſingle force, without having his power of re-acting increaſed by the force wherewith the others thruſt againſt him. But in our caſe the thing is quite otherwiſe; for ſuppoſing, that ſome aerial particles be ſo placed, that a ſolid body hinders them to recoil or expand themſelves, we are to conſider, that as the contiguous corpuſcles of air preſs againſt them, not by their own ſingle weight or preſſure, but as they tranſmit the action of all the other particles of the air, which by their weight or preſſure thruſt them on; ſo the aerial particles, contiguous to the ſolid body, reſiſt not barely by that force, which they would have if they were not compreſſed, but by vertue of the ſpringineſs they acquire upon the ſcore of the forcible inflection they ſuſtain from the action of the corpuſcles, that either mediately, or immediately, thruſt againſt them; and conſequently, in proportion to that external force, the elaſticity of theſe compreſſed particles will be increaſed, as we ſee, that a bow, or other ſpringy body, the more it is bent by an external force, the greater power it has to reſiſt further compreſſion. Upon which grounds it need to be no wonder, that a ſmall portion of air, being almoſt included in a ſolid body, and having for ſome (though but very little) time been expoſed to the outward air, ſhould be capable of reſiſting the preſſure of as much of the whole atmoſphere, as can come to preſs againſt it. For, this preſſure of the atmoſphere being continual, if the ſpringineſs of the aerial particles were not now great enough to reſiſt that preſſure, they muſt neceſſarily have been beforehand inflected or compreſſed by it, till the endeavours of the one, and the other were reduced to an equipollency. Of which I ſhall give you an inſtance in ſo obvious a body as a bubble at the top of water. For, though there be but a little air included in a very thin and tranſparent film of water, yet this little air is ſo well able to reſiſt the weight of all the atmoſphere, that can come to bear againſt it, that all the preſſure of it is not able to make the film ſhrink, or become wrinkled; which it would do, if the corpuſcles of the internal air were not reduced to a ſpringineſs, which makes its power of reſiſting equal to the endeavour of the external atmoſphere to compreſs it. And to let you ſee, that we may well conceive ſuch a ſpringineſs of the air included in the bubbles, I have elſewhere related, how, by barely withdrawing the preſſure of the ambient air from glaſs-bubbles, hermetically ſealed with air in them, not compreſſed beyond its uſual ſtate, the ſpring of the internal air would make the bubbles fly in pieces: and this will happen to ſtronger glaſſes than bubbles, as you will find in one of the former experiments *. And if we would illuſtrate what we are debating of by an example, it ſhould not be by conſidering, as the doctor does, the endeavour of five men againſt the ſixth, that hath his back to the wall; but that of five bladders full of air, piled up, and reſting upon a ſixth. For in this caſe, whatever force or power of preſſing we ſuppoſe in the incumbent bladders, they all bear jointly upon the lower, which continuing at a ſtand, muſt thereby be ſo compreſſed, as to be able to reſiſt their joint endeavours; as it is manifeſt, becauſe otherwiſe it would not continue in that ſtate, but be farther compreſſed; which is againſt the ſuppoſition.

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* See the Hiſtory of Fluidity and Firmneſs, p. 222. of the ſecond edition.

THIS notion about preſſure and reſiſtance I have the more particularly deduced, becauſe I found many modern naturaliſts, and even hydroſtaticians themſelves, to be great ſtrangers to it. For which reaſon, I ſhall add, that I have evinced it by purpoſely deviſed experiments in the continuation of the phyſico-mechanical experiments † about the air. Were it not for this, I ſhould perhaps have ſpared myſelf the labour of ſetting down theſe thoughts, as not neceſſary to the ſolution of the doctor's objections. For he admits a layer, or (as he aptly ſpeaks) an area of aerial particles to be interpoſed between the upper marble and the wooden plate; and therefore the flatneſs and ſtiffneſs of thoſe two bodies muſt keep them from an immediate contact, as well at the edges, as by the help of the ſame area they do elſewhere; and conſequently, that interpoſed air may communicate with the ambient air. From whence the laws of the hydroſtaticks (which I have elſewhere ſhewn) will allow me to conclude, that the weight of the atmoſphere endeavours to depreſs the upper ſurface of the wooden plate; and ſo what the examiner urges of the inconſiderable reſiſtance, that the few aerial particles, interpoſed between the flat bodies, can make to the great preſſure of the column of air, that thruſts the wooden againſt the marble plate, would not conclude, though our former anſwer could not have been made; ſince the reſiſtance, made by the interpoſed aerial particles to the preſſure upwards of the atmoſphere, is not, in our preſent ſuppoſition, made by thoſe particles alone, but by the weight of the lateral and ſuperior part of the atmoſphere, exerciſed by the intervention of theſe particles. Which being ſo, what the learned doctor adds, that the weight of the wooden plate itſelf is here of no conſideration, muſt needs be a miſtake. For the two equal atmoſpherical preſſures, the one againſt the upper ſurface of the wooden plate, and the other againſt the lower, countervailing, and conſequently fruſtrating the endeavour of each other, the gravity of the wood itſelf will ſuffice to make it fall, as well as if it were preſſed againſt by neither of them. And from this diſcourſe you will eaſily judge, whether the Pag. 146. doctor had reaſon to ſay as he does, *Quam ab omni ratione igitur abſonum eſt, ut ſuperficies illa ſive area aerearum particularum, quæ inſinuant ſe laminam ligneam inter & marmor, ſolidam columnam hujuſmodi particularum, vi elaſtica ſurſum enitentium, contra laminam ligneam obnitendo vincat, ipſamque laminam in terram deturbet.*

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* See the Tract about the Preſſure of the Air's Spring on Bodies under water.

† Exper xxv. and elſewhere.

CHAP. VI.

WHAT he adds in the ſixteenth number againſt thoſe, that fancy the aerial particles to be endowed with perception, and to act with deſign *pro re nata*, does not all concern me; and what he adds in the next paragraph, wherewith he concludes his twelfth chapter, I ſhall altogether paſs by, as far as it concerns the extravagant conceit he oppoſes. But becauſe at the cloſe of the paragraph, he makes an inference, which comprizes our opinion alſo; ſince he concludes, that the experi-Pag. 150. ment by him alledged, *Certiſſimum eſt indicium, particulas aerias nec cum conſilio nec ſine conſilio inferius marmor ſuſtinere nec ſuffulcire*: it will not be amiſs to ſhew, that our opinion is undeſervedly included in the inference; which I ſhall do by briefly ſolving the phænomenon the doctor lays ſo much weight on. For if we conceive with him, that the two flat marbles formerly mentioned be ſuſpended, and that to the lower of them, a flat wooden plate of the ſame ſhape and extent be applied; I ſee no cauſe to wonder, why the two marbles ſhould ſtick together, and not the lower of them to the wooden plate. For, as I lately noted, there being an area, or bed of aerial particles interpoſed betwixt the marble and the wood, the weight of the atmoſphere, exerciſed by the intervention of thoſe aerial corpuſcles, ought to be æquipollent to the preſſure of the atmoſpherical cylinder, that bears againſt the lower ſurface of the plate; which conſequently by its own weight muſt drop down: whereas there being no ſuch layer of aerial particles interpoſed betwixt the two marbles, the preſſure of the ambient atmoſphere, which touches them every where, ſave where their poliſhed ſurfaces are contiguous, muſt keep them ſtrongly coherent.

I preſume I need not mind you, that hitherto I have diſcourſed upon ſuppoſition, that the doctor experimentally knows, what he delivers concerning the non-adheſion of an exactly ſmooth wooden plate to a marble one; and upon his conceſſion, that, becauſe of the want of ſufficient congruity between the ſurfaces of two bodies, there is a bed of aerial corpuſcles interpoſed between them. But now, I think, it will not be unfit to take notice to you, that though to illuſtrate, on this occaſion, a ſubject, that is generally ſo little underſtood, as the exerciſe of preſſion among fluid bodies, I have anſwered my learned adverſary's objections, as if I had nothing more to ſay for my explication of the ſuſpenſion of coherent marbles, than what I many years ſince delivered in the little tract by him cited; yet I have ſince abundantly confirmed that explication by the 50th of the experiments publiſhed in my continuation; which if the doctor had been pleaſed to read, perhaps he would have received the ſame ſatiſfaction, that other learned men have done; ſince there I experimentally ſhew, that the undermoſt marble, without the accuſtomed clog, would, upon the bare withdrawing of the ſuſtaining air, drop off from the upper. And whereas the two marbles in our vacuum would not cohere, as ſoon as the formerly excluded air was let in upon them, it did by its ſupervening preſſure make them ſtick together very ſtrongly.

The SECOND SECTION.

CHAP. I.

I PROCEED now to the ſecond of thoſe two chapters, that I am intereſted to conſider, in which the learned examiner is pleaſed to attack three or four of my hydroſtatical opinions and explications; in the defence whereof, I hope, I ſhall be the leſs put to exerciſe your patience, becauſe the learned doctor himſelf is pleaſed to grant me almoſt as much as I need deſire concerning the truth of the hypotheſis, whereon my paradoxes and explications are founded. For whereas the main thing I ſuppoſed in my hydroſtatical papers is, that in water, though ſtagnant, the ſuperior parts do actually, though not always prevalently, gravitate upon the inferior, or (if you will) preſs upon them, even when they do not ſenſibly depreſs

preſs them; the doctor in divers places, allows this hypotheſis to be conſonant to the principles of the mechanical philoſophy; and accordingly having ſhewed, that in a ſuſpended tub of water the whole liquor gravitates upon the bottom of the tub; he ſubjoins, Page 161. *Jam verò cum tota hæc aqua conſtet ex particulis aqueis non compactis vel concretis, ſed ſolutis à ſe invicem, impoſſibile eſt, ut omnes fundum ſitulæ premant, niſi inſima quæque ab omnibus ſuperioribus prematur, quemadmodum clarè demonſtravimus in ſecunda ſectione hujus capitis; nempe, ſi nullæ cauſæ niſi purè mechanicæ (quales ſunt motus localis, magnitudo, figura,* &c.) *in edendo hoc phænomeno ſe intermiſcent.*

AND elſewhere in the ſame chapter he ſpeaks thus of the gravitation of liquors (towards the cloſe of the ſecond paragraph.) Page 152. *Neceſſe utique eſt, ut partes ſingulæ gravitent, cum totius ſit gravitatio, ſi non ſit aliquid immateriale principium in rerum natura,* &c. And adds, at the beginning of the next number; *atque ſanè huic externi motûs hypotheſi, & gravitationis elementorum in propriis locis inde neceſſariò emergentis, apprimè conſonum eſt primum illud experimentum, quod ſcriptor profert in paradoxis ſuis hydroſtaticis.*

AND now, Sir, I preſume you do not much wonder, if I think theſe conceſſions reach the main thing I pretend to. For though I do as freely and heartily, as the doctor himſelf, who, I dare ſay, does it very ſincerely, admit, or rather aſſert an incorporeal being, that made and governs the world; yet all that I have endeavoured to do in the explication of what happens among inanimate bodies, is to ſhew, that, ſuppoſing the world to have been at firſt made, and to be continually preſerved by God's divine power and wiſdom; and ſuppoſing his general concourſe to the maintenance of the laws he has eſtabliſhed in it, the phænomena, I ſtrive to explicate, may be ſolved mechanically, that is, by the mechanical affections of matter, without recourſe to nature's abhorrence of a vacuum, to ſubſtantial forms, or to other incorporeal creatures. And therefore, if I have ſhewn, that the phænomena, I have endeavoured to account for, are explicable by the motion, bigneſs, gravity, ſhape, and other mechanical affections of the ſmall parts of liquors, I have done what I pretended; which was not to prove, that no angel, or other immaterial creature, could interpoſe in theſe caſes; for concerning ſuch agents, all that I need ſay, is, that in the caſes propoſed we have no need to recur to them. And this being agreeable to the generally owned rule about hypotheſes, that *entia non ſunt multiplicanda abſque neceſſitate*, has been by almoſt all the modern philoſophers of different ſects thought a ſufficient reaſon to reject the agency of intelligences, after *Ariſtotle*, and ſo many learned men, both mathematicians and others, had for many ages believed them the movers of the celeſtial orbs.

CHAP. II.

BUT you will tell me, that the doctor's conceſſions will not avail me, ſince he urges againſt the gravitation of the elements in their proper places, which gravitation he would have to be ſuſpended by his incorporeal principle, an experiment, which he ſays is moſt manifeſtly repugnant to our hypotheſis. He conceives then, that in a tub or pail full of water, with a perfectly cylindrical cavity, whoſe diameter is of ſixty two parts, there is violently kept at the bottom, by the help of a ſtick, a round plate of wood, whoſe diameter amounts but to ſixty one of thoſe parts; and that as ſoon as ever the ſtick is removed, the wooden plate will emerge to the top and float. *Quod*, ſays he, *prorſus impoſſibile eſſet, ſi omnes partes aquæ ab (F G) ad (H J) non ſolùm junctim fundum vaſis, ſed ſingulæ ſingulas in eadem ſerie ſubjectas actu premerent.* To which aſſertion he immediately ſubjoins this argument to prove it by; *Cum diameter laminæ ligneæ (H M) partes* 61, *habeat æquales, diameter vaſis (H I) habeat* 62, *manifeſtum eſt, quod ſuperficies fundi vaſis ad ſuperficiem laminæ ſe habet ut* 3844, *ad* 3721; *quorum differentia eſt* 123. *Itaque rotundum intervallum inter latera vaſis & marginem laminæ ligneæ habet ſe ad aream laminæ, ut* 123 *ad* 3721, *hoc eſt, area laminæ ligneæ excedit aream dicti intervalli pluſquam triginta vicibus. Ac proinde aqua incumbens ligneæ laminæ excedit magnitudine aquam incumbentem dicto intervallo inter marginem laminæ & latera vaſis plus quàm triginta vicibus, pondúſque ſive preſſio hujus alterius pondus preſſionémque vincit pluſquam triginta vicibus. Adeò ut impoſſibile ſit, ut aqua incumbens prædicto intervallo ita premat aquam ipſi ſubjectam, ut hujus vi ſublevetur lamina, quam vis tricies major deprimit. Quod* (ſays he, by way of inference) *æque abſonum atque abſurdum phænomenon eſſet*, &c. Page 155.

How little this ratiocination agrees with the experiments I have formerly told you of, about the caſes, wherein light bodies will be detained under water, or emerge to the top of it, you will eaſily perceive, if you compare the one with the other, which you may quickly do, if you pleaſe to compare the doctor's diſcourſe with the following narratives of thoſe trials *, to which alone I might therefore refer you. But yet in the mean time, you may, if you think fit, conſider a little, whether the argument, whereon the doctor lays ſo much ſtreſs, be any more than a paralogiſm.

FIRST then, ſince according to his computation the area of the interval, between the ſides of the veſſel and the edges of the round boards, is 123 of ſuch parts, whereof the area of the board amounts to 3721; it is evident, that there muſt be room enough for the water to paſs between the ſides of the veſſel and the edges of the board, which is ſuppoſed on all hands to be of ſome wood lighter in ſpecie than water, ſince elſe it would not emerge upon the withdrawing of the ſtick.

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* See the Tract of the Poſitive or Relative Levity of Bodies under water. Exp. I. &c.

NEXT, this board or wooden plate is not here intimated, or ſuppoſed to be (and indeed in practice can ſcarce be) made exactly congruous to the bottom of the veſſel, and conſequently the water may get in between them; for which cauſe it is neceſſary to keep the wooden plate forcibly down with a ſtick, which elſe were needleſs. And conſequently this interpoſed water will communicate with the laterally ſuperior water in the veſſel, which ſuperior water may, according to the laws hydroſtatical, by the intervention of the interpoſed, exerciſe its preſſure upwards againſt the lower ſurface of the wooden plate.

THIRDLY, the doctor's ſcheme allows and aſſiſts us to conceive, (which we may do however,) an imaginary plane of water to be parallel to the bottom of the veſſel, and to paſs along the bottom of the board; ſo that, of the water, that lies between this plane and the bottom of the veſſel, one part is covered by the wooden plate; and the other, between the edges of that and the ſides of the tub, is covered with the incumbent water only.

CHAP. III.

THESE things being premiſed, I thus argue: it is manifeſted by hydroſtaticians after *Archimedes*, that in water, thoſe parts, that are moſt preſſed, will thruſt out of place thoſe, that are leſs preſſed: which both agrees with the common apprehenſions of men, and might, if it were needful, be confirmed by experiments. It is alſo evident, that that part of the above-mentioned imaginary plane, that is covered by the wooden plate, muſt be preſſed by a leſs weight than the other part of the ſame plane; becauſe the wood being bulk for bulk lighter than water, the aggregate of the wood and water incumbent on the covered part of the ſame plane muſt be lighter in ſpecie, than the water alone, that is incumbent on the uncovered part of the ſame plane; and conſequently this uncovered part being more preſſed, than the other part of the plane, the heavier muſt diſplace the lighter, which it cannot do but by thruſting up the board, as it does, when the external force that kept it down is removed. And, to add this upon the by, this greater preſſure againſt the bottom than againſt the top of bodies immerſed in water ſpecifically heavier than they is a true reaſon of their emerſion, as I have elſewhere ſhewn. So that there happens no more in this caſe, than what uſually happens in the aſcenſion of bodies in liquors ſpecifically heavier than themſelves, on the account of the newly mentioned difference of preſſure. And it is with an expreſs, or ſuppoſed, exception of ſuch a difference, which in many other caſes may be ſafely neglected, that, which I deſire you to take notice of, in moſt places of this diſcourſe I ſpeak of the preſſure of ambient fluids on immerſed ſolids, as uniform or every way equal.

IT is true, that according to the doctor's ſupputation, if the ſolid cylinder, conſiſting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance, it would there many times out-weigh the hollow cylinder of water alone, that leans upon the uncovered part of the imaginary plane. And that is it, that ſeems to have deceived the learned doctor. But there are divers hydroſtatical caſes, wherein the phænomenon depends not ſo much upon the abſolute weight of the compared bodies, as upon their reſpective and their ſpecifick gravity; on whoſe account it is, that a ſmall pebble, for inſtance, that weighs not a quarter of an ounce, will readily ſink to the bottom of the river, on whoſe ſurface a log of wood of a hundred pound in weight will float. It is a rule in hydroſtaticks, that when two portions of water, or any other homogeneous liquor preſs againſt each other, the prevalency will go, not according to the abſolute weight, but the perpendicular height of thoſe portions. And accordingly we find, that if a ſlender pipe of glaſs, being filled with water, have its lower orifice unſtoped at the bottom of a veſſel of water, which contains much more of that liquor than the pipe; yet if this laſt named water were, for inſtance, two foot high, and that in the veſſel but one, the water in the pipe will readily ſubſide, till it come almoſt to a level with the external water, though it cannot do ſo without raiſing the whole maſs of water that ſtagnated in the veſſel.

AND now I ſhall ſubjoin an experiment, which, though at firſt it may ſeem ſlight, and was made in leſſer glaſſes and quantities, than I would have employed, if I could have procured better accommodations, has the advantage of requiring no curious inſtruments, and yet I hope, will ſerve for an ocular proof of the fallaciouſneſs of that reaſoning the doctor is ſo ſtrangely confident of.

WE took an open mouthed glaſs, ſuch as ſome call jars, and ladies often uſe to keep ſweet-meats in, which was three inches and a half, or better in diameter, and ſomewhat leſs in depth, and had the figure of its cavity cylindrical enough. Into this having put ſome water to cover the protuberance, wont to be at the bottom of ſuch glaſſes, we took a convenient quantity of bees-wax, and having juſt melted it, we poured it cautiouſly into the glaſs, warmed before-hand to prevent its cracking, till it reached to a convenient height. This veſſel, and the contained liquors we ſet aſide to cool, in expectation, that when the heat, that had dilated the wax, was gone, it would ſhrink from the glaſs, and conſequently leave a little interval every where between the concave ſuperficies of the veſſel, and convex of the hardened wax; which accordingly came to paſs, and ſaved me the labour of getting the wax ſhaped for my purpoſe with tools; which might have been done, but not without trouble and leſs exactneſs. And now it was eaſy for me to try the experiment I deſigned; for pouring in warily ſome water between the glaſs and the wax, ſo that it filled all the interval, left between thoſe two bodies, both at the bottom and the ſides, the wax was made preſently to float, being viſibly lifted up from the bottom, and its upper part appearing a little above the level of the water, which was no more than I did, and had reaſon to expect, according to the true principles of hydroſtaticks. For water being ſomewhat, though but little, heavier in ſpecie, than wax, and that, which was poured into the bottom and ſtagnated there, being preſſed by the collateral water, every way interpoſed between the concave part of the glaſs, and the convex of the wax (ſo that this collateral liquor anſwered what I lately called a hollow cylinder of water in the doctor's experiment) that part of the ſtagnant water, that was leaned upon by the wax, being leſs preſſed than the other part of the ſame ſtagnant water was by the water incumbent on it; this latter muſt diſplace the former, which it could not do, but by raiſing up the wax, that leaned upon it. And yet this collateral water was ſo far from being heavier, than the wax its preſſure impelled up, that both the collateral, and the ſtagnant water all together, being weighed in good ſcales, amounted to little above a quarter of the weight of the wax, which happened by reaſon of the narrowneſs of the veſſel, which, if it had been wide enough, I doubt not but the experiment would have ſucceeded, though the wax had outweighed the collateral water ten times more, than in our experiment it did. But that the ſolid body exceeded almoſt four times the weight, not only of the collateral, but the ſtagnant liquor too, does ſufficiently overthrow the doctor's ratiocination. Whoſe fallaciouſneſs will yet further appear by two other improvements, among others, which I made of one experiment.

FOR, I. though we poured in more and more water, as long as the veſſel would contain any, the cylinder of wax was but lifted higher and higher from the bottom of the glaſs, but did not appear raiſed more, than at the firſt, above the upper ſurface of the water; which argues, that it was not at all the quantity of the inferior water, which was continually increaſed, but the preſſure of the collateral water, which continued ſtill at the ſame height in reference to that wax, that cauſed the elevation of the body.

AND II. to manifeſt yet more clearly the doctor's miſtake, I deviſed the following trial. We took a round plate of lead about the thickneſs of a ſhilling, and having made it ſtick faſt to the bottom of the cylinder of wax, to make this body ſink the more directly, we placed one after another, upon the upper part of the wax, divers grain weights (firſt wetted to keep them from floating) till we had put on enough to make the wax ſubſide to the bottom: for the facilitating whereof we had pared off its edges; by this means, the glaſs having been at firſt almoſt filled with water, there ſwam about an inch or better of that liquor above the upper ſurface of the wax. And laſtly, we took off by degrees the grain weights, that we had put on, till we ſaw the wax, notwithſtanding the adhering lead, riſe, by degrees, to the top of the water, above which ſome part of it was viſibly extant.

FROM this experiment I thus argue: it is manifeſt, that, according to the doctor's ſuppoſition, here was incumbent upon the wax a cylinder of an inch in height, and of the ſame diameter or breadth with the round ſurface of the wax; whereas upon the removing part of the water, that lay at the bottom when the wax began to riſe, there was incumbent no greater weight than that of the collateral water, and as much of the ſuperior and ſtagnant, as was directly incumbent upon that collateral water (and would have deſerved the ſame name, if we had ſuppoſed the convex ſurface of the wax to have been continued upwards as high as the glaſs reached.) But now, whereas, according to the doctor's ratiocination, this cylinder of water incumbent on the wax, being an inch deep, and a good deal above three inches broad, muſt preſs the wax with a greater weight by ſeveral times, than that, which the lateral and hollow cylinder of this ſtagnant water could have upon the reſt of the collateral water; yet the height of this aggregate of collateral waters being the ſame with that of the wax and the water ſwimming upon it, the difference of the preſſure was ſo ſmall, that barely taking off a weight of four or five grains, the wax would, notwithſtanding the preſſure of the water incumbent on it, be impelled up and made to float; and by the like weight, put again upon it, it would be made to ſink, and by another removal of ſuch a weight, (for I purpoſely reiterated the trial more than once,) it would, though ſlowly, re-aſcend. And theſe phænomena do ſo much depend upon a mechanical æquipollence of preſſure, that even four grains would not have been neceſſary to make the wax riſe or ſink, if it had not been for ſome little accidental impediments, that are eaſily met with in ſuch narrow glaſſes; for otherwiſe in a larger veſſel we have made the ſame lump of wax readily enough ſink or float, by the putting in or taking off a ſingle grain or perhaps leſs.

BY this you may ſee, that for the regulation of hydroſtatical things, nature has her balance too, as well as art; and that in the balance of nature the ſtatical laws are nicely enough obſerved.

YOU may alſo take notice, upon the by, how little the weight of the cylinder of water upon a body immerſed in ſtagnant water is conſiderable, whilſt there is a preſſure of collateral water to counterbalance it; ſince in this laſt trial, though the cylinder of incumbent water did continually increaſe or decreaſe in length, whilſt the lump of wax was ſinking or emerging; yet the ſame deſpicable weight of a grain or leſs, that was juſt able to depreſs it beneath the upper ſurface of the water, did by its preſſure or removal procure its ſinking to the very bottom, or riſing again to the top, and on both occaſions with an equal ſlowneſs, bating that little acceleration of motion, that ought to happen upon another account, and which therefore is to be obſerved in the wax, during its riſing as well as during its ſinking.

CHAP. IV.

SOME other phænomena I produced, by varying the hitherto mentioned experiment, which are very favourable to our notions about hydroſtaticks. But, ſince they do not directly concern the preſent controverſy, I ſhall in this place only annex a couple, the former whereof affords an eaſy confirmation of that paradox, which we lay as the ground of divers others, and the contrary whereof is maintained not only by doctor *More*, but by many other famous and learned men; namely, that in ſtagnant water the upper parts do actually preſs the lower.

WE took then a very ſlender pipe of glaſs, whoſe cavity was narrower than that of an ordinary gooſe-quill, that heterogeneous liquors may not be able to get by one another in it. This pipe, near one end, was bent upwards like a ſyphon, that it might have a ſhort leg, as parallel as the artificer could make it to the longer. Into this crooked pipe we put a little oil, and then held it perpendicularly in a ſomewhat deep and wide-mouthed glaſs, filled partly with water, and partly with a lump of wax, of the bigneſs and ſhape of that already mentioned; that ſo the preſſure of the incumbent water, upon the open orifice of the ſhorter leg, might impel the oil into the longer leg, ſomewhat above the ſurface of the water in the veſſel; which it was convenient ſhould be done, that we might the better ſee the motions of the oil, and which we knew muſt be done by the courſe we took; both becauſe oil is lighter in ſpecie than water, and conſequently required not an equal height of water to counterbalance it; and becauſe, in very ſlender pipes, water is wont to aſcend a little above the level of the external water, whereinto they are immerſed. The pipe being, as was ſaid, held upright, it was eaſy to take notice by a mark, fixed on the outſide, to what height the oil reached in it.

Now if we conceive a horizontal plane, parallel to the bottom of the veſſel, to paſs by the baſis of the floating wax, it is evident by what has been formerly ſhewn, that, of this imaginary plane, that part, on which the wax is incumbent, is as ſtrongly preſſed by the weight of the wax, as the lateral part of the ſame plane is by the weight of the water incumbent on it; (otherwiſe theſe preſſures would not be æquipollent, but the wax would be raiſed:) and conſequently, that part of this plane, that is placed directly over the orifice of the ſhorter leg of the pipe, is no more preſſed, than any equal portion of that part of the ſame plane, that is covered by the wax. This body being taken out of the water, the liquor ſubſided a great way in the veſſel, and ſo did proportionably the oil in the longer leg of the pipe. And laſtly, having weighed out in a good pair of ſcales as much water, as we found the wax to amount to, this liquor was, inſtead of the wax, poured into that which remained in the glaſs; whereupon the oil, in the longer leg of the pipe, was again impelled up very near to the former mark, to which it had been raiſed by the wax. Whence we may gather, that the water newly put in, though in the air it weighed no more than the wax, yet it did as much preſs the water, that lay beneath the forementioned imaginary plane, and conſequently, that which was directly over the ſhorter leg of the pipe, as the wax, that had been taken out, had done. And ſince we have already proved, that the wax did conſiderably preſs that plane, it ought not to be denied, that the water alſo (which inſtead of it was able to impel upon the oil in the pipe) did in like manner preſs that plane; and conſequently, that water may be gravitated in water, as well as a ſolid body, ſuch as wax is, can. And this is the firſt additional uſe, I told you, I would make of our experiment.

BUT, to come now to the ſecond, there is another phænomenon of it, viz. the abovementioned tenderneſs of nature's balance, whoſe uſe ſeems to be of no leſs general concernment to the true doctrine of the hydroſtaticks. For, by duly conſidering that phænomenon, and reaſoning a while upon it, we may be helped to rectify that plauſible miſtake, which has long deluded both philoſophers and mathematicians, and does yet impoſe on moſt of them; namely, that a body does not actually gravitate when it does not deſcend. For we have ſeen already, and ſhall further ſhew by and by, that the ſunken wax, and the braſs grains, that lie on it, do actually preſs or gravitate upon the ſubjacent water and bottom of the veſſel on which it is incumbent; and conſequently its preſſure being not ſurmounted by that of the collateral water, which is unable to raiſe it, muſt be as great, as that of this collateral water. Therefore, when upon the removal of a ſingle grain, the wax, with its incumbent weight, is made to aſcend, and that but very ſlowly, it is evident, that it was ſo far from not gravitating before, becauſe it did not actually deſcend, that it retained its gravity even whilſt it aſcends: as may appear not only by the ſlowneſs of its motion upwards, proceeding from its being in nature's balance very little leſs heavy, than it need be, to countervail the preſſure of the collateral water; but by this alſo, that if but a ſingle grain be laid on it, when it begins to riſe, its aſcenſion will be checked and hindered, which could not be done by the addition of ſo inconſiderable a weight, if the wax and the adhering metal did not, even during their aſcent, retain their former gravity, though that were fruſtrated as to the act of deſcending, or ſo much as keeping their ſtation by the prevailing preſſure of the collateral water: ſo that, ſince, as we found, the wax and adhering metal amounted to a good deal above four-thouſand grains, it did in the balance of nature weigh, whilſt it was aſcending, not ſo much as a four-thouſandth part leſs than it did, whilſt it was actually deſcending.

CHAP. V.

I Should beg your pardon, Sir, for having detained you ſo long with my reply to a ſingle objection of the doctor's, how pompouſly ſoever propoſed; but that I thought it not amiſs to do ſome ſervice to the true theory of hydroſtaticks, by taking this occaſion to preſent you ſome things, that I thought not unlikely to illuſtrate ſome parts of that theory, though above what was neceſſary to anſwer the doctor's argument; to which, I confeſs, I was troubled to ſee ſo learned a man ſubjoin the following concluſion: *Hæc tam luculenta demonſtratio contra gravitationem particularum aquæ inter ſe quamvis junctæ ſitulæ fundum urgeant, ſi non ſit vera atque ſolida, equidem nec mei ipſius nec ullius unquam mortalis in poſterum ratiociniis credam.* But I hope he will not be as bad as his word, but will be pleaſed to conſider, as well as I do for him, that a man may be very happy in other parts of learning, and of greater moment, that has had the misfortune to miſtake in hydroſtaticks, a diſcipline, which very few ſcholars have been at all verſed in, and about which divers of thoſe few have had the misfortune to err, not only in the concluſions they have drawn, but in the very principles they have embraced.

To the foregoing argument the doctor, though he declares he thinks it needleſs, adds in the fifth paragraph another, taken from the laſt experiment of my hydroſtatical paradoxes, by which he ingenuouſly acknowledges, that I ſeem at firſt ſight to have demonſtrated what I pretend to, about the gravitation of the upper parts of ſtagnant water upon the lower. And I am ſorry that I cannot in return acknowledge, that his objection, at firſt ſight, ſeemed to me a cogent one, for neither at the ſecond nor third peruſal can I clearly diſcern where his ratiocination lies, ſuppoſing it to be meant for an anſwer to my experiment. And though I conſulted with ſome learned members of the Royal Society, whereof two are mathematicians, and one his particular friend; yet they all confeſſed he had not ſufficiently explained himſelf on this occaſion, nor could they ſhew me to what argumentation I might properly direct my reply. Only one of the doctor's correſpondents, having ſeriouſly peruſed his diſcourſe, and the annexed ſcheme, told me, that what ſeemed the moſt probable to him, was, that though the doctor was too civil to give me, *in terminis*, the lye; yet he did indeed deny the matter of fact to be true. Which I cannot eaſily think, the experiment having been tried both before our whole Society, and very critically, by its royal founder, his majeſty himſelf. But ſince you have your ſelf ſeen, and made it more than once, I need not ſpend words to convince you, that the matter of fact is true.

But after I had in vain ſought the doctor's meaning where I expected it, chancing lately to caſt my eyes on another place, where I ſaw my ſcheme repeated, I find this paſſage in the explication he endeavours to give of the phænomenon by his hylarchical principle: *Cum verò tam profundè immergitur tubus, ut obturaculum tangat ſuperficiem V W, vis retractionis aeris ita augetur, ut etiam ponderis appenſi ſuperadditam depreſſionem ſuperet. Videtur igitur quaſi quædam ſurſum-ſuctio aeris in tubo contenti, & conformis ac contemporanea aquæ compulſio in obturaculum, quo tam firmiter in os valvulæ comprimitur, ibique cum appenſo pondere ſuſtentatur.* What conſiderable intereſt the ſuppoſed, but unproved, retraction of the valve, or the air itſelf, can have in this phænomenon, I confeſs I do not diſcern; not being able to ſee, but that the experiment would ſucceed, when tried *in vacuo*, although all the atmoſpherical air were annihilated. But if I miſtake the doctor's meaning, I am to be excuſed, ſince I do it not willingly, and his own obſcurity has been acceſſary to it. Nor am I very apprehenſive of being unable to defend my account of an experiment, which (as you know) has had the good fortune to recomend the doctrine, for the proof whereof I deviſed it to many learned and curious perſons, ſeveral of which were ſufficiently indiſpoſed to admit it.

And to avoid all miſtakes and diſputes, that may ariſe (which I think they muſt do needleſsly) upon the ſcore of the valve employed in our experiment, I ſhall remind you of another, that I remember I have ſometimes ſhewn you, and divers other virtuoſi, though I remember not whether I have mentioned it in any of my publiſhed writings. The ſum of this trial is, that an arbitrary quantity of quickſilver being, by ſuction, raiſed into a very ſlender glaſs-pipe, whoſe upper orifice is ſtopped with the experimenter's finger, to keep the mercury from falling before its time, the open end of the pipe with the mercury in it is thruſt into a competently deep glaſs of water till the little cylinder of mercury have, beneath the ſurface of the water, attained to a depth, that is at leaſt fourteen times at great, as the mercurial cylinder has of height. For then, the finger being removed from the upper orifice, the glaſs-pipe will be open at both ends, and there will be nothing to hinder the quick-ſilver's falling down to the bottom, but the reſiſtance of the cylinder of water, that is under it, which cylinder can reſiſt but by virtue of the weight or preſſure of the ſtagnant water, that is ſuperior to it, though but collaterally placed above it: and yet this water being by the pipe, whoſe upper part is higher than its ſurface, and acceſſible only to the air, kept from preſſing againſt the mercury any where but at the bottom of the pipe, and being about a fourteenth part of the weight of an equal bulk of mercury, it is able at that depth to make the ſubjacent water preſs upwards againſt the mercury, which is but a fourteenth part as high as the water is deep, with a force equivalent to that of the gravity wherewith the mercury tends downwards. And to manifeſt, that this phænomenon depends meerly upon the æquilibrium of the two liquors; if you gently raiſe the lower end of the pipe towards the ſurface of the water, this liquor, being not then able to exerciſe ſuch a preſſure, as it could at a further and greater depth, the mercury preponderating, will, in part, more or leſs, as the pipe is more or leſs raiſed, fall out to the bottom of the glaſs. But if, when the quickſilver is at the firſt depth, inſtead of raiſing the pipe, you thruſt it down farther under the water, the preſſure of that liquor againſt the mercury increaſing with its depth will not only ſuſtain the mercury, but impell it up in the pipe to a conſiderable diſtance from the lower orifice of it, and keep it near about the ſame diſtance from the ſurface of the laterally ſuperior water. And this experiment may not only ſerve for the purpoſe, for which I here alledge it; but alſo, if duly conſidered and applied, may very much both illuſtrate and confirm the explication formerly given of the ſeemingly ſpontaneous aſcent of the clogged ſucker in our exhauſted air-pump.

The laſt argument the doctor urges againſt the gravitation of water in what they call its proper place, is deduced from what happens to the divers, who in the middle of the ſea, though the ſalt water of that be much heavier, than that of freſh water rivers, do not find themſelves oppreſſed, or ſo much as feel themſelves harmed or compreſſed by the vaſt load of the incumbent water.

But that the equality of the preſſures of an ambient fluid will go a great way towards the ſolving of this difficulty, you will find by the experiments and conſiderations you will meet with in the following * papers, to which, for that reaſon, I refer you. And though the doctor in this ſame paragraph objects, *tametſi hæc preſſio æqualis ſit, nihil tamen impedit, quò minùs ſubtiliores partes corporis magiſque fluidas exprimat & elidat:* I remember I anſwered that exception before, by ſaying, that thoſe liquors, that he ſuppoſes ſhould be ſqueezed out, cannot be ſo, becauſe there is as great a preſſure againſt thoſe parts, at which they ſhould iſſue, as againſt any of the reſt, if the parts that ſhould be ſqueezed out, be not too ſpirituous and ſubtile, which if they be, I ſhould gladly learn how the doctor knows, that no ſuch minute and ſpirituous particles are really expelled: eſpecially if that be obſerved, which we ſhall ſoon have occaſion to relate, that a ſmall animal, being vehemently compreſſed in water, ſeemed a little, though but a little, to ſhrink.

But that we may the more diſtinctly conſider this grand argument, taken from the experience of the divers, that is wont to be employed by the ſchools, and others, for the vulgar opinion, and is now urged by the learned doctor to prove his; it will be convenient to obſerve, that it does, at once, both propoſe a queſtion, and contain an objection, grounded upon the ſurmiſed inſolubleneſs of that queſtion.

And to begin with the problem, "whence "it is, that divers are ſo far from being killed "or oppreſſed by the weight of the incumbent water, that they are not ſo much as "hurt by it, nay, that they ſcarce feel it at "all?" We may take notice, that there is in it ſomewhat ſuppoſed, as well as ſomewhat demanded. For, in the queſtion, it is taken for granted, that divers, though at never ſo great a depth, feel no preſſure exerciſed againſt them by the water; which is an affirmation in point of fact, of whoſe truth I make ſome queſtion, for the reaſons I ſhall ere long have occaſion to mention.

But it will clear the way for what is to follow, if I here divide the noble and difficult problem, we are to conſider, into two queſtions; the firſt, why a diver ſhould not be oppreſſed and cruſhed to death by the preſſure of the incumbent and ambient water. And the ſecond, why at leaſt he ſhould not be made ſenſibly to feel it, by ſuffering ſome conſiderable inconvenience from it.

In anſwer to the firſt of theſe queſtions, you will eaſily perceive, that divers things may be pertinently applied, that you will meet with in the following paper, to ſhew the difference betwixt the preſſure of fluid, and that of ſolid bodies. And that *de facto* the preſſure of water may be exceeding great, without deſtroying an animal quite ſurrounded with that liquor, I have long ſince ſhewn in another † treatiſe, by the experiment of a little tadpole, which being, together with the water it ſwam in, included in a bent glaſs ſealed at one end, the animal was not killed or ſenſibly hurt, but only (according to what was lately noted by anticipation) ſeemed to ſhrink into ſomewhat, and but little, leſſer dimenſions.

If it be here alledged, that this experiment makes rather againſt me than for me, the learned doctor having made uſe of it with a ſcheme to explain it in his ſixteenth paragraph; it will be fit for me to conſider his objection. Having then recited the matter of fact newly delivered, he adds, *Quod certè fieri non poſſet niſi juxta legem quartam contruſio particularum aquæ contra ſe invicem principio hylarchico inhiberetur & eluderetur. Atque hinc fit, ut quamvis aqua in tubo (A B C) vi trudis (G F) aliquantò facta ſit condenſatior, partes tamen ſic compreſſæ, ut propiùs ad ſe invicem accedant, nihilo inde inter ſe fiunt comprimentiores.* And then ſubjoining the following paſſage; *Neque enim ſequitur ex earum contactu, quod premant ſe invicem, quandoquidem particulæ, uti fit in duris corporibus, in unum coaleſcere poſſunt, & tamen non mutuò ſe premere*; (wherein are ſome things that might be queſtioned, if it were neceſſary;) he thus purſues his diſcourſe: *Cùm verò hìc particulæ aquæ, ſi omninò premerent ſe invicem, preſſura in gyrinum, columnæ aqueæ, ducentos vel trecentos pedes, æneæ verò, plus viginti vel triginta pedes altæ, preſſionem adæquaret, luculentum eſt indicium, quod revera particulæ ſe invicem non premant. Nam planè eſt incredibile, columnam æneam pro corpore quidem gyrini latam, ſed altam viginti vel triginta pedes & amplius, gyrinoque ad perpendiculum incumbentem, omnia viſcera tam tenellæ gelatinæ non eſſe eliſuram.* Notwithſtanding which allegation I am apt to think, you will judge the argument, from this experiment, to be more probable on my ſide than on the doctor's. For there being in our caſe an animal, exceedingly much more tender than a man, expoſed to a preſſure, which he affirms is ſo great, that if it were exerciſed on the tadpole, it ought to ſqueeze out all his guts, I think, I may pretend to have given a pertinent inſtance, that a diver may be at a conſiderable depth under water preſerved from being cruſhed to death by the weight of it. And whereas the doctor tells us, that the cauſe of the incolumity of the tadpole is, that the preſſure or contruſion of the particles of the water againſt one another is hindered or fruſtrated by the *principium hylarchicum*, I reply; that what I affirm is matter of fact, and evident, (namely, that there was a great external force duly, and yet ineffectually applied to preſs to death, by means of the water, the animal ſwimming in it;) but that this mechanical force was ſuſpended, or made ineffectual, by ſome inviſible and immaterial agent, is but the doctor's hypotheſis, and a thing, which, whether it be true or no, is at leaſt not manifeſt.

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* The Author means the new experiments of the differing preſſure of heavy ſolids and fluids.

† The Author points at the Appendix to the Hydroſtatical Paradoxes.

HAVING ſaid thus much about the firſt queſtion; I now proceed to the ſecond, "Why divers, though at never ſo great a depth, complain not of the preſſure of the water, nor ſuffer any harm nor inconvenience by it?"

AND here, Sir, the queſtion highly meriting a particular curioſity, I ſhall not ſcruple in the more full enquiry I am now entering upon, as well ſometimes to employ and inlarge particulars already mentioned in the laſt of the following papers, as oftentimes to ſtrengthen them with new ones. And I ſhall alſo, for a while, ſuſpend my difference with the doctor, and addreſſing myſelf to you, who, I am ſure, will allow me, that water weighs in water, propoſe, according to my cuſtom, not as a dogmatiſt, but as an enquirer, ſome particulars, that may tend to the ſolution of a problem, which I take to be as difficult as noble. Not that I doubt, but it muſt and will be explicated upon the mechanical principles; but partly, becauſe the application of them to the ſolution, will not offer itſelf to every ſeeker; and partly, becauſe we are not yet well furniſhed, either with experiments made on bodies under water, or ſo much as with ſo competent an account of the matter of fact, as I think may keep wary men from heſitations about it. For what is commonly reported concerning the divers, is (as has above been intimated) grounded but upon their own relations and anſwers, perhaps amplified or procured by leading queſtions from perſons, who are generally either ſlaves or ignorant men, taken from the leſs ſober part of the illiterate vulgar, and prepoſſeſſed with the common opinion of the non-gravitation of water in its own place; and conſequently are not like to make over-accurate obſervations, but prone to refer the inconvenient alterations, they feel, to any other cauſe, than the preſſure of the water, which they are taught to be none at all. If obſervations about diving were made by philoſophers and mathematicians, or, at leaſt, intelligent men, who would mind more the bringing up out of the ſea inſtructive obſervations, than ſhipwracked goods, we ſhould perhaps have an account of what happens to men under water, differing enough from the common reports.

YOU will in one of the following papers find mention of a learned phyſician of my acquaintance, that, upon his diving leiſurely, perceived a conſtriction to be made of his thorax by the action of the ſurrounding ſea-water.

A Spaniſh prelate, that lived long in *America*, ſpeaking of the deplorable condition of thoſe wretched Indians, that were employed by their inhuman maſters about the fiſhing for pearls, gives us this account of them: "It is impoſſible, that men ſhould be able to live any long ſeaſon under the water, without taking breath, the continual cold piercing them; and ſo they die commonly parbreaking of blood at the mouth, and of the bloody-flux cauſed by the ſtomach. Their hair, which are by nature cole-black, alter and become afterwards a branded ruſſet, like to the hairs of ſea-wolves, &c."

Purch. Tom. IV. Lib. 8. p. 1587.

AND a general of the Engliſh in the *Eaſt-Indies*, being by them employed on an embaſſy to the emperor of *Japan*, has this paſſage concerning ſome female divers, that he met with in his voyage: "All along this coaſt, and ſo up to *Ozaca*, we found women divers, that lived with their houſhold and family in boats upon the water, as in *Holland* they do the like. Theſe women would catch fiſh by diving, which by net and line they miſſed, and that in eight fathom depth. Their eyes, by continually diving, grew as red as blood, whereby you may know a diving woman from all other women." I know it may be ſaid, that theſe diſeaſes may proceed from the coldneſs and moiſture, or other qualities of the ſea; nor would I confidently reject ſuch a ſurmiſe: but it may alſo be poſſible, that the compreſſion, they ſuffered under water, might have, at leaſt, a ſhare in the production of theſe ill effects. For how are we yet certain, that the preſſure of the water againſt their bodies, though it does not manifeſtly diſlocate any ſolid or firm part, but only ſomewhat preſs inwards, as in the abovementioned tadpole the outward ſkin and the fibres, (both which will eaſily yield a little way, without being painfully ſtretched,) may not, by ſtraitening the veſſels, and otherwiſe inconveniently alter the circulation of the blood and the motion of the humours, ſpirits, and other fluid parts of the body? And I am not ſure, that much of the cold, that divers are wont to complain of, when under water, may not be a diſaffection produced in the nervous and membranous parts, occaſioned by the compreſſion of the ambient water, there being divers things, and preſſure among others, beſides actual cold, that will make men complain of being cold; and in our caſe, this ſenſation may be excited, or aſſiſted, by the hindering of the uſual perſpiration at the conſtipated pores of the ſkin. And it ſeems not impoſſible, that one, not ſo ignorant and heedleſs as divers are wont to be, may refer a new ſenſation, that really proceeds from preſſure, to other cauſes; ſince learned and intelligent men, when prepoſſeſſed (as theſe common divers uſually are) with the vulgar opinion about the non-graviation of water and air in their natural places, do almoſt always refer * an experiment of

Purch. Tom I. Lib. 4. C. 1.

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* The reaſon of which experiment may be gathered from the fourth Chapter of the Author's long ſince publiſhed Defence againſt *Linus*.

of my engine to ſuction, which is indeed the effect of the preſſure of the ambient, (as I have * elſewhere clearly ſhewn,) and affirm, that the pulp of the finger, or hand, is drawn up into a hollow pipe, into which it is indeed thruſt by the weight of the ambient air. But all theſe things I have mentioned, not as if I laid any great weight upon each of them, but to let you ſee, that it was not altogether without cauſe, that I complained of the incompetency of the hiſtory of what divers feel under water; eſpecially at great depths, where this want of information may be more conſiderable: for, as far as I have yet learned by peruſing voyages, and enquiring of travellers of my acquaintance, the places, where they are wont to dive for pearl, are but moderately deep, and indeed ſhallow, in compariſon of the great depths of the ſea; ſo that if we were furniſhed with as many relations of theſe profound places, as we have of the others, poſſibly the accounts would be different enough to render doubtful, or correct the received opinions about the conditions of divers at the bottom of the ſea. For, I remember, that a credible eye-witneſs, who, if I miſtake not, was the intelligent *Oviedo*, ſpeaking of the pearl-fiſhing on the American iſland of *Cubagna*, has, among many other notable obſervations, ſuch a paſſage as this: “ But whereas the “ place is very deep, a man cannot naturally “ reſt at the bottom, by reaſon of the abun“ dance of aery ſubſtance, which is in him, “ as I have oftentimes proved. For although “ he may by violence and force deſcend to the “ bottom, yet are his feet lifted up again, ſo “ that he can continue no time there. And “ therefore where ſea the is very deep, theſe In“ dian fiſhers uſe to tie two great ſtones about “ them with a cord, on each ſide one, by the “ weight whereof they deſcend to the bottom, “ and remain there, until them liſteth to riſe “ again, at which time they unlooſe the ſtones, “ and riſe up at their pleaſure.”

AND now to come cloſer to the explication of our difficult problem; there yet occurs to me nothing more likely in order to it, than what I have already mentioned in the paper you will meet with about the differing preſſures, &c. And therefore it ſhall here ſuffice me to enlarge, and by further conſiderations and experiments confirm, what is there more ſummarily diſcourſed; namely, that the phænomenon may depend, chiefly, upon theſe two things, the uniform preſſure of the fluid ambient, and the robuſt texture of a human body expoſed to this preſſure.

IN one of the following † papers, you will find examples of the great preſſure, that may be ſuſtained unharmed by ſuch frail bodies as eggs and thin glaſſes, that one would expect ſhould be broken in pieces thereby, provided the preſſure be exerciſed by the intervention of an ambient liquor; as water. And by the account, elſewhere referred to, of the tadpole, it ſeems highly probable, that even that tender animal, when it ſeemed by ſome ſmall diminution of the bulk to be every way a little compreſſed inwards, was put to no conſiderable, or perhaps to any ſenſible, pain or inconvenience, ſince it ſeemed to ſwim without any irregular motions, which would in likelihood have enſued, if it had been much harmed or incommodated. Which example, with thoſe formerly pointed at, may teach us, that there may be a vaſt difference betwixt the reſiſtance, that a body can make, when compreſſed immediately by ſolid bodies, and when in the compreſſion every way ambient fluids intervene. Which you will the leſs admire, if you conſider, that by reaſon of the groſneſs, hardneſs, or rigidneſs of viſible ſolid bodies, the preſſure can never be made every where ſo equally, as by the parts of liquors, whoſe ſmallneſs, which renders them ſingly inviſible, fits them to accommodate themſelves far more cloſely and conveniently to all the ſuperficial parts of the body immerſed in them, and to have the force of the compreſſing body more uniformly diſtributed to them. But becauſe the inſtances referred to are taken from bodies ſurrounded with water, I will take two or three about the reſiſtance of bodies to violently compreſſed air; partly, becauſe thoſe made in our engine are wont to be performed with air, not condenſed, but rarified or expanded beyond its uſual conſiſtence; and partly, becauſe it will not be denied, that the corpuſcles of air may be really compreſſed or thruſt againſt one another, ſince it is clear, that they may be crouded into far leſs room, than they poſſeſſed before, and bear ſo ſtrongly againſt the glaſſes, that impriſon them, as not ſeldom, if too much compreſſed, to burſt them in pieces.

CONSIDER then, that among bodies not fluid, the ſwims of ſmaller fiſhes are likely to be judged none of the moſt able to reſiſt compreſſion, ſince they conſiſt of bladders ſo thin and delicate, that a piece of fine Venice paper is very thick in compariſon, and that they contain nothing in them but ſoft air, not compreſſed by any outward force. I cauſed one of theſe bladders, of above an inch in length, and proportionably great, to be taken out of a roach, and anointed it with oil to keep it ſupple, and preſerve it from being pierced or ſoftened by the water; and having by a weight of lead, faſtened to the neck of it, let it down to the bottom of a hollow cylindrical tube, ſealed at one end, and made purpoſely large, and about 56 inches long, for ſome hydroſtatical experiments; we could not perceive, that by the weight of all the incumbent water it was manifeſtly compreſſed, or that it did diſcover the leaſt wrinkle or other depreſſion of the very thin membrane, though ſtuffed but with air. And this trial was made more than once with the ſame ſucceſs; and yet, that this proceeded rather from the robuſtneſs of the bladder, that was able to reſiſt the weight of a taller pillar of water, than from the non-gravitation of water in the upper part of the tube on that in the lower, we ſhewed, by preſently letting down ſuch a mercurial-gage, as is deſcribed, and often mentioned in the Continuation of our New Experiments.

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* In a paradox about ſuction.

† New experiments about the differing preſſure of heavy ſolids and fluids.

For letting down this by a ſtring to the bottom of a tube, the weight of the incumbent water forced up ſome of the mercury out of the open leg of the ſyphon into the ſealed one, and conſequently compreſſed the air included there, which though it were not very much, yet it was very manifeſt. For the uncompreſſed air being three inches and $\frac{3}{4}$ in length, we judged it at the bottom of the tube about $\frac{5}{8}$ by the intruſion of the mercury, that was impelled up; and to ſatisfy myſelf, and others, that if the incumbent water had been heavy enough, it would have viſibly depreſſed the bladder in ſpite of any *principium hylarchicum*, ſince I could not have a tube long enough, the bladder was ſunk into a chryſtal glaſs, that had a long and cylindrical neck, and was ſo well ſtuffed with a ſtopple, that was cylindrical too, that it was very difficult for any thing to get out betwixt it and the orifice of the glaſs; then a competent quantity of air being left above the water, the ſtopple was warily, and by degrees thruſt down, and ſo leſſening the capacity of the glaſs, compreſſed the air, that was next it, and by the intervention of that, the water, that was under it. And though there did not, upon a ſlight compreſſion of the outward air, appear any ſenſible operation upon the bladder, that was at the bottom of the water; yet, upon a farther intruſion of the ſtopple, the preſſure being increaſed, the immerſed bladder diſcovered not only one, but two conſiderably deep wrinkles, which preſently diſappeared upon the drawing up of the ſtopple. Upon whoſe being thruſt in again, depreſſions were again to be ſeen on the ſwim. And we having been careful to convey into the ſame glaſs ſuch a mercurial gage as has been lately ſpoken of, we eſtimated, by the condenſation of the air in the ſealed leg of that gage, that the bladder had been expoſed to a preſſure, that might be equivalent to that of a pillar of about forty foot of water.

THIS I hope will leſſen the wonder, that bodies of ſo firm a texture as thoſe of luſty men, ſhould ſupport the preſſure of the water at ſuch depths, as divers are wont to ſtay at; ſince we ſee, what reſiſtance can be made by ſo exceding thin and delicate a membrane ſtuffed only with air, in compariſon of the ſtrong membranes and fibres of a man, ſtuffed beſides air, with more firm parts. I will not here urge, that great weights may be ſuſtained in the air by ſuch tendons, or cords of fibres, and by other fibres, as it were, interwoven into membranes, in compariſon of what an ordinary man would expect: but I ſhall invite you to conſider with me, that not only upon the account of the ſtable parts of the human body, but of the ſpirits too, it may reſiſt very violent preſſures (and ſuch as perhaps have not yet been conſidered) of a fluid body, not only without any manifeſt contuſion or diſlocation of parts, but without any ſenſe of pain; which I ſuppoſe you will grant me, if, conſidering what great effects guſts of wind have upon doors, trees, nay maſts of ſhips, blowing them down, nay breaking them; and that yet a man, without being extraordinary ſtrong, will ſtand againſt the impetuoſity of ſuch a ſtrong wind, and walk directly againſt it, by vertue of the vigour of his muſcles and ſpirits, without being thrown down, or bruiſed by ſo violent a current of air, as beats upon him, but without ſo much as complaining, that he feels any pain; and this, though the wind that beats againſt him, however it be a fluid body, yet becauſe it acts as a ſtream, does not uniformly compreſs him, but invade only the fore-part of his body. Likewiſe, in the lifting up heavy weights by porters, carriers, and other luſty men, we may ſee the ſlender tendons of the hands loaded with a hundred, or a hundred and fifty, or perhaps a far greater number of pounds, without having their fibres ſo far compreſſed, or ſtretched, as to make the lifters complain of pain, though ſometimes they may of difficulty. So that, (as I could, if it were needful, confirm by other inſtances) a human body is an engine of a much firmer ſtructure than ſcholars are wont to take notice of. And here let me add, that I doubt whether, if the ſtructure of a man were not conſiderably (though not perhaps equally) firm, he would, eſpecially in a deep ſea, be able to bear the preſſure of the water, though not immediately applied, without pain. For (to give you one reaſon more of my not acquieſcing in vulgar reports about diving) having ſeveral times converſed with a man, apt enough both to enquire and obſerve, who got his living by taking up ſhip-wrecked goods, he anſwered me, when I aſked him, whether he felt any peculiar preſſure againſt the drums of his ears, which are membranes not ſo well backed as thoſe of other parts; that when he ſtaid at a conſiderable depth, as ten or twelve fathoms, under the ſurface of the ſea, he felt a great pain in both his ears, which often put him to ſhifts to leſſen it; which, by his manner of deſcribing it, I concluded was from the incompetent reſiſtance of the air, which he acknowledged to me, he found by manifeſt tokens to be notably compreſſed by the ſuperior water. Which relation from ſuch a perſon does not only confirm our explication, but likewiſe warrant us to doubt, whether the common reports, that are made concerning divers, be fit to be relied on, without farther examen and obſervation.

IN the mean time, I ſhall add two or three experiments more, to confirm the reſiſtance, that animals may make to a great preſſure, when exerciſed by the mediation of a fluid body. And I the rather gave you an account of this way of making trials, becauſe it may be alſo helpful to diſcover the reſiſtances of inanimate bodies, whoſe ſhape and conſiſtence we may chooſe and vary, almoſt at pleaſure, to the preſſure of (totally, or in great part) ambient fluids. And if I had been furniſhed with a tube wide enough, and a quantity of mercury great enough, I might, by the way, have ſhewn you, that whatever the learned Dr. *More* is pleaſed to ſuppoſe, that to butter itſelf, even as conſiderable a preſſure may be ſo applied, as not to be able to make it yield thereunto. For on this occaſion I ſhall add, that I well remember, that, among other trials to the ſame purpoſe, I cauſed a piece of freſh butter, about the bigneſs of a ſmall hen-egg, to be brought to an irregular ſhape, that if the compreſſion were ſuch, as many would expect, the long corners, or ſolid angles, being at leaſt flatted, the butter might be reduced into a more capacious figure, and leſs remote from roundneſs. But, though having put this lump of butter into a bladder, almoſt full of fair water, we proceeded, both in the ſame braſs cylinder, and much after the ſame manner, that I employed about the egg mentioned in the fourth experiment of the Tract of the differing preſſure of heavy ſolids and fluids; yet I found, that after the plug had been loaded with a weight of lead of above fifty pound, neither I, nor the operator, perceived the irregular figure of the butter to be altered. Nor was this the only trial of this kind I made with the like ſucceſs upon butter, though I dare not charge my memory with the circumſtances; and therefore I ſhall, without delay, proceed to what I was about to recite concerning the reſiſtance of animals.

We took then a common fleſh-fly, neither of the biggeſt ſort of all, nor of the leaſt, but of a middle ſize; and having put it into the ſhorter leg of a bent glaſs, which we cauſed to be hermetically ſealed at the end, there was put in as much mercury as filled that leg, and a part of the other, leaving little more than an inch of air between the quick-ſilver and the ſealed end, that there might be room both for the fly, and the condenſation of the air, and then with a little rammer, fitted for the purpoſe, we cauſed the mercury in the open leg to be thruſt againſt that in the ſealed leg, which thereupon did neceſſarily croud the air near the fly into leſs room; ſo that, by our gueſs, it was condenſed into about a third part of the ſpace, which it poſſeſſed before, and which it regained when the rammer was withdrawn: and though this were done more than once, yet not only the fly was thereby not killed, but not ſo much, that appeared, as ſenſibly hurt; and I perceived her, whilſt ſhe was pent up, to move her legs, and to rub them one againſt the other, as it is uſual with that ſort of inſects to do of their own accord in the free air. Nor did I queſtion, but that, if the glaſs had not been inconveniently ſhaped to admit the rammer farther into it, the fly would have ſupported a far greater preſſure.

Another experiment, to the ſame purpoſe, we tried with water inſtead of mercury: but, whereas this laſt named liquor could neither wet nor drown our fly, (for which reaſon I chiefly made choice of it) the other did firſt wet its wings, and ſoon after, by a miſchance, drown it. But firſt we had an opportunity to compreſs the air into a third, if not into a fourth part of its former dimenſions; and yet the fly continued to move divers of her parts, and eſpecially her legs, very vigorouſly, as if nothing troubled her, but her being, as it were, glued to the inſide of the glaſs by part of her wetted wings. And this, I hope, will keep the reſiſtance of divers to the ambient water from ſeeming incredible; ſince ſuch flies were able to reſiſt, and, for aught appeared, without harm or pain, the preſſure of the crouded particles of the air; though we gueſſed this to have been as much compreſſed by the force of the rammer, as it would have been by a cylinder of water of fifty, or between fifty and ſixty foot high. By which alſo we may be helped to conceive, how great a difference there is, whether the ſame preſſure be exerciſed by a ſolid, or by a fluid body. For, according to our eſtimate, the preſſure againſt the body of the fly was as great, as if a ſlender pillar of marble, having the fly for its baſe, and eighteen or twenty foot in height, had leaned upon the little animal; which, I preſume, you will eaſily think was more than enough to cruſh her to death.

But becauſe, though the foregoing trials are not like to be rejected by the ſkilful, yet they require a ſomewhat dextrous and nimble experimenter, and leave ſomething to his eſtimate, I will ſubjoin an experiment more eaſy to be made, and wherein the weight may be determined by meaſure, rather than conjecture, being made to be perpendicularly incumbent on the fly, or other animal. For the experiment may be as well made on other inſects, as worms, though ſome, that I had provided, chanced to miſcarry before they came to be uſed.

We took then ſome ordinary black flies (ſuch as uſe to haunt butchers ſtalls in warm ſeaſons,) of a middle ſize, (the length of the body and head of one animal, which for trial's ſake we meaſured, being about three eighths of an inch,) and having placed one of them with the head upwards, that there was ſome diſtance left betwixt her and the ſealed end of the glaſs-tube nine or ten inches long; we poured in quick-ſilver very ſlowly and cautiouſly, leſt the force of ſo heavy a body, acquired by the acceleration of its deſcent, ſhould, more than the meer weight itſelf of the liquor, oppreſs the fly. To this effect ſtooping the glaſs very much towards the horizon, and letting the mercury paſs into the tube thorough a funnel, whoſe lower part was very ſlender, that it might come down but by little and little, we at length got in as much mercury as the tube would receive, and then holding it upright, we watched, whether the fly would make any motions; and finding, that ſhe did manifeſtly ſtir notwithſtanding the incumbent mercury, we meaſured the height of the mercurial pillar, reaching from the middle of her body to the top of the liquor, and found it to be about eight inches; and the quickſilver being poured out, the fly appeared to be ſo lively and vigorous, that I doubted not, but if we had a longer glaſs, the experiment had been much more conſiderable. But, when afterwards I was able to procure a better tube, the ſeaſon of flies being almoſt quite paſt, I could ſcarce get any, and thoſe not briſk, as they are wont to be in ſummer. But however, we repeated the experiment with one of the beſt we could take of the above-mentioned ſize, and ordering the matter ſo, that the mercury incumbent on her, (for there was ſome beneath her,) appeared to be of a greater height than the formerly employed tube was of, we ſaw her move one or other of her little legs divers times, though the tube were held upright; and therefore meaſuring the height of the mercury above her, we found it to amount to ſixteen inches and better, and then freeing her from this preſſure, we obſerved, that ſhe immediately found her legs again, and moved up and down briſkly enough; but when ſhe was loaden with twenty-three or twenty-four inches of the ſame quick-ſilver (though the liquor were ſoon after poured out) ſhe gave no ſigns of life; which I ſuſpected might happen, not ſo much from her having been oppreſſed by the greatneſs of her weight, as from the great care of the operator to let down the mercury very obliquely and warily upon her. And this I was the rather confirmed in, becauſe, having got another fly of about the ſame bigneſs, though when ſhe was at the bottom of the quickſilver, ſhe ſeemed ſo compreſſed, as not to have any motion, we could take notice of, yet, upon her being taken out of the glaſs, ſhe preſently appeared to be alive by walking about, and beginning to diſplay her wings, though the pillar of mercury, that had leaned upon her, amounted to above twenty-ſeven inches. And I preſume, the ſucceſs would have been much more conſiderable, if the experiment had been tried in the ſummer, when theſe creatures are briſk and lively, and not, as it was, in the winter; beſides that probably theſe little animals were hurt or weakened by the violence, that would ſcarce fail to be uſed in catching them, and putting them into ſuch a place and poſture in the glaſs, as was required; the actual coldneſs of the quickſilver perhaps alſo making them ſomewhat torpid, whilſt it touched them ſo many ways. And it muſt not be here omitted, that a fly, that ſeemed but about half ſo big as one of thoſe hitherto mentioned, being well placed, with ſome mercury under it, in a glaſs-pipe held upright, ſuſtained a mercurial pillar of ſomewhat above twenty-five inches; and though ſhe was not obſerved to move under ſo great a weight, yet when once it was taken off, ſhe did not appear hurt, much leſs cruſhed to death by it, and probably would have eſcaped under a much greater weight, if the tube, which was too large, had not already employed all the ſtock of mercury we then had at hand. But I do preſume, that what we did try, will be available to our purpoſe, ſince we ſee clearly, that ſo ſmall an animal as a fly may ſurvive ſo great a preſſure, and that ſhe could not only live, but was able to move ſuch long and ſlender bodies as her legs, when ſhe was preſſed againſt by above ſixteen inches of mercury, and conſequently by a weight, equivalent to a pillar of water of above eighteen foot and a half, which being above five-hundred and ninety times her own length, and, according to the eſtimate our meaſure ſuggeſted, many times more her own height; ſo that a diver, ſix foot tall, (which is ſomewhat more than an ordinary man's ſtature,) to have as many times his height of water above him, as our fly might have had, and yet have moved under it, muſt dive, at leaſt in freſh water, to near a hundred fathom, which is a far greater depth, perhaps by five or ſix times, than, for aught I could learn by enquiry, the divers either for coral or pearl are wont to deſcend.

And now, Sir, having tendered you the likelieſt conjectures, that occurred to me about the ſolution of this difficult problem, I ſhall return to doctor *More*, and conſider the objection he frames from the ſuppoſed inſolubleneſs of it. And on this occaſion, I ſhall have two or three things to repreſent to you.

The firſt is, that there would be much more weight in what he objects, if our aſſertion of the gravitation of water in water were, like the *principium hylarchicum*, a meer hypotheſis advanced, without any clear poſitive proof; whereas our doctrine is not only elſewhere directly proved by particular experiments, but by the very controverted one of the tadpole; to elude whoſe force, ſo ingenious a perſon is fain to fly to a principle, that, to ſay here no more, is not phyſical. And from this firſt of the things, I lately mentioned, I ſhall haſten to the ſecond, becauſe it will require to be longer inſiſted on.

I ſhall then further repreſent, that whatever power he is pleaſed to ſuppoſe at the bottom of the ſea, to ſuſpend the impreſſion of the incumbent water, I think, that ſuppoſition ought to give place, if not to our former ratiocinations, yet to experience itſelf, which ſhews, there really is a great preſſure exerciſed by the water at the bottom of the ſea. I remember, that a friend of the learned doctor's and mine, who is ſo eminent a virtuoſo, as to have been often preſident of the royal ſociety, related a while ſince to me, that a mathematical friend of his, whom he named, having had an opportunity to try an experiment, I have in vain endeavoured to get tried for me, had the curioſity to let down in a deep ſea a pewter-bottle, with weight enough to ſink it, that he might try, whether any ſweet water would ſtrain in at the orifice or any other part; but when he had pulled it up again, he was much ſurprized to find the ſides of his pewter-bottle very much compreſſed, and, as it were, ſqueezed inward by the water. I alſo, not long ſince, enquired of an obſerving acquaintance of mine, that has a conſiderable eſtate in *America*, whether he had not tried to cool his drink, when he ſailed through the torrid zone, by letting down the bottles to a great depth into the ſea, and if he did, in what condition he found them when they were drawn up again. To which he anſwered, that he had ſeveral times employed that expedient for the refrigeration of his drinks, but was at firſt amazed to find the corks, with which the ſtrong ſtone-bottles had been well ſtopped before, ſo forcibly and ſo far thruſt in, that they could ſcarce have been ſo violently beaten in with a hammer, and it was ſcarce poſſible to get them out. And another ingenious perſon, that practiſes phyſick in the *Indies*, having the like queſtion put to him, anſwered me, that he had ſome while ſince had the curioſity to try, in a very

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deep part of the ſea, whether any freſh water would ſtrain into ſtone-bottles through a thick cork ſtrongly ſtopped in, and having let it down with a convenient weight to one hundred fathom, was much diſappointed, when he drew it up, by finding, that the preſſure of the water at ſo vaſt a depth had quite thruſt down the cork into the cavity of the bottle (which elſe perhaps would have been cruſhed to pieces;) an effect, which he would ſcarce have expected from the ſtrokes of a mallet. And if to all this it be objected, that it was not the preſſure, but the coldneſs of the water, that did the recited feats, by condenſing the included air, and obliging nature to do the reſt for fear of a vacuum; I will not launch into the controverſy, whether nature do any thing *ob fugam vacui*, but only anſwer, that I cannot find, by the relations of the divers, or otherwiſe, that it is ever ſo cold at the bottom of the ſea, as it is frequently above ground in winter, when great fiſhes are commonly ſaid to return to the deep parts of the ſea for warmth; and yet, in the ſharpeſt winters, I never obſerved corks to be driven in by the cold of the ambient; nay, I purpoſely tried with a frigorifick mixture, that very intenſe degrees of cold, ſuch as would quickly freeze many liquors, would not occaſion the breaking of thin bubbles of glaſs, purpoſely blown at the flame of a lamp and hermetically ſealed.

AND to ſhew *ad oculum* (as they ſpeak) that water may preſs more and more, as it grows deeper, againſt the ſtopple of a bottle, though the veſſel be inverted, I will ſubjoin this experiment. Becauſe we have no water hereabouts, that is near deep enough to force in a cork, as the ſea-water did in the above recited trials, I thought of a way of ſo cloſing the glaſs-veſſel, as that the ſtopple ſhould keep aſunder the air in the veſſel and the outward water, and hinder all immediate intercourſe between them, and alſo make ſome reſiſtance againſt the preſſure of the external water, and yet be capable of freely moving up and down, and ſo be a good ſuccedaneum to a ſolid ſtopple. Taking then a glaſs-vial, furniſhed with a ſomewhat long cylindrical neck, whoſe cavity was large in proportion to the reſt of the veſſel, we put into it as much quickſilver as would in the neck make a ſhort mercurial pillar of between half an inch and an inch; then, a piece of very fine bladder, dipped in oil, was ſo tied over the orifice of the glaſs, that no mercury could fall down, or get out, nor water get in at the orifice, and yet the bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the cavity of the vial, or depreſſed by the weight of the mercury. This little inſtrument, firſt furniſhed with a weight of lead to ſink it, being inverted, the mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple, and yet, with its lower part, depreſſed the bladder beneath the horizontal plane, that might be conceived to paſs by the orifice; then the glaſs being a while kept in the water, that the included air might be brought to the temperature of the ſurrounding liquor, and by a ſtring let further down into the ſame glaſs veſſel filled to about two foot in height, the preſſure of the liquor againſt the orifice of the vial did by degrees drive up the bladder and the mercurial ſtopple into the cavity of the neck, as was manifeſt by the aſcenſion of the quickſilver; and when the inſtrument was leiſurely drawn up again, the weight of this mercury made it ſubſide and plump up the bladder again as before. An experiment a-kin to this, and therefore fit to confirm it, I have delivered in another diſcourſe*.

AND here I ſhall ſubjoin what very opportunely occurred to me ſince the writing of the laſt page. Meeting caſually with an ingenious mechanician, (whom you will find I have elſewhere † mentioned) that deviſed a ſuit of cloaths and other accommodations, wherein I once ſaw him let down into the water, by whoſe help, and that of a boat, he could, and did continue there a great while, at a conſiderable depth under water, and there work; I aſked him afreſh (to obtain fuller informations than formerly) whether he felt not the preſſure of the water againſt his breaſt and belly; to which he anſwered me (more circumſtantially than he had before) that when he was about four or five yards under water, though but in the river *Thames*, his breaſt and abdomen was ſo compreſſed, that there being hardly room enough left for the free motion of his lungs, he could ſcarce fetch his breath, and was neceſſitated to make them draw him quickly up, and that (among his later trials to improve his engine) having for remedy hereof, cauſed a kind of armour for the cheſt and back to be made of copper, though the ſtiffneſs of the metal defended him from receiving any miſchief in thoſe parts, yet in the others, where only the leather, though ſtrong, was interpoſed, when he came to the depth of about ſix fathom, though in freſh water, he found a great preſſure againſt his legs and arms and all the other parts againſt which the water was able to thruſt the leathern ſuit inwards. And this preſſure being found by him, as he told me, pretty equal, againſt all the expoſed parts, (for from the other, which were more yielding and obnoxious, the armour kept it off,) he received no miſchief from it, nor yet much incommodity (and ſome he might expect from the ſtiffneſs and unequal yeilding of the leather:) ſo that he could ſtay under water, though not ſtill at ſo great a depth, about two hours or longer. And upon the whole matter he anſwered me, that he was well ſatisfied by his trials, that the ambient water endeavoured to preſs him and his diving ſuit every way inwards. Whether the coldneſs of the water had any intereſt in this phænomenon, I particularly enquired of the engineer; but he replied, that by reaſon of the tightneſs of his diving ſuit or inſtrument, the warm ſteams of his body, that were pent in, and other concurring circumſtances kept him from feeling any cold, and made him ſometimes feel a greater heat than he wiſhed. He has promiſed me, before it be very long, to make for me a trial or two, that

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* See the paradox about ſuction.

† In the tract of the differing preſſure of heavy ſolids and fluids.

I propounded to him, from whoſe ſucceſs, if he can but reduce them to experiment, I hope to be able to preſent you a farther confirmation of our hypotheſis. In the mean time, the things already recited, together with the preceding experiments, may well ſuffice for our preſent purpoſe. For, by what hath been ſaid, it appears, that water does actually preſs againſt bodies, whether ſpecifically lighter or heavier than itſelf, placed under water, and that this preſſure increaſes with the height of the water above the emerſed bodies. And this being ſo, it is not more neceſſary for me, than for men of other opinions, to give a clear reaſon, why divers can reſiſt ſo great a preſſure of the incumbent water. And the preſſure of the water in our recited experiment having manifeſt effects upon inanimate bodies, which are not capable of prepoſſeſſions, or giving us partial informations, will have much more weight with unprejudiced perſons, than the ſuſpicious, and ſometimes diſagreeing accounts of ignorant divers, whom prejudicate opinions may much ſway, and whoſe very ſenſations, as thoſe of other vulgar men, may be influenced by prediſpoſitions, and ſo many other circumſtances, that they may eaſily give occaſion to miſtakes. I know, that learned men, that never were converſant in hydroſtaticks, are wont to think it very difficult, if not impoſſible, to conceive, how ſo weak a thing, as they fancy an animal to be, ſhould avoid the being oppreſſed, or ſo much as harmed by ſo great a weight of water. But they, that ſhall attentively conſider what has been offered towards the removal of this difficulty, and remember, how little they would have believed, that there is ſo great a difference, as we have by the tadpole, the fly, and other inſtances, ſhewn there really is between the preſſure of ſolid and of fluid bodies, will, I preſume, be apt to think it fit, that if, for want of a ſufficient hiſtory of matters of fact, any ſcruple remain about the ſolution we have offered from the nature of the uniform preſſure of fluids, and the firm ſtructure of the human body, we ſhould, to remove thoſe remaining ſcruples alſo, rather range about for other phyſical helps to ſolve more compleatly the problem, about ſuch a thing as compreſſion, which is an action purely corporeal and mechanical, than for want of a ready and compleat ſolution to fly to the immediate interpoſition of an immaterial and intelligent, yet created agent, to explain clearly whoſe manner of working would be a much more difficult taſk, than the ſolution of the phænomenon without it.

AND now, Sir, having preſented to you the reflections I thought requiſite to write upon the learned doctor's diſcourſes againſt my hypotheſis and explications, relating to the gravitation and preſſure of fluids, I have little more to trouble you with in this paper. For, though in the latter part of the thirteenth chapter, the doctor is pleaſed to ſpend divers pages in the explication of divers of my hydroſtatical phænomena by the agency of that incorporeal director, that he calls *principium hylarchicum*; yet ſince theſe explications of his are rather attempts to accommodate the phænomena to the hypotheſis, than objections directly levelled againſt my ſolutions, I ſhall altogether forbear to examine them; the main thing, that I intended in this paper, according to what I told you at the beginning, being to ſhew, that the arguments urged againſt the mechanical ſolutions of the experiments by me recited do not evince any of them to be erroneous. And I have neither the deſign nor the leiſure ſolicitouſly to examine the doctor's hylarchical principle. Of which I ſhall only ſay, that though he tells us, it is *paratum ad movendum quoquoverſum materiam pro data occaſione*; Page 175. yet ſince he alſo tells us, *Quòd particulæ molis* Page 167. *corporeæ, ſive ſtabilis ſive fluidæ, à principio hylarchico in unam aliquam partem omnes junctim urgeri poſſunt & premi, quamvis ſingulæ ſingulas in nullam partem premant, quodque pro magnitudine molis major minorve totius ſit preſſio*; and that the force, by which it endeavours to keep the elements in their true and natural conſiſtence, though it be very great, is not invincible: I ſee no need we have to fly Ibid. to it, ſince ſuch mechanical affections of matter, as the ſpring and weight of the air, the gravity and fluidity of the water and other liquors, may ſuffice to produce and account for the phænomena, without recourſe to an incorporeal creature, which it is like the Peripateticks, and divers other philoſophers, may think leſs qualified for the province aſſigned it, than their fuga vacui, whereto they aſcribe an unlimited power to execute its functions. I leave it therefore to you, Sir, to judge, which of the two ways of explicating an hydroſtatical phænomenon, the learned doctor's, or, that which I have made uſe of, reliſhes moſt of the naturaliſt. And I ſhall only tell you, that if I had been with thoſe Jeſuits, that are ſaid to have preſented the firſt watch to the king of *China*, who took it to be a living creature, I ſhould have thought I had fairly accounted for it, if, by the ſhape, ſize, motion, &c. of the ſpring-wheels, balance and other parts of the watch I had ſhewn, that an engine of ſuch a ſtructure would neceſſarily mark the hours, though I could not have brought an argument to convince the Chineſe monarch, that it was not endowed with life. From which compariſon you will eaſily gather, that what I have thought my ſelf concerned to do in this place, was not to demonſtrate in general, that there can be no ſuch thing, as the learned doctor's *principium hylarchicum*, but only to intimate, that, whether there be or not, our hydroſtaticks do not need it. Nor do I think it neceſſary to the doctor's grand and laudable deſign, wherein I heartily wiſh him much ſucceſs, of proving the exiſtence of an incorporeal ſubſtance. For as I think, truth ought to be pleaded for only by truth; ſo I take that, which the doctor contends for, to be evincible in the rightest way of proceeding by a perſon of far leſs learning than he, without introducing any precarious principle; eſpecially experience having ſhewn, that the generality of heathen philoſophers were convinced of the being of a divine architect of the world, by the contemplation of ſo vaſt and admirably contrived a fabrick, wherein yet taking no notice of an immaterial *principium hylarchicum*, they believed things to be managed in a mere phyſical way, according to the general laws, ſettled among things corporeal, acting upon one another. And after this I have nothing more to ſay, but that I would not have any thing, that I have ſaid, miſconſtrued to the learned doctor's prejudice. For it is not neceſſary, that a great ſcholar ſhould be a good hydroſtatician. And a few hallucinations about a ſubject, to which the greateſt clerks have been generally ſuch ſtrangers, may warrant us to diſſent from his opinion, without obliging us to be enemies to his reputation. And therefore, if you have found any thing in this paper inconſiſtent with a juſt tenderneſs of that, you have not only my conſent, but my deſire to alter it, as an expreſſion, that doth not well comply with my intentions of not appearing any farther his adverſary in our debate, than the deſire of ſhewing my ſelf a friend to the truth I was to defend, ſhould exact of,

SIR,

Yours, &c

AN
HYDROSTATICAL LETTER,

Written *February* 13, 167$\frac{2}{3}$.

CONTAINING,

A Dilucidation of an Experiment of the Honourable Author of theſe TRACTS, about a Way of weighing Water in Water, upon the occaſion of ſome Exceptions, made to it by Mr. *George Sinclair* *.

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* In his Hydroſtaticks, printed at *Edinburgh*, 1672. p. 146. ſſ.

To the READER.

WHEN this diſcourſe was juſt finiſhing in the preſs, there came to the publiſher's hands a dilucidation of an experiment of the honourable author of theſe tracts, about a contrivance of his for eſtimating the weight of water in water, formerly publiſhed in Number L. of the *Philoſophical Tranſactions*, and by the following diſcourſe cleared from the exceptions to be met with in Mr. *George Sinclair*'s book, entitled *The Hydroſtaticks*, &c. printed at *Edinburgh*, 1672. Which dilucidation, becauſe of the affinity of the ſubject, was thought fit to be here annexed.

AN HYDROSTATICAL LETTER, &c.

SIR,

CALLING this night in *Paul's Church-yard* for the ingenious Mr. *Ray's* travels, that you yeſterday commended to me, I was alſo ſhewn a new treatiſe, that I never ſaw before, of a learned gentleman, and haſtily running over the index, found an experiment of mine declared inſufficient; and though, being hindered to make haſt home, it be ſo late, that, far from having time to peruſe the book itſelf, (which I tell you, that you may not now expect any character of it from me) I have been ſcarce able to read over, more than once, what directly concerns me in it; yet I ſhall adventure to ſay ſomething about it this night, for fear I ſhould not, in ſo buſy a time as this, be allowed to do it to-morrow.

WHEREAS then the learned objector, having recited my experiment about weighing water in water, as you were pleaſed to publiſh it in a book enriched with ſo many better things, the *Philoſophical Tranſactions*, begins his animadverſions with ſaying, that "herein is a great miſtake:" I ſhall not in that much oppoſe him; for poſſibly the diſpute between us is not much more than verbal. And becauſe my experiment coming abroad by itſelf, and ſuppoſing things, that I had formerly proved, and publiſhed, but which were not expreſly referred to in it, I wonder not, that my meaning ſhould not by all readers be fully underſtood. And therefore, to explain myſelf on this occaſion, give me leave both to repeat my opinion, and to ſhew you, on what occaſion, and how far, I deſigned to confirm it by this experiment. My opinion then was, and ſtill is, that as water is a heavy fluid, ſo it does retain its gravitation and power of preſſing; by which I mean a tendency downwards (whatever the cauſe of that gravity be) whether it have under it a body either ſpecifically heavier or lighter than itſelf, or equiponderant to it. For I ſee not what ſhould deſtroy or aboliſh this gravity, though many things may hinder ſome effects of it. And therefore I ſuppoſe, that water retains its gravity not only in air, but in water too, and in heavier liquors, and conſequently, by vertue of this, the liquor preſſes upon them; but if a ſurrounded fluid have, upon the ſcore of its ſpecifick gravity, an equal, or a ſtronger tendency downwards, than water, it will, by vertue of that, be able to impel up this liquor, or to keep it from actually deſcending: ſo that a portion of water, ſuppoſed to be included in a veſſel of the ſame ſpecifick weight with water, this portion, I ſay, placed in a greater quantity of the ſame water, will neither riſe nor fall, as I have elſewhere ſhewn; but yet it retains its gravity there; only this gravity is kept from making it actually deſcend by the contrary action of the other water, whoſe ſpecifick gravity is ſuppoſed equal: as when a juſt balance is loaded with a pound weight in each of its ſcales, though neither of the weights actually deſcend, being hindered by its counterpoiſe, yet each retains its whole weight, and with it preſſes the ſcale it leans upon; ſo that our lately mentioned included portion of water does really preſs the ſubjacent water, though it does not actually depreſs it, or (as perhaps a ſchool-man would phraſe it) does gravitate on it, but not pregravitate. Nor do I think, that the only way of judging, whether a body gravitates, is to obſerve, whether it actually deſcends, ſince in many caſes its gravity may be proved by the reſiſtance it makes to heavy bodies, which, if it were not one, would raiſe it: as may be declared by what I juſt now noted about equal weights in a balance. And for want of this diſtinction I have known even learned men, treating of hydroſtatical things, miſtake both me and the queſtion.

Numb. L.

THE next thing I had to tell you, is, that the adverſaries I had to deal with, both in print, and in diſcourſe, denied, that in ſtanding water, the upper parts did preſs or gravitate upon the lower; and though they could not but grant, that the whole weight of the water did gravitate upon the bottom of the veſſel; yet they would have the parts of it to do ſo *actione communi* (as they ſpeak) and fancied I know not what power of nature to keep the homogeneous portions of water, as well as other elements, from preſſing one another, when it is in its proper place. Againſt this opinion, (which I preſume my learned adverſary and I agree in oppoſing,) it was alledged, beſides other things, which I found many, otherwiſe good ſcholars, were not fitted to underſtand, that if a glaſs-vial or bottle, well ſtopped, were deeply immerſed under water, it would ſtrongly tend upwards; but if it were dextrouſly unſtopped, when it was thus immerſed, ſo as the water could get in, abſtracting from or allowing for the weight of the glaſs itſelf, it would by the water, that crouds in and thruſts out the air, be made ſtrongly to tend downwards, and continue ſunk. But this not ſatisfying, becauſe it was pretended, that the reaſon of the empty bottle's emerging, when ſtopped, was the poſitive levity of the air it was filled with, and the ſinking of it, when unſtopped, was from the receſs of the ſame air, that by the intruding water was driven with large bubbles out of the bottle; I thought this evaſion might be obviated by contriving an experiment, wherein the water ſhould be plentifully and ſuddenly admitted into the glaſs, and yet no air expelled out of it, (which circumſtance I therefore took notice of, where

I ſay, “no bubble of air appeared to emerge or eſcape through the water,”) ſo that if then the glaſs, that was kept up before, ſhould fall to the bottom, with a gravitation amounting to a conſiderable weight in reſpect of its capacity, the ſinking of it could not by them be aſcribed, as they ſuppoſe, with poſitive levity, but to the weight of the admitted water, which, when thus weighed, would be invironed with water of the ſame kind: and to ſhew, that this admitted water might have a conſiderable weight, notwithſtanding the place it was in, I employed a pair of ſcales after the manner, that is recited in the experiment.

By what I have been diſcourſing, you may conceive, that however my expreſſions diſagree with thoſe of my adverſary, the diſtance of our opinions is not ſo wide, as at firſt ſight it ſeems. For he allows, as well as I, that the ſuperior parts of water do by their gravity, (for I know not on what other ſcore they can do it) preſs the inferior. But this he would not have amount to this expreſſion, “that water weighs or gravitates in water;” whereas I ſcruple not to cloath my ſenſe in that expreſſion, becauſe I think water does always exerciſe its gravity, though it does not always pregravitate, or actually deſcend, being often, as I noted above, either impelled up by an oppoſite and prepollent weight, or hindered from deſcending by the reſiſtance of other water, that counterpoiſes it: ſo that, if he thinks, that in my experiment I meant to propoſe a method of making water deſcend in water, and weigh it in that liquor with a pair of ſcales, juſt as if I would weigh in the ſame water a piece of lead, or a portion of mercury, which are bodies much heavier in ſpecie than water, either he miſtakes my intention, or I did not ſufficiently declare it. But that, which I deſigned to ſhew, and, for aught I can yet foreſee, have ſhewn, was, that by the help of an ordinary balance it may be made appear, that water admitted into the glaſs bubble, I employed, did make the glaſs bubble weigh ſo much heavier than it did before that liquor entered into it; and that this new weight, that was manifeſted by the balance, was not due, as my adverſary ſuppoſed, to ſuch a receſs of the air, as I mentioned a while ago.

And now, Sir, it will be proper to take notice of ſome paſſages in the objector's diſcourſe, in order to dilucidate the ſubject of it. Whereas he ſays, (page 149, and 150.) “Take a piece of wood, that is lighter in ſpecie than water, and add weight to it by degrees, till it become of the ſame weight with water; knit it with a ſtring to a balance, and weigh it in water; and you will find the whole weight ſupported by the water:” I anſwer, that this does not at all overthrow my opinion, but agrees very well with it. For ſuppoſe the weight you add to the light wood be lead, it cannot be ſaid, that the metal loſes its native ponderoſity, whilſt it reſts in the water; and the reaſon, why it deſcends not, is, that it, and the wood it is joined to, are hindered by the counterpoiſe of the collateral water, which, by its preſſure, would raiſe the ſurface of the water, whereon the floating or ſwimming body leans, if it were not hindered by the weight of theſe incumbent ſolids: and this reſiſtance of theirs to the endeavour upwards of the water, being exerciſed only upon the account of their gravity, ſhews, that they do in my ſenſe gravitate, though not pregravitate.

Again, if you pleaſe to conſider the caſe, put by the objector, page 151, and caſt your eyes upon his ſcheme, which, ſuppoſing you to have his book, I ſhall, for brevity ſake, make uſe of at preſent; you will find him thus argue. “Now, I ſay, it is ſix ounces of the Pag. 151. weight (*B*) that makes this alteration, and turns the ſcales: for if twelve ounces ſink the glaſs below the water, when it is full of air, and no water in it, then ſurely ſix are ſufficient to ſink it, when it is half full. And the reaſon is, becauſe there is a leſs potentia, or force, in ſix inches of air, by the one half, to counterpoiſe a weight of twelve ounces, than in twelve inches of air. Therefore this air being reduced from twelve inches to ſix, it muſt take only ſix ounces to ſink it.”

To which I anſwer, that I know not yet what, on this occaſion, he means by a potentia, or force, in ſix inches of air, to counterpoiſe a weight of twelve ounces. For by the term counterpoiſe, where the queſtion is about weighing, one would think he ſpeaks of weight; and yet air, according to the vulgar opinion, is poſitively light; according to us, though it have a gravity, yet in our caſe that muſt amount to ſo little, that what air the bubble needed to fill it, could not weigh at moſt above four or five grains, which therefore might ſafely be neglected. But, according to my opinion, the reaſon of the phænomenon is clear enough, without meddling with the potentia of the air. For if we conceive a horizontal plane to divide the water mentally, and paſs by the bottom of the ſuſpended bubble; before the little ſtem be taken off, there is a far greater preſſure upon the other parts of that plane, than upon that, which lies under the bubble, in regard they are preſſed by the weight of the collateral water (*A*, *L*, *G*, *D*, *M*, *C*,) whereas the other is preſſed only by the weight of a body very much lighter than its equal bulk of water: ſo that, to keep the bubble from being forcibly buoyed up, there was requiſite eighteen ounces of lead, that make up the plummet (*B*,) to detain it under water, and keep the beam of the balance horizontal; that when acceſs is given (at *C*) to the neighbouring water, it is by the weight of the collaterally ſuperior water impelled into the cavity of the bubble, where the air, being much rarified before, could not reſiſt its ingreſs, and thereupon, ſix ounces of water getting in, that part of the imaginary plane, on which the bubble was incumbent, is preſſed by a greater weight than formerly by ſix ounces, and conſequently, there needs the like weight in the oppoſite ſcale of the balance, to reduce the ſcale to an æquilibrium. And if we ſuppoſe, with our author, the glaſs to be compleatly full of water, and the counterpoiſe in the ſcale (*O*) to need ſix ounces more to make a new æquipondium, the account of the phænomenon will be the ſame, as, if you attentively conſider it, you will clearly perceive. And the reaſon, why the additional weight of ſix ounces is required, will be, that the upper half of the bubble, that before contained leſs than three or four grains weight of air, being now filled with water, amounted to ſix ounces more of water than formerly, and ſo the counterpoiſe, in the oppoſite ſcale (*O*,) will need the weight of ſix ounces to make a new æquipondium.

Pag. 152. Congruously to this explication, when the examiner ſays, "Now I enquire, whether theſe eighteen ounces are the æquipondium of the water within the glaſs, or of the weight of the lead (*B*)? It is impoſſible they can counterpoiſe both, ſeeing the water is now twelve, and *B* eighteen. It muſt then either be the counterbalance of the water, or the counterbalance of the lead. It cannot be the firſt, becauſe twelve cannot be in æquipondio with eighteen; it muſt then be in the ſecond: or if theſe eighteen ounces in the ſcale (*O*) be the counterpoiſe of the water within the glaſs, I enquire what ſuſtains the weight of the lead (*B*)? The weight of it cannot be ſuſtained by the water, becauſe it is a body naturally heavier than water; it muſt therefore be ſuſtained by the balance." I anſwer, that this ſpecious objection ſeems, (for it is ſomewhat obſcurely worded) to be founded upon a miſtake of my meaning in the queſtion. However, as to the phænomenon itſelf; according to my ſenſe, the eighteen ounces in the ſcale (*O*,) are the counterpoiſe of the eighteen ounces, that hang from the oppoſite and æquidiſtant ſcale, and make up the leaden plummet (*B*,) which anſwer, I ſee not how our author prevents.) But then you will aſk, what counterpoiſes the water in the bubble, which alone weighs twelve ounces? I anſwer, that it is the gravitation of the collateral water, which preſſes the other parts of the lately-mentioned imaginary plane, as much as the water in the bubble, the weight of the glaſs being here not reckoned by either of us; and the water incumbent on the bubble does preſs that part of the plane on which they lean; ſo that there being in all thirty ounces to be ſuſtained, the eighteen of the plummet, and the twelve contained in the glaſs, the lead, that hangs in the water, is counterpoiſed by eighteen ounces in the ſcale, and the water in the bubble by the preſſure of the collateral water.

But you will ſay, that it appears not, that the included water preſſes at all, ſince it does not at all deſcend. To which I anſwer, that as long as the water was getting into the cavity of the bubble, ſo long it did manifeſtly gravitate upon the ſubjacent plane, and actually deſcend, raiſing the counterpoiſe in the ſcale: but when, by adding more weight to that counterpoiſe, things are brought to a new æquilibrium, there is no reaſon, why the gravitation of the water ſhould again change the now regained æquipondium. Suppoſe, in the two ſcales of a balance there were placed two equally capacious and equiponderant vials, whereof one is quite full, and the other almoſt full; it is evident, that the full veſſel will keep the ſcale it leaned upon depreſſed, and if you gently pour in as much water into the unfilled, as the filled has more than it, the ſcale, that was formerly kept raiſed, will be now depreſſed, till the beam be brought to be horizontal; to which poſture when it is once brought, the æquilibrium will continue: and yet it will not be ſaid, that though the added water, whilſt it was filling the glaſs, depreſſed the ſcale it belonged to, yet it loſt its weight, or, which in my ſenſe is all one, did not gravitate upon the ſcale, when the balance was come to an æquilibrium, becauſe then this water did no longer depreſs it. And how much the water in our bubble does, notwithſtanding its immerſion, gravitate, would be viſible, if, by ſuppoſition, it were all annihilated, and no other ſuffered to ſupply its room. For then the ſubjacent part of the imaginary plane being much leſs preſſed, than immediately before, the weight of the collaterally ſuperior water would ſtrongly impel up the bubble, if it were not kept in its place by a proportionable addition of weight to the plummet. Nor ſhould it ſeem a ſtrange thing, that I ſhould ſay, that the thirty ounces, lately mentioned, ſhould be counterbalanced partly by the weight in the oppoſite ſcale, and partly by the water, that fills the immerſed bubble; ſince this notion may be warranted even by the common practice of weighing heavy ſolids hydroſtatically. For if you would, for inſtance, weigh a lump of copper of nine pound in common water, the metal, hanging by a horſe-hair under water, will need, according to my elſewhere mentioned experiments, either juſt or near about eight pound in the oppoſite ſcale, to keep the balance horizontal, ſo that the whole nine pound, that the lump weighed in the air, is counterpoiſed partly by the eight pound newly mentioned in the oppoſite ſcale, and partly by the weight, or reſiſtance following from weight, of as much of the water as the copper fills the room of; which, as experience ſhews, is one pound: and if we ſhould conceive water in a veſſel adiaphorous, as to gravity and levity, to be ſubſtituted in the place of the metalline lump, it would weigh as much as the ninth part of the copper lump weighed in the air, and the ſame counterpoiſe of eight pound would maintain the æquilibrium.

What the learned objector has, at the cloſe of his diſcourſe about the natural and artificial balance, could not without prolixity, and is not here neceſſary to be dwelt upon; eſpecially ſince you will ſee, in what I ſuppoſe you have now received from the preſs, in anſwer to the ingenious Dr. *More*, what is to be ſaid on that ſubject, according to my hypotheſis. Wherefore, though my learned adverſary does in the 152d page conclude, "That water cannot weigh in water," and aſſerts "that the preſſure of water is one thing, and water to weigh in water is another;" yet, as

I ſaid at firſt, I conceive much of our difference may be verbal; and, in my ſenſe, when water preſſes ſubjacent water, becauſe it does ſo upon the ſcore of its gravity, it gravitates in water, though it does not pregravitate, that is, actually deſcend. And ſince it is in the ſenſe of this laſt expreſſion, that our author, if I miſtake him not, ſpeaks of weighing in water, his concluſion, that water cannot weigh in water, does not contradict me, who affirm not, that water does ſo weigh in water. Whether we ſhall agree in all other points of Hydroſtaticks, you will eaſily believe, that I cannot yet tell; though by the expreſſion he is pleaſed to uſe in the 146th page, to uſher in his objection with, it is probable we may. And as to the now-diſpatched debate, if I have employed ſome words in another ſenſe than he, I preſume he is ſo equitable as to conſider, that I did not write of theſe things after having ſeen this book of his, but ſome years before; and have ſince found thoſe expreſſions juſtified by the uſe, that eminent writers have thought fit to make of them. And however I am glad, that he has given me this opportunity of clearing my experiment, and declaring by examples, as well as words, the opinion it relates to; eſpecially, if it ſeems to others, that I omitted to expreſs myſelf ſo fully; my deſign being, as I formerly told you, to convince ſuch adverſaries, as I then had met with, by ſhewing, that the above-recited phænomena of the emerſion and ſinking of a glaſs vial depended upon the gravity of the water, and not upon the poſitive levity of the air.

NEW EXPERIMENTS

Of the Positive or Relative

LEVITY of BODIES under Water.

It is obvious, even to the vulgar, as well as to philoſophers, that if wood, wax, or another body, that is lighter in ſpecie than water, and naturally floats upon it, be detained under water, it will, upon removal of that force, emerge to the top. And this it does ſo readily, and, as it ſeems, ſpontaneouſly, that not only the Peripatetick ſchools, but the generality of philoſophers, both ancient and modern, do, as well as the vulgar, aſcribe this aſcenſion of lighter bodies in water to an internal principle, which they therefore call poſitive levity.

But this principle was not always ſo univerſally received among philoſophers, as in later ages it proved to be; *Democritus*, and ſeveral of the ancients, both atomiſts and others, admitting no abſolute, but only a relative or reſpective levity; which opinion ſome of the moderns have ingeniouſly attempted to revive.

But, becauſe whatever wit they may have employed in arguing, yet the ſchools ſeem to have the advantage in point of experience, the obvious inſtances, given by the Peripateticks, having neither been ſolved by real and practical variations of the ſame inſtances, nor counterbalanced by new experiments of a contrary tendency; the importance and difficulty of the ſubject invited me to attempt, when I was upon hydroſtatical trials, whether I could experimentally ſhew, that whatever becomes of the general queſtion about poſitive levity, we need not admit it for the true and adequate cauſe of the emerſion of wood, and ſuch lighter bodies, let go under water.

EXPERIMENT I.

The inſtance, that is wont to be urged to prove the poſitive levity of wood in water, ſeems to me to have been too perfunctorily made, to be ſafely acquieſced in. For even as it is propoſed with advantage by a learned foreign mathematician, I cannot think it accurate enough to determine the preſent controverſy: for I will readily allow him to ſuppoſe, that in caſe a flat board, as for inſtance, a trencher, have its broad ſurface kept by a man's hand or other competent force upon the horizontal bottom of a tub full of water, if the hand or other body, that detained it be removed, it will ordinarily happen, that the trencher will haſtily aſcend to the ſurface of the water. But I do not perceive, that a deciſive experiment of this kind is eaſy (not to ſay, poſſible) to be made with ſuch materials. For the wood, whereof both the trencher and the bottom of the barrel conſiſt, are ſuppoſed to be lighter in ſpecie than water; and to be ſo, they muſt be of a porous and not very cloſe texture. To which agrees very well, that the ſolider woods, as lignum vitæ, Braſil, &c. whoſe texture is more cloſe and compact, will not float on water but ſink in it: and therefore, if there be not much more care uſed, than I have yet heard, that any experimenter has employed, to bring the ſurfaces of the trencher, and the bottom of the barrel, to a true flatneſs, and as much ſmoothneſs, as they can be brought to, I ſhall not think the trial ſo accurately made, as it might be; not to ſay, which I ſuſpect, that though it be mentally, yet it is ſcarce practically poſſible to bring ſuch porous bodies, as thoſe of the lighter woods, to be fit for ſuch a contact as might be neceſſary to make the trial accurately. And in caſe that were actually done, I ſhould be kept from expecting, with my adverſaries, the emerſion of the trencher, by the experiment by and by to be recited, and by the true reaſon of it.

I think then, that the cauſe, why in ordinary inſtances, wood, wax, and other bodies ſpecifically lighter than water, being let go at the bottom of a veſſel full of that liquor, emerge to the top, is chiefly, that there is no ſuch exquiſite congruity and contact between the lowermoſt ſuperficies of the wood, and the upper ſurface of the bottom of the veſſel, but that the lateral parts of the water, being impelled by the weight of the parts of the ſame liquor incumbent on them, are made to inſinuate and get between the lower parts of the wood and the bottom of the veſſel, and ſo lift or thruſt upwards the wood, which bulk for bulk is leſs heavy than the water that extrudes it.

THAT this is the reaſon of the emerſion or aſcenſion of bodies, lighter in ſpecie than the fluids they ſwim in, is moſt conſonant to the laws of * Hydroſtaticks, as I have elſewhere ſhown. But whereas the whole force of the argument of thoſe I diſpute with, conſiſts in a ſuppoſition, that, becauſe the trencher (formerly ſpoken of) is placed upon the bottom of the barrel, no water can come between to buoy it up, whence they conclude, it muſt aſcend by an internal and poſitive principle of levity, I thought fit to make the experiment after another, and, if I miſtake not, a better manner.

WE took then two round plates of black marble ſhaped like cheeſes, which had thoſe ſuperficies, that were to be clapped together, ground very flat, and poliſhed very carefully, that the ſtones being laid one upon the other, might touch in as many of the ſuperficial parts, as the workman could bring them to do; that, whilſt they were in that poſition, the uppermoſt being taken up, the other would ſtick to it, and aſcend with it. And to keep out the water the better, the internal ſurfaces were, before they were put together, lightly, and but very lightly, oiled; which did not hinder them from moſt eaſily ſliding along one another, either forward or backwards, or to the right, or to the left, as long as the contiguous ſurfaces were kept horizontal.

THESE things being done, a blown bladder, of a moderate ſize, was faſtned to the upper marble, and both of them were let down to the bottom of a tub of water, where, by the help of an eaſy contrivance, the lower marble was kept level to the horizon. And now the patrons of poſitive levity would have concluded, that the bladder, being a body, granted to be by vaſt odds lighter than wood, and being in an unnatural place beneath the ſurface of the water, ſhould, of its own accord, and with impetuoſity, emerge; but I expected a contrary event, becauſe the bladder being tied to the upper marble, ſo that both of them might in our caſe be conſidered as one body, the water could not impel them up, in regard, that the cloſe contact of the ſurfaces of the two marbles kept the water from being able to inſinuate itſelf between them, and conſequently from getting underneath the upper marble, and preſſing againſt the lower ſuperficies of it. And to ſhew, that this was the reaſon of the bladder's not emerging, I cauſed one of the by-ſtanders to thruſt his arm down to the bottom of the tub, and with his hand to make part of the oiled ſurface of the upper marble ſlide off, on any ſide, from that of the lower, which, by reaſon of the ſmoothneſs and ſlipperineſs of the ſurfaces, he found moſt eaſy to do. But the contact ſtill continuing according to a greater part of the ſurfaces than was requiſite, I bid him yet ſlide, but by ſlow degrees, more and more of the upper marbles from the lower, till at length, when, according to his gueſs, the marbles touched but in one half of their ſurfaces, the endeavour of the water to extrude the bladder full of air being ſtronger than the reſiſtance, which the contact, but of part of the ſurfaces of the ſtones, was able to make, they were ſuddenly disjoined, and the bladder was by the extruding water impetuouſly, as it were, ſhot up, not only to the top of the water, but a good way beyond it.

WITH theſe marbles we made ſeveral other experiments of this kind, moſt commonly letting down the marbles both together; but once or twice at leaſt placing the upper marble under water upon the lowermoſt already fixed to the bottom of the barrel.

THAT it was not the weight of the upper marble, nor want of lightneſs, whether poſitive or relative, of the air included in the bladder, that kept it from aſcending, was plain, not only upon the newly-mentioned impetuous emerſion of it, upon the disjoining of the marbles, but by this, that the bladder would lift up from the lower parts of the water, not only the upper ſtone, when it touched not the other, but a weight of ſeven or eight pound hanging at it.

AND that a *fuga vacui* was not an adequate cauſe of the coheſion of the marbles in our experiment, may be argued from this, that whether or no nature do any thing, at any time, out of abhorrence of a vacuum (which may be much diſputed;) yet, in our caſe, this abhorrency could not be well pleaded by its aſſertors, ſince many of them hold it to be unlimited, and the more modeſt, to be at leaſt capable of lifting up prodigious weights; whereas, in our experiment, the levity of a bladder, that could not raiſe ten pound weight, was ſufficient to disjoin the marbles, when they yet touched one another according to half their ſurfaces.

EXPERIMENT II.

TO ſhew, now whether it is not rather the gravity and preſſure of the water, or other ambient fluid, than the poſitive levity
of

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* See the Hydroſtatical Paradoxes.

of a body lighter in ſpecie than it, that makes the immerſed body aſcend to the ſurface of the liquor, I deviſed this experiment:

WE took a bladder, out of which a great part of the included air had been expreſſed, and tying the neck of it very cloſe, that none of the remaining air might get out, we faſtned to it a conſiderable weight of ſome very ponderous body, as lead or iron. By the help of this, we ſunk the bladder to the bottom of a wide mouthed glaſs, full of water, that the ſurface of the liquor might be a good deal higher than the upper part of the bladder: this wide mouthed glaſs we included in a great receiver (whoſe orifice muſt be very large to be able to admit ſuch a veſſel) which I cauſed to be carefully cemented on to the engine. The main ſcope of this experiment was to ſhew, that though the air, included in the bladder, was very far from being able, by its abſolute levity, to lift up ſo great a weight, as the bladder was clogged with, yet the ſame air, continually included in the bladder, would, by its meer expanſion, without any new external heat, acquire a power of aſcending in ſpite of that weight; which aſcenſion therefore muſt be attributed to the water, which, according to the laws hydroſtatical, ought, *cæteris paribus*, to reſiſt, or buoy up more potently thoſe immerſed bodies, that being lighter in ſpecie, than it, poſſeſs the greateſt place in it, and hinder the more water from acquiring its due ſituation: as we ſee, that among hollow ſpheres of glaſs and metal, equally thick and well ſtopped, there is a much heavier weight requiſite to ſink a large one than a ſmall one. For the proſecution of this trial, we began to pump the air out of the great receiver; and its preſſure upon the ſurface of the water being thereby more and more leſſened, (according to what we elſewhere more fully declare) the ſpring of the included air began by degrees to diſtend the ſides of the bladder, till at length that veſſel of air ſwelling every way, took up ſo much more room in the water than it did before, that the water was able to lift the bladder and the annexed weight to the top, and detain it there, till we thought fit to let in again ſome of the excluded air, which forcing that in the bladder to ſhrink in its dimenſions, the weight was preſently able to ſink it to the bottom.

AND here it may be noted, that if, inſtead of hanging ſo great a weight at the neck of the bladder, we faſtened but a moderately heavy piece of lead, ſuch as would only ſerve to ſink the bladder, and keep it at the bottom of the water, ſo that the aggregate of the bladder, air, and metal, was but a little heavier than a bulk of water equal to them: then, upon the firſt ſuck or operation of the pump, which could withdraw but a ſmall part of the air in the receiver, the air in the bladder ſuddenly expanding itſelf, would forthwith be impetuouſly extruded by the water, though after ſome reciprocations it would float in its due poſition, till upon the return of a little outward air, ſometimes as little as we could conveniently let in, it would immediately ſubſide.

BUT this is not ſo neceſſary to be inſiſted on, as it is to take notice, that I foreſaw it may be objected, that the aſcenſion of the weight was not effected by the preſſure of the water, but by this, that rarity and levity being qualities exceedingly of kin, the great rarefaction of the air might proportionably increaſe the levity of it, and conſequently enable it to perform much greater things than it could do before.

I will not here diſpute, whether, generally ſpeaking, a body rarified without heat, would, in vacuo, or in a fluid not heavier in ſpecie than the body when rarified, meerly, by ſuch a greater diſtance of its parts as may ſuffice to entitle it to rarefaction, become really heavier or lighter than before. I will not, I ſay, diſcuſs this queſtion here, where it may ſerve my turn to ſatisfy the recited objection by the following experiment.

EXPERIMENT III.

ABOUT the neck of a conveniently ſhaped vial capable to hold ſome few ounces of water, I cauſed to be carefully tied the neck of a ſmall bladder, whence the air had been diligently expreſſed, ſo that the bladder, being very limber of itſelf, and probably made more ſo, as well as more impervious to air and water, by the fine oil we had cauſed it to be rubbed with, lay upon the orifice of the vial like a ſkin clapped together with many folds and wrinkles.

THIS done, we let down the vial into a conveniently ſhaped veſſel full of water, and the vial, being poiſed before-hand for that purpoſe, ſunk perpendicularly in the liquor, till the neck of the glaſs was partly above and partly beneath the ſurface of the water: then covering the external glaſs with a large receiver, we cauſed the air to be pumped out, and as the preſſure of that was gradually withdrawn, the air in the floating vial did little by little expand itſelf into the bladder, and unfolded the wrinkles of it, till at length it became full blown, without altering the erected poſture of the glaſs it leaned upon. But this great expanſion, being made above the water, and conſequently in a medium not heavier than the included air, gave that highly rarified air no ſuch increaſe of levity, as enable us to perceive, that it made ſo much as the neck of the glaſs ariſe higher in the water, than it did before. Nor did we take notice, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarified eſtate, made the glaſs ſink deeper than before. But when the experiment was tried with the ſame glaſs and bladder, at the bottom of the water, then, upon the pumping out the air, the bladder being dilated under water, was after a while carried up to the top, and took up with it about eight or ten ounces, that had been, to clog it, faſtened to the bottom of the vial.

NEW EXPERIMENTS
About the PRESSURE of the
AIR'S SPRING
ON BODIES UNDER WATER.

I Do not think it were difficult for an intelligent peruſer of our phyſico-mechanical experiments, to find there divers phænomena, whence it may be deduced, that bodies under water, though kept by that liquor from the immediate contact of the air, may yet be expoſed to its preſſure, whether the air act as having a weight, or as a ſpring. But becauſe not only the vulgar, but philoſophers, have been ſo long and generally poſſeſſed with an opinion, that a fluid ſo little heavy as the air, cannot by its weight act upon a liquor, that is, like water, bulk for bulk, a thouſand times heavier than it: and becauſe alſo it ſeems yet more ſtrange, that a little air, perhaps not amounting to a ſcruple or drachm in weight, ſhould in its ordinary ſtate of laxity act conſiderably upon bodies, which, being covered with water, ſeem, by the interpoſition of that liquor, to be fenced from the incumbent air; it may be worth while to add three or four hydroſtatical experiments, to confirm a truth, that very few are yet acquainted with; and add to the proofs, already given of the power of the ſpring of the air, ſome of the operations we have diſcovered it to have upon bodies placed under water.

There are two ſorts of trials, that I ſhall employ to ſhew, that a ſmall quantity of encloſed air may, by its preſſure, (which in our caſes muſt depend upon its ſpring) have a conſiderable operation upon bodies under water, notwithſtanding the interpoſition of that liquor.

For, this preſſure we ſpeak of, may be manifeſted, in the firſt place, by what it directly and poſitively operates upon bodies covered with water: and, in the next place, by the things, that regularly enſue upon the removal of the encloſed air, or the weakning of its ſpring.

EXPERIMENT I.

To begin with the former way of ſhewing the preſſure of the air, I thought it ſufficient, in regard of the trials to be referred to the ſecond way, to make the following experiment.

We took a ſquare glaſs-vial, gueſſed to be capable of holding between half a pint and a pint of water; the neck of this we luted on carefully and ſtrongly (for elſe it would have been buoyed up) over the orifice of the ſmall pipe, at which the air paſſes in our engine out of the receiver into the pump: then whelming over this glaſs a great receiver, we luted it ſtrongly to the engine (that it might as well keep in the water as keep out the air) and at the top poured in as much water as ſufficed to inviron the internal receiver (if I may ſo call it) and cover it to a pretty height. This done, we exactly cloſed with a turning-key, the hole in the great receiver, at which the water had been poured in, that no air might get in or out that way. And laſtly, we began to pump out the air contained in the internal receiver; to the end that that air, which by the above-mentioned pipe had communication with the external air, might no longer by its preſſure aſſiſt the glaſs to reſiſt the preſſure, which the incumbent and encloſed air, by vertue of its ſpring, conſtantly exerciſes upon the ſubjacent water, and by its intervention upon the ſides and bottom of the internal receiver.

And as we expected not, that this glaſs by its own ſingle force, ſhould reſiſt the preſſure of the air encloſed in the upper part of the great receiver notwithſtanding the interpoſition of the water; ſo the event fully juſtified our conjecture: for at the firſt exſuction, which could not be ſuppoſed to have well emptied the internal glaſs, this veſſel was, by the preſſure of the ſuperior air upon the circumſtant water, broken into I know not how many pieces. And the ſame experiment, though with a little ſlower ſucceſs, was repeated with a ſtronger internal glaſs.

EXPERIMENT II.

I Proceed now to the ſecond way of manifeſting the preſſure of encloſed air upon bodies under water, which is by ſhewing the phænomena, exhibited by thoſe bodies upon the removal or leſſening of that preſſure.

Having ſqueezed out of a moderately ſized bladder the greateſt part of its air, we tied the neck of it very cloſe, and then faſtening to it a competent weight, we placed it at the bottom of the talleſt and largeſt glaſs we could cover with our great receiver, that ſo, though the incumbent air were pumped out, none of the water might be pumped out with it, but ſtill retain the ſame height above the bladder. Having then poured upon the bladder as much water, as would ſwim a great way above the upper part of it, we covered this glaſs of water with a great receiver, which being carefully cemented on to the engine, the pump was ſet a work, and as the air, which by its ſpring preſſed upon the ſurface of the included water, was by degrees pumped out, ſo the air, that was impriſoned in the bladder, did gradually expand it ſelf at the bottom of the water, as if no ſuch liquor had interpoſed between them otherwiſe than by its weight, upon whoſe account it muſt be allowed to give ſome little impediment to the expanſion of the bladder, in proportion to the height it had above it.

The event of our experiment was ſuch as was expected, namely, that the immerſed bladder was at length full blown, by the dilatation of the air incloſed in it; and by its intumeſcence made a conſiderable part of the water run over by the ſides of the glaſs, that before contained it all. And when acceſs was given again to the external air, the internal being compreſſed, the bladder was preſently reduced to its wrinkled ſtate.

EXPERIMENT III.

WE took a ſmall but fine bladder, whoſe neck was ſtrongly tied up, when it was, by gueſs, about half full of air: this we put into a ſhort braſs cylinder, the lower of whoſe baſes was cloſed with a braſs-plate, and the other left open; this open orifice we afterwards ſtopped, but not exactly, with a cylindrical plugg, that was ſomewhat leſs wide than it, and was by a rim at the top hindered from reaching too deep into the cavity of the cylinder, that it might not do miſchief to the bladder, that lay there beneath it; upon this plugg we placed an almoſt conically ſhaped weight of lead, and this pile of ſeveral things being ſo placed upon our engine, that we could cover it with a great receiver, we carefully cemented on this veſſel, and at the top of it poured in ſo much water, as would ſerve to fill the vacant part of the braſs cylinder, and the cavity of the engine to ſuch a height, that it covered all the leaden weight, which was ſeveral inches high, except a rim, which was faſtened to the top of it for the convenienter removing of it.

All this being done, the pump was ſet to work, and long before we had exhauſted the air of the receiver, that, which was incloſed in the lank bladder, had by degrees, diſplayed ſo vigorous a ſpring, that it had heaved up the weight, that lay upon it, to a notable height, and kept it there, till the air was let in from without, to aſſiſt its being depreſſed by the leaden weight, which amounted to no leſs than about 28 pound.

EXPERIMENT IV.

THERE remained yet one trial to be made, which, in caſe it ſhould ſucceed, ſeemed likely to appear as great an evidence of the force of the air's ſpring upon bodies under water, as could be reaſonably deſired of us; it having been looked upon by many virtuoſi, as the conſiderableſt inſtance of the force of the air's ſpring, even when no water intervened in the trial.

To ſatisfy therefore our curioſity, we took a copper veſſel of a cylindrical ſhape, and a conſiderable height; into this, being firſt almoſt filled with water, we put a ſquare glaſs-vial, capable, by gueſs, to hold nine or ten ounces of water, and exactly ſtopped with a cork and a cloſe cement: this vial, by a competent weight, was detained at the bottom of the water, from whoſe upper ſurface it was conſiderably diſtant: then the copper veſſel being placed upon the engine, and included in a great receiver well cemented on, the air was by degrees pumped out; but before it was quite exhauſted, the glaſs at the bottom of the water was, by the ſpring of the air included in it, burſt into many pieces, not without great noiſe, and a kind of ſmoak or miſt, that appeared above the ſurface of the water.

Another glaſs of the ſame ſort had been broken after the ſame manner in another veſſel; but having afforded us no particular phænomenon, I barely mention it, to ſhew, that we made more than one trial of this kind.

The conſequence, that will naturally reſult from the three laſt experiments, is this, that ſince barely upon the withdrawing of the preſſure of the included air (which was perhaps but very little in quantity,) the air reſiding in the immerſed bodies, did, by virtue of its ſpring, expand itſelf ſo forcibly as we have recited, and perform notable things, the air above the water muſt have exerciſed a very powerful preſſure upon the ſurface of it, ſince (ſetting aſide the weight of the water, of ſmall moment in our trials,) it muſt have been, at leaſt, equivalent to (and probably much exceeded) that force of the immerſed air, whoſe exerciſe it was able totally to hinder.

And from hence it may be eaſily deduced, that the weight of the atmoſphere acts upon bodies under water, notwithſtanding that the interpoſed liquor is, by vaſt odds, heavier in ſpecie than air; for we have juſt now proved the preſſure of incloſed air, (which conſiſts in its ſpring,) upon bodies under water; and it is manifeſt, that the ſtrength of the ſpring of this inferior air, we make our trials with, is cauſed by the weight of the ſuperior air, which bends and compreſſes thoſe little aereal ſpringy particles, whereof our air conſiſts; ſo that the weight of the atmoſphere being equivalent to the ſpring of the inferior air, (for elſe it could not compreſs it as much as it does,) muſt lean upon the ſurface of the ſubjacent water, with a force equivalent to the ſpring of that part of it, that is contiguous to the water.

This experiment brings into my mind another, that I once made, which, though not properly hydroſtatical, yet relating to poſitive levity, may perhaps be not uſeleſly added on this occaſion: wherefore I ſhall here ſubjoin a tranſcript of the phænomenon, that belongs to our preſent purpoſe, as it is regiſtered ſoon after the experiment was made.

[To examine, by a viſible experiment, the common doctrine, that a portion of air, by being much dilated, rarified or expanded, does acquire a new and proportionable degree of poſitive levity, I deviſed to put in practice the following way:

We took a bladder of a moderate ſize, that was very fine and limber, that it might be the lighter and more eaſily diſtended. The moſt part of the air being ſqueezed out of the bladder, the neck of it was tied up very cloſe, that no air might get out of it, nor any external get into it. This limber bladder was hung at one of the ſcales of a balance, whoſe beam had been purpoſely made more than ordinarily ſhort, that the inſtrument, (which yet was tickliſh enough) might be ſuſpended, and capable of playing in the cavity of a great receiver, into which we conveyed it, having firſt carefully counterpoiſed the bladder with a metalline weight put into the oppoſite ſcale.

This done, the air was pumped out, and, as that was withdrawn, the bladder was more and more expanded by the ſpring of the internal air, till at length, when the receiver was well exhauſted, it appeared to be quite full. Notwithſtanding which great dilatation of the included air, it did not appear by the depreſſion of the oppoſite ſcale, to be grown manifeſtly lighter, than it was at firſt. And the bladder ſeemed alſo to retain the ſame weight, after it had, by the air, that was let into the receiver, been compreſſed into its former wrinkled ſtate.]

NEW EXPERIMENTS
ABOUT
THE DIFFERING PRESSURE
OF HEAVY
SOLIDS AND FLUIDS.

SINCE not only in vulgar ſpectators of phyſico-mechanical experiments, but even among ſome learned men, it has proved a great impediment to men's freely acquieſcing in the doctrine founded on thoſe phænomena, that if the atmoſphere could really exerciſe ſo great a preſſure, as we aſcribe to it, it would unavoidably oppreſs and cruſh all the bodies expoſed to it, and conſequently neither other animals, nor men, would be able to move under ſo great a load, or ſubſiſt in ſpite of ſo forcible a compreſſion.

This I readily grant to be a plauſible objection; but I ſuppoſe the force of it will be taken away by the following conſiderations put together.

And firſt, the power of preſſing, that we aſcribe to the air, is not a thing deduced, as too many other conſequences in phyſick are, from doubtful ſuppoſitions or bare hypotheſes, but from real and ſenſible experiments. And therefore ſince we have clear and poſitive proofs of the preſſure of the air, though we could not explain, how men and other animals are not deſtroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifeſts to be a truth: as is generally practiſed in treating of the attractive and other powers of the loadſtone, which are freely acknowledged, even by thoſe, that confeſs themſelves unable to explicate them; though, if experience did not ſatisfy us of them, they were liable to divers more conſiderable objections, than any, that is urged againſt the preſſure of the air.

Secondly, but though it be not abſolutely neceſſary, that we ſhould anſwer the above-recited objection otherwiſe, than by thus declaring, that the ſpring of the air is not to be rejected for it; yet we will endeavour very much to leſſen it, if not quite remove the difficulty, before we put an end to the diſcourſe.

I conſider then thirdly, that they, that urge the lately mentioned objection againſt the great preſſure of the air, ſeem not to be aware, that we were conceived and born in places expoſed to the preſſure of the atmoſphere, and therefore how great ſoever that preſſure appeared to be, it ought not to cruſh us now, ſince when we were but embrios, or new-born babes, and conſequently very much more weak and tender than we now are, we were able to reſiſt it, and not only live, but grow in all dimenſions in ſpite of it.

If there were any place about the moon, or ſome other of the cœleſtial globes, that ſome learned men fancy to be inhabited, that has no atmoſphere, or equivalent fluid about it, and where yet men could be generated a-new, if one of thoſe men ſhould be ſuppoſed to be tranſported thence, and ſet down upon our earth, there might be made an experiment fitted for our controverſy. In the mean time, I doubt, that ſince nature is not obſerved to make things ſuperfluouſly ſtrong, ſuch a human body being not made to reſiſt any weight or preſſure of air, would be of ſo tender and compreſſible a make, that it would eaſily be cruſhed inwards by our atmoſpherical preſſure. And though we cannot give an inſtance of this kind, yet we make trials ſomewhat analogous to it in our pneumatical engine. For when we place water in our receiver, and pump out the air, that was above it, there will be generated a multitude of bubbles, ſome of which, when the air is carefully withdrawn, will be of a ſtrange and ſcarce credible bigneſs; theſe bubbles being generated where the air cannot preſs upon them, theſe dimenſions are ſo natural to them, that if the receiver be ſuppoſed not to leak, nor other unfriendly accidents to intervene, they would (for aught we know) laſt a good while; ſince I have elſewhere ſhewn, that the ſpring of highly dilated air did continue for many months, and a bladder would for no leſs time continue blown and filled in our vacuum by a little air, that was left in it, when the ambient air began to be withdrawn from it. And yet the large bubbles abovementioned, when once the outward air is ſuffered to come in upon them, are thereby ſo violently compreſſed, that in a trice they ſhrink into dimenſions, too ſmall to keep them ſo much as viſible; and if I could have ſucceeded in my attempt of producing ſuch living bodies as I endeavoured (but did not expect) in our vacuum, I ſuppoſe the ſucceſs would have confirmed what I have been ſaying.

FOURTHLY, but you will tell me, that ſo great a weight and preſſure, as I aſſign the atmoſphere, muſt needs make a man feel pain, and, if not otherwiſe diſlocate ſome of the parts, muſt, at leaſt, preſs the whole body inward.

BUT firſt, being accuſtomed to the preſſure from our very birth, and even before it, ſo early and long an accuſtomance hinders us from taking notice of it; thoſe preſſures only being ſenſible to us, that are made ſo by ſome additional cauſe, which by making a new impreſſion excites us to take notice of it. So we are not ſenſible of the weight of the clothes we are accuſtomed to wear; and ſo a healthy man is not ſenſible of the heat in his heart, becauſe it is conſtant there, and the ſentient parts of the heart have been ſtill uſed to it, whereas that heat oftentimes has been very conſiderable; and when in living diſſections a man puts his finger into the heart of an animal, which probably has a fainter, or at leaſt no ſtronger degree of heat than a human heart, he will feel in his fingers, accuſtomed to the air, a manifeſt degree of heat, if they be but in their uſual temper. 2. I have elſewhere proved by experiments, that a cubick inch of air, for inſtance, has as ſtrong a ſpring, as ſuffices to enable it to reſiſt the weight of the whole atmoſphere, as far as it is expoſed thereunto; for elſe it would be more compreſſed than *de facto* it is. And 3. I have alſo ſhewn, that a very little portion of air, though it will much ſooner looſe its ſpring by expanſion than a greater, yet it will reſiſt further compreſſion as much as a greater. And 4. I have alſo ſhewn, that in the pores of the parts of animals, whether fluid or conſiſtent, as in their blood, galls, urines, hearts, livers, &c. there are included a multitude of aereal corpuſcles, as may appear by the numerous bubbles afforded by ſuch liquors, and the ſwelling or expanſion of the conſiſtent parts in our exhauſted receiver. 5. To this we may add, that, beſides the bones, whoſe ſolidity is not queſtioned, a much greater part of the human body, than is wont to be imagined, does really conſiſt of membranes and fibres, and the coalitions and contextures of theſe; and that theſe ſubſtances are, by the providence of the moſt wiſe author of things, made of a much cloſer and ſtronger texture, than thoſe, that have not tried, will be apt to think; as I could make probable by the great force that bladders will endure, and the very great weight, that tendons of no great thickneſs will lift up or ſuſtain, and by other things, that I ſhall not now inſiſt on. Laſtly, there is a far greater difference, than men are wont to ſuſpect, between the effects of the preſſures made upon bodies by incumbent, or otherwiſe applied ſolid weights, and thoſe, that they ſuffer from heavy, but every way ambient fluids; as will appear by the experiments to be mentioned by and by.

FROM the particulars contained in theſe conſiderations, we may be aſſiſted to ſhew, why it is not neceſſary, that the preſſure of the atmoſphere, though as great as we ſuppoſe it, ſhould oppreſs and cruſh the bodies of men, that live under it: for, the ſolidity of the bones, and the ſtrong texture of the membranes and fibres, and the ſpring of the aereal particles, that abound in the ſofter, as well as in the fluid parts of bodies, is equivalent to the preſſure of as much of the atmoſphere, as can exerciſe its preſſure againſt them, and makes the frame of a human body ſo firm, that it may well reſiſt the preſſure of the outward air, without having any part violently diſlocated, whilſt the external preſſure is exerciſed but by the air, which being but an invironing fluid, preſſes it equally (as to ſenſe) on every ſide. And becauſe our bodies have been produced in the atmoſphere, and from our very birth expoſed, without intermiſſion, to the preſſure of it; our continual accuſtomance to this preſſure, and the firmneſs of their ſtructure, keep us from being ſenſible of the weight or preſſure. And that it was not impertinent for me to mention the firmneſs of the frame of our bodies on this occaſion, I ſhall manifeſt by an inſtance, that will upon another account alſo be proper for this place.

WE know, that multitudes of men have had occaſion to paſs over high mountains; and beſides, that I have been myſelf upon the *Alps* and *Appennines*, I have enquired of travellers, that have viſited the Aſian and American mountains, and ſome, that have been upon the top of the pick of *Tenerif* itſelf: but though divers of them took notice of a great difference in the air at the top and bottom, as to ſome other quality, as coldneſs and thinneſs; yet I never met with, nor heard of any, that took notice of a difference, as to the weight of air he ſuſtained, or that complained, that when he was come down to the foot of the mountain, he felt any greater compreſſion from the air, than at the top. And yet the experiments made, as well by others as by ourſelves, ſufficiently witneſs, that on more elevated parts of the earth, which have a leſs height of the atmoſphere incumbent on them, the weight and preſſure of the air is not ſo great as below. And on very high mountains, it is not unlikely, that this difference may be very conſiderable, ſince, when the Torricellian experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, (which is none of the higheſt mountains in the world, being found, by the ingenious makers of that obſervation, to be but about 500 fathoms,) they found the difference of the mercury, at the top and bottom, to amount to about three inches: and conſequently, if the trial had been made with water inſtead of quickſilver, the difference would have been about three foot and a half in the perpendicular height of the water. And it is very probable, that in much higher mountains, the difference of the mercurial cylinders height, at the top and bottom, may be much greater; and at the bottom of ſome very deep well or mineral groove, which may, without improbability, be ſuppoſed to be placed at, or near the foot of one of theſe mountains, if we conceive the baroſcope to be let down, the variation of the height of the mercurial cylinder may be yet much more conſiderable; and yet we find not, that the diggers in the deepeſt mines, in mountainous countries, are ſenſible of being leaned on or compreſſed by any unuſual weight. But not here to build on any thing but matter of fact, it appears by the newly-named obſervation, that, when a man was at the bottom of the hill, he had as much greater weight of air leaning upon his head, than he had at the top, as was equal to the height of an imaginary veſſel full of water, which having his head for baſis, were three foot and a half high: which is ſo conſiderable a weight, as could not but have been, not only ſenſible, but very troubleſome and uneaſy to ſupport. And what has been ſaid of the gravity of a pail of water, that leaned on his head, may be proportionably applied to his ſhoulders, arms, &c.

Whence I think I may infer, that the reaſon, why ſuch a weight was not felt by the man it compreſſed, was not, that the air, that preſſed him, was not conſiderable, but that the preſſure was exerciſed after the uniform manner of fluid bodies.

And this may ſuffice to ſhew, that there is no neceſſity, that the compreſſion of the atmoſphere ſhould make it impoſſible to live in it. But becauſe 'tis obſerved, that thoſe, that dive to great depths under water, are not oppreſſed by the great weight of the incumbent water, and the cauſe of this ſtrange phænomenon is not ſo eaſy to be aſſigned, and therefore has been made one of the two grand arguments, whereon the non-gravitation of water in water, and air in air, has been, and ſtill remains founded: I ſhall here offer ſomething *ex abundanti* towards the ſolution of that noble and difficult problem.

And firſt, that what is obſerved by the divers, does not evince, that water does not weigh in water, I have elſewhere * proved by ſuch reaſons and experiments, as had the good fortune to convince eminently learned men, that were ſufficiently prepoſſeſſed with the vulgar opinion: and in the ſame treatiſe I have given a clear account, why a bucket full of water is not felt conſiderably heavy, whilſt it is under water, in compariſon of what it is whilſt it is drawn up into the air; which is the other phænomenon, that I freſhly intimated the common opinion to be founded on.

Next, I do not think it ſtrange, that that follows not, which it is objected ſhould follow from our hypotheſis; namely, that a diver ſhould be violently depreſſed to the bottom of the water, by the weight of ſo great a pillar of the ſea as is placed perpendicularly over his body, For if we imagine a plane ſo to cut the ſea-water, as to paſs by the diver's body; then as that part of the plane, on which his body leans, will be preſſed by it, together with the water, that is perpendicularly incumbent on it; ſo all the other parts of the ſame plane will be preſſed by equally tall pillars of water perpendicularly incumbent on them; and conſequently, if the man's body were juſt equiponderant to an equal bulk of water, it and the water, that leans on it, would be ſuſtained by the preſſure of the collateral water incumbent on the other parts of the ſame plane (as may be eaſily underſtood by what I have elſewhere † ſaid.) And therefore there is no reaſon, why the divers bodies ſhould be more forcibly depreſſed than its depreſſion is reſiſted. It is true, that this body will ſink, but that is becauſe it is not only, as we lately ſuppoſed it, æquiponderant to an equal bulk of water, but heavier than that. But then, ſince the water, by its gravity and reſiſtance, takes off as much of the weight of the diver's body, whilſt that is immerſed, as a quantity of water equal to it, would weigh in the air, the ſubſiding of the human body by its own weight ought to be but ſlow, becauſe that being not in ſpecie much heavier than water, it can ſink but by virtue of the ſurpluſage of weight, that it has above water. And, in effect, I have been informed by ſwimmers, that in the ſea, whoſe water, by reaſon of the ſaltneſs, is ſpecifically heavier than the common water, they could hardly dive when they had a mind; the ſalt-water did ſo much ſupport them. And having, becauſe I had no conveniencies to make trials upon the parts of human bodies, examined the weight of parts of other animals in air and water, I found the overplus of the weight of the animal ſubſtance sabove an equal bulk of water to be but very ſmall. And this may ſuffice to take off the wonder, why, though water may be admitted to gravitate in water, yet divers are not depreſſed by that, which leans upon them; the endeavour, they uſe to keep themſelves from ſinking, by ſtriking the reſiſting water with their arms and legs, eaſily compenſating their weak tendency downwards, which the ſmall ſurpluſage of gravity above-mentioned gives them.

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* See the Hydroſtatical Paradoxes.

† See appendixes to the Hydroſtatical Paradoxes.

BUT it ſeems to me far more difficult to render a reaſon, why thoſe, that are a hundred foot beneath the ſurface of the ſea, are not cruſhed inwards, eſpecially in their cheſts and abdomens, or at leaſt ſo compreſſed as to endure a very great pain.

To clear up or leſſen this difficulty, I have two things to offer.

1. I confeſs, that I am not entirely ſatisfied about the matter of fact; for I do not yet know, whether it fares alike with the divers in all depths under water: for, according to the anſwers I obtained from perſons, that had been, one of them at the coral-fiſhing in the *Streights*, and the other at the pearl-fiſhing near *Manar*, I do not find, that the divers are wont to deſcend to the greateſt depths of the ſea, which if they did, perhaps they would find a notable difference.

AND in ſmall, or but moderate depths, thoſe, that dive without engines, uſually make ſuch haſte, or are ſo confounded, or have their minds ſo intent upon their work, that they take not notice of ſuch leſſer alterations, as elſe they might obſerve, eſpecially they being perſons void of curioſity and ſkill to make ſuch obſervations. Which I the rather mention, becauſe having met with a learned phyſician, that living by the ſea-ſide in a hot climate, delighted himſelf much in diving; and enquiring of him, whether he felt no compreſſion, when he paſſed out of the air into the water, he anſwered me, that when he dived nimbly as others uſe to do, he took not notice of it, but when he let himſelf ſink leiſurely into the water, he was ſenſible of an unuſual preſſure againſt his thorax, which he ſeveral times obſerved.

A man, that gets his living by fetching up goods out of wrecked ſhips, complained to me, that if with his diving-bell he went very deep into the ſea, and made ſome ſtay there, he found himſelf much incommodated; which though he imputed to the coldneſs of the water, yet by the ſymptoms he related, I was inclined to ſuſpect, that the preſſure of it upon the genus nervoſum might have an intereſt in the troubleſome effect. And I have been aſſured by an eminent virtuoſo of my acquaintance, that he was lately informed by a perſon, whoſe profeſſion it is to fetch up things from the bottom of the ſea by the help of a diving-bell, that ſeveral times, when he deſcended to a great depth under the ſurface of the water, he was ſo compreſſed by it, that the blood was ſqueezed out at his noſe and eyes; which relation ſeems to favour our conjecture, and would much more confirm it, if I were ſure, that the effect was no way cauſed by ſome fermentation, or other commotion in the blood itſelf, occaſioned by the great denſity, or other alterations of the air he breathed in and out, or by ſome other operation of the ambient medium diſtinguiſhable from the compreſſion of the water, though perhaps conjoined with it.

AND on this occaſion I remember, that queſtioning an engineer, who had made uſe of an engine to go under water, quite differing from the diving-bell; he anſwered me, that when he came to a conſiderable depth, he found the preſſure ſo great againſt the leathern caſe, wherein he deſcended, and by that means againſt his belly and thorax, that he feared it would have ſpoiled him, which forced him to make haſte up again. But this obſervation, to have much built upon it, ſhould be further enquired into.

THESE things, and not theſe only, make me wiſh, that what is felt by thoſe, that dive to great depths, and ſtay at them, might be more heedfully obſerved by intelligent men, that being fully informed, what is true in point of fact, we may the better and more chearfully indagate the reaſons.

IN the mean while, taking things as they are thought to appear, I ſhall propoſe two things towards the ſolution of our difficulty; namely, the firmneſs of the ſtructure of a human body, and the uniformity of the preſſure made by fluids.

OF the firſt of theſe I ſhall add but little to what has been already ſaid, where I ſpoke of the reſiſtance made by our bodies to the compreſſion of the atmoſphere; only ſhall here take notice, that whereas the membranes are very thin parts, and therefore ſeem unfit to make any great reſiſtance; we have tried, that if a piece of fine bladder were faſtened to the orifice of a braſs-pipe, of about an inch in diameter, we could not, by drawing the air from beneath it, make the weight of the atmoſphere break the bladder, though the weight were perhaps equivalent to an erected cylinder of water, of the wideneſs of the orifice, and about thirty foot high, and were indeed ſuch, that divers men, that laid their hands on the orifice, when the air was pumped out from beneath, complained, that they were not able to lift off their hands again, till ſome of the air was re-admitted.

BUT the main thing I ſhall propoſe, towards the ſolving of the difficulty we are conſidering, is the uniformity, wherewith fluid bodies preſs upon the ſolid ones, that are placed in them. And becauſe I remember not to have met with experiments purpoſely made to ſhew, how this ſort of preſſure is more eaſy to be reſiſted, than that of ſolids againſt ſolids, I ſhall ſubjoin the following trials.

EXPERIMENT I.

IN the ſhort cylinder of braſs above-mentioned, we put a fine bladder tied ſo cloſe at the neck, that none of the air (whereof it was about half full) could probably get out. Which we did, to the end, that the hen-egg, we were to bed in it, might lie ſoft, and have its ſides almoſt covered with the limber and flaccid bladder and contained air: this done, we covered the remaining part of the egg with another bladder, that nothing, that was hard, might come to bear immediately upon the ſhell: then we put the wooden plug into the cylinder, and a weight upon the plug, which is to be done very ſlowly and warily, leſt the quick deſcent of the weight ſhould make the plug break the egg it leans on. Laſtly, the cylinder thus fitted, being covered with a large receiver, and the air being drawn out, that air, which was tied up in the bladders, by degrees expanded itſelf ſo ſtrongly, as to lift up the plug and the incumbent weight to a pretty height, and keep it there, till the external air was re-admitted.

Now ſince it will be readily granted, and appears by divers experiments, elſewhere related, that the air in ſuch caſes expands itſelf vigorouſly every way, it appears by the recited trial, that it preſſed againſt the egg with the ſame force, that it preſſed proportionably againſt the bottom of the plug, and that force was more than ſufficient to lift up the weight, which (together with the plug) amounted to about thirty pound, and yet the egg being taken out, appeared perfectly whole and no way harmed; whereas, upon the ſame egg, if I miſtake not, or at leaſt another of the ſame kind, laying warily a while after ſmall weights, one upon another, the egg was cruſhed to pieces by about four pound weight. This experiment, though it ſeemed conſiderable to thoſe that ſaw it, and may prevent an objection, for which reaſon I here mention it; yet will appear in no way ſtrange to them, that conſider, that the weight of the atmoſphere, which the egg ſupported, before it was put into the cylinder, was more than æquivalent to ſuch a preſſure of the air, as may ſuffice to lift up the plug: wherefore I thought fit to make further trials of a differing nature.

EXPERIMENT II.

WE took a glaſs bubble of about an inch and half in diameter, which we cauſed to be blown at the flame of a lamp, that it might be far more thin and eaſy to break, than the thinneſt vials, that are wont to be blown in the glaſſer's furnaces. This bubble we included between bladders, as we did the egg in the former experiment; and then having warily put the plug into the cylinder, ſo as it might preſs upon the bladder that invironed the glaſs, we leiſurely put the weights upon the plug, till they, together with the plug, amounted to thirty pound, or more, which being removed, the plug was taken out, and the glaſs-bubble, though it were extraordinarily thin, perhaps no thinner than fine white paper, was taken out whole.

EXPERIMENT III.

BUT leſt the great reſiſtance of ſo thin a glaſs, which yet was not hermetically ſealed, ſhould be aſcribed to the ſphericalneſs of its figure, we employed, inſtead of it, the ſhell of an egg, whence by a hole, made at one end of it, the yolk and white had been taken out. This empty and imperfectly cloſed ſhell we handled, as we did the glaſs-bubble in the former experiment; and, notwithſtanding the great leaden weight, that leaned by the intervention of the plug upon the ſoft body, that invironed it, it was taken out, not only uncruſhed together, but, for aught we could perceive, without the leaſt crack.

EXPERIMENT IV.

AND to ſhew, that what we obſerved about the nature of the compreſſion of fluid bodies will hold as well in water as air, though it ſeemed difficult to make the trial with the accommodations we then had, we thought upon the following expedient.

INTO a limber bladder, almoſt full of water, we put a hen egg, and tying the neck very ſtrait, that nothing might get in or out, we ſo placed the bladder in the braſs cylinder, that the egg might not be immediately touched by any thing, that was hard: then putting the plug into the cylinder, we warily and leiſurely heaped upon it flat-bottomed weights of lead conveniently ſhaped, till they amounted (if both I and another miſremember not) to about ſeventy-five pound; notwithſtanding all which, the egg was taken out ſound and uncracked; and probably might have ſupported a much greater preſſure, if we had been furniſhed with more weights of a commodious figure to heap upon it.

IF we compare with this what was noted at the cloſe of the firſt experiment, about the breaking of an egg with four pound weight, when no fluid body was interpoſed, it will be obvious to conclude, how great a difference there is between the reſiſtance, that a body may make to the preſſure of ſolid bodies, that bear hard againſt ſome parts, and not againſt others; and its reſiſtance to others, that compreſs it uniformly, or in all places alike. For though it be denied, and that, I think, upon very inſufficient grounds, that bodies under water are preſſed by the incumbent water, becauſe, as it is pretended, the elements gravitate not in their proper place; yet this objection cannot be pretended to take place in our laſt experiment, where the main thing, that leaned upon the water, which ſurrounded the egg, being not a pillar of homogeneous water, but a great and ſolid weight of lead, the included egg muſt, by the intervention of the water, have been compreſſed. Nor were eggs the only bodies we endeavoured to cruſh after this manner, the trial having been alſo made upon a ſubſtance more ſoft, and of a very irregular ſhape.

To apply this now to divers, when they are at a moderate depth under water; it ſeems not improbable, that the ſtructure of their bodies ſhould be robuſt enough, not to be violated by the preſſure of the incumbent, and otherwiſe ambient water. For we have ſeen by the former experiment, and eſpecially by the laſt recited

cited, that a body, eaſy to be broken inwards by an incumbent ſolid weight, will remain entire, and unaltered in point of figure, under a very much greater weight, that compreſſes it after the manner of an ambient fluid. And though it would ſeem to many, that even in our ſuppoſition, the thorax being, as they think it, a kind of empty ſpace in the body, the ribs and muſcles ought, by the weight of the water, to be cruſhed into the great cavity intercepted between them; yet it is to be conſidered on the other ſide, that the air contained in the cheſt, eſpecially when its ſpring is increaſed by thoſe accidental cauſes, that may take place, when men are deep under water, particularly the preternatural heat, which the want of the uſual reſpiration is apt to produce, will very much help the cheſt to reſiſt the preſſure, as they will eaſily grant, that have tried the reſiſtance, that air makes, to be conſiderably compreſſed under water, the difficulty of farther compreſſing it ſtill encreaſing, as in ſprings it ought to do, the more it is compreſſed. And I further obſerve, that the ſtructure of the thorax is much more firm, than men are wont to ſuppoſe; as appears by the very great ſolid weights, that ſome men do, for gain, or to ſhew their ſtrength, ſuffer to be laid on their breaſts, without receiving any miſchief thereby. And if I ſhould admit, that at great depths the water had ſome little compreſſive operation upon the cheſt; yet that can be no other than the preſſing the parts a little inwards, and that the ſtructure of the thorax itſelf, fitted by nature for conſtriction and dilatation (as may appear in vehement takings in and blowings out of the air) may admit with ſmall inconvenience. To which purpoſe I recall to mind, what I lately mentioned concerning the phyſician, that found his thorax ſomewhat compreſſed when he leiſurely dived; as alſo what I have * elſewhere delivered concerning a tad-pole, which ſwimming in water, that was ſtrongly compreſſed by an external force, ſeemed thorough the glaſs, that contained the water, to be ſomewhat leſſened in bulk, and yet not killed, nor ſenſibly cruſhed, notwithſtanding its great tenderneſs. And if there were parts of a human body, that were of a texture too weak, and too diſproportionate to the reſt, I think it poſſible, that this compreſſion inwards might be great enough to be very ſenſible to the divers. For having purpoſely enquired of a certain man, whoſe trade it was to fetch up goods out of ſhips caſt away, by the help of a diving inſtrument, he told me, that when he was at a conſiderable depth under water, as about ten or twelve fathoms, he found, ſuitably to my conjecture, ſo great a preſſure againſt the drums or thin membranes of his ears, which were not ſufficiently counterpreſſed from within, as put him to a great deal of pain, till he had found ſome contrivances to leſſen the inconvenience. Nor was this man the only diver, that has complained of this troubleſome preſſure, which ſeems to argue, that, at leaſt at great depths under water, the firmneſs of the ſtructure of a man's body does concur with the uniformity of the fluid's preſſure, to keep him from being hurt by the incumbent, and otherwiſe ambient air.

BUT I ſhall now ſay no more of the problem about divers, ſince (beſides that the matter of fact is not yet, in my opinion, accurately enough ſtated and determined) the true ſolution of it is not neceſſary to give a reaſon, why the weight of the air, a fluid ſo much lighter than water, ſhould not oppreſs nor cruſh the bodies of animals; though what has been already ſaid, about the reſiſtance of bodies under water, may ſerve very much to confirm the reaſons I propoſed, why we, that live in the atmoſphere, are not ſenſibly compreſſed, much leſs oppreſſed by its weight.

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* In the Appendix to Hydroſt. Paradox.

SOME OBSERVATIONS
ABOUT
SHINING FLESH,
Both of VEAL and of PULLET,
AND THAT
Without any ſenſible PUTREFACTION in thoſe Bodies.

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YESTERDAY, when I was about to go to bed, an amanuenſis of mine, accuſtomed to make obſervations, informed me, that one of the ſervants of the houſe, going upon ſome occaſion into the larder, was frighted by ſomething of luminous, that ſhe ſaw (notwithſtanding the darkneſs of the place,) where the meat had been hung up before. Whereupon, ſuſpending for a-while my going to reſt, I preſently ſent for the meat into my chamber, and cauſed it to be placed in a corner of the room capable of being made conſiderably dark, and then I plainly ſaw, both with wonder and delight, that the joint of meat did, in divers places, ſhine like rotten wood or ſtinking fiſh; which was ſo uncommon a ſight, that I had preſently thoughts of inviting you to be a ſharer in the pleaſure of it. But the late hour of the night did not only make me fear to give you too unſeaſonable a trouble, but being joined with a great cold, I had got that day by making trial of a new teleſcope, you ſaw, in a windy place, I durſt not ſit up long enough to make all the trials, that I thought of, and judged the occaſion worthy of. But yet, becauſe I effectually reſolved to employ the little time I had to ſpare, in making ſuch obſervations and trials, as the accommodations I could procure at ſo inconvenient an hour would enable me, I ſhall here give you a brief account of the chief circumſtances and phænomena, that I had opportunity to take notice of.

1. THEN I muſt tell you, that the ſubject, we diſcourſe of, was a neck of veal, which, as I learned by enquiry, had been bought of a country-butcher on the tueſday preceding.

2. IN this one piece of meat I reckoned diſtinctly above twenty ſeveral places, that did all of them ſhine, though not all of them alike, ſome them of doing it but very faintly.

3. THE bigneſs of theſe lucid parts was differing enough, ſome of them being as big as the nail of a man's middle finger, ſome few bigger, and moſt of them leſs. Nor were their figures at all more uniform, ſome being inclined to a round, others almoſt oval, but the greateſt part of them very irregalarly ſhaped.

4. THE parts, that ſhone moſt, which it was not ſo eaſy to determine in the dark, were ſome griſly or ſoft parts of the bones, where the butcher's cleaver had paſſed; but theſe were not the only parts, that were luminous; for by drawing to and fro the medulla ſpinalis, we found, that a part of that alſo did not ſhine ill: and I perceived one place in a tendon to afford ſome light; and laſtly, three or four ſpots in the fleſhy parts, at a good diſtance from the bones, were plainly diſcovered by their own light, though that were fainter than in the parts above mentioned.

5. WHEN all theſe lucid parts were ſurveyed together, they made a very ſplendid ſhew; but it was not ſo eaſy, becauſe of the moiſtneſs and groſſneſs of the lump of matter, to examine the degree of their luminouſneſs, as it is to eſtimate that of gloworms, which being ſmall and dry bodies, may be conveniently laid in a book, and made to move from one letter or word to another. But by good fortune having by me the curious tranſactions of this month, I was able ſo to apply that flexible paper to ſome of the more reſplendent ſpots, that I could plainly read divers conſecutive letters of the title.

6. THE colour, that accompanied the light, was not in all the ſame, but in thoſe, which ſhone livelieſt, it ſeemed to have ſuch a fine greeniſh blue, as I have divers times obſerved in the tails of gloworms.

7. BUT notwithſtanding the vivid neſs of this light, I could not, by the touch, diſcern the leaſt degree of heat in the parts, whence it proceeded; and having put ſome marks on one or two of the more ſhining places, that I

might know them again, when brought to the light, I applied a ſealed weather-glaſs, furniſhed with tincted ſpirit of wine, for a pretty while, and could not ſatisfy myſelf, that the ſhining parts did at all ſenſibly warm the liquor: but the thermoſcope, though good in its kind, being not fitted for ſuch nice experiments, I did not build much upon that trial.

8. NOTWITHSTANDING the great number of lucid parts in this neck of veal, yet neither I, nor any of thoſe, that were about me, could perceive, by the ſmell, the leaſt degree of ſtink, whence to infer any putrefaction; the meat being judged very freſh, and well-conditioned, and fit to be dreſſed.

9. THE floor of the larder, where this meat was kept, is almoſt a ſtory lower than the level of the ſtreet, and it is divided from the kitchen but by a partition of boards, and is furniſhed but with one window, which is not great, and looks towards the ſtreet, which lies northward from it.

10. THE wind, as far as we could obſerve it, was then at ſouth-weſt, and bluſtering enough. The air, by the ſealed thermoſcope, appeared hot for the ſeaſon. The moon was paſſed its laſt quarter. The mercury in the barometer ſtood at 29$\frac{3}{16}$ inches.

11. WE cut off, with a knife one of the luminous parts, which proved to be a tender bone, and being of about the thickneſs of a half crown piece, appeared to ſhine on both ſides, though not equally; and that part of the bone, whence this had been cut off, continued joined to the reſt of the neck of veal, and was ſeen to ſhine, but nothing near ſo vividly as the part we had taken off, did before.

12. TO try, whether I could obtain any juice, or moiſt ſubſtance from this, as I have ſeveral times done from the tails of glow-worms; I rubbed ſome of the ſofter and more lucid parts, (which I cauſed to be purpoſely cut off) as dexterouſly as I could, upon my hand, but I did not at all perceive any luminous moiſture was thereby imparted; though the fleſh ſeemed, by that operation, to have loſt ſome of its light.

13. I cauſed alſo a piece of ſhining fleſh to be compreſſed betwixt two pieces of glaſs, to try, how well the contexture of it would reſiſt that external force; but I did not find the light to be thereby extinguiſhed, during the ſhort time I could allot to the experiment.

14. BUT ſuppoſing, that high rectified ſpirit of wine might ſo alter the contexture of the body it permeated, as to deſtroy its faculty of ſhining, I put a luminous piece of veal into a chryſtalline vial, and pouring on it a little pure ſpirit of wine, that would have burned all away, after I had ſhaken them together, I laid by the glaſs, and in about a quarter of an hour, or leſs, I found, that the light was vaniſhed.

15. BUT water would not ſo eaſily quench our ſeeming fires; for having put one of them into a China cup, and almoſt filled it with cold water, the light did not only appear, perhaps undiminiſhed, through that liquor, but above an hour after was vigorous enough not to be eclipſed by being looked upon at no great diſtance from a burning candle, that was none of the ſmalleſt; and probably the light would have been ſeen much longer, if we could have afforded to watch out its duration.

16. WHILST theſe things were doing, I cauſed the pneumatical engine to be prepared in a room without fire, (that the experiment might be tried in a greater degree of darkneſs;) and having conveyed one of the largeſt luminous pieces into a ſmall receiver, we cauſed the candles to be put out, and the pump to be plied in the dark; but the diminution of light, after the pump ſeemed to have been employed for a competent while, appeared ſo inconſiderable, (whether becauſe our eyes had leiſure to be fitted to that dark place, or for what other cauſe ſoever,) that I began to ſuſpect, that the inſtrument, having been managed in the dark, had leaked all the while. Wherefore cauſing the lights to be brought in, and a mercurial gage to be put into the receiver, when we were ſure, that this glaſs was well cemented on to the engine, the candles being removed, the pump was ſet a work again; and then opening my eyes, which I had kept cloſed againſt the light of the candles, I could perceive, upon the gradual withdrawing of the air, a diſcernible and gradual leſſening of the light; which yet was never brought quite to diſappear (as I long ſince told you, the light of rotton wood and glowworms had done) or to be ſo near vaniſhing as one would have expected; though, upon the bringing in of the candles again, it appeared by the gage, that the pump had been diligently applied. But the room being once again darkned; by the haſty increaſe of light, that had diſcloſed itſelf in the veal, upon this letting in of the air to the exhauſted receiver, it appeared more manifeſtly than before, that the decrement, though but ſlowly made, had been conſiderable. This trial we once more repeated with a not unlike ſucceſs; which, though it convinced us, that the luminous matter of our included body was more vigorous, or tenacious, than that of moſt other ſhining bodies, yet it left us ſome doubts, that the light would have been much more impaired, if not quite made to vaniſh, if the ſubject of it could have been kept long enough in our exhauſted receiver: but the unſeaſonable time of the night reducing me at length to go to bed, I could not ſtay to proſecute this, or any other trial.

17. ONLY, whilſt I was undreſſing, this further obſervation occurred, that ſuppoſing there might be, in the ſame larder, more joints of the ſame veal than one, ennobled with this ſhining faculty, it was found, that a leg of veal, which was cauſed to be brought into my chamber, had ſome ſhining places in it; though they were but very few, and faint, in compariſon of thoſe, that were conſpicuous in the above mentioned neck.

18. WHAT further phænomena this morning might have afforded me, I cannot tell, having been haſtily called up, before day, for a neice, that I am very juſtly, and exceedingly concerned for; who was thought to be upon

the point of death, and whoſe almoſt gaſping condition had too much affected and employed me, to leave me any time for philoſophical entertainments, that require a calm, if not a pleaſed mind. Only this I took notice of, becauſe the obſervation could not coſt me a minute of an hour, that whilſt they were bringing me candles for to riſe by, I looked upon a clean vial, that I had laid upon the bed by me, after a piece of our luminous veal had been included in it, and found it to ſhine vividly at that time, which was between four and five of the clock this morning; ſince when I have made no one obſervation, or trial.

POSTSCRIPT.

19. NEAR two days after I had made the fore-mentioned obſervations, thoſe horrid ſymptoms of my neice's diſeaſe, that had ſo much alarmed the phyſicians, and me, being, through God's goodneſs, conſiderably abated, I began to reſume the thoughts of our ſhining veal; and though having in the hurry I was in forgotten to take any order about it, I found it was already diſpoſed of; yet the piece, I lately mentioned to have been included in a vial, being preſerved in it, I looked upon it the third day (incluſively) after we had firſt obſerved the meat, it was cut off from, to be luminous; and I found it to ſhine in the dark as vigorouſly as ever. The fourth day its light was alſo conſpicuous; ſo that I was able, in a dark corner of the room, to ſhew it, even in the day time, to three or four very ingenious phyſicians, all of them, ſave one, members of the Royal Society; and I preſume, I need not remind you, that the following night, I invited you to be a ſpectator of it, though before that time the light had begun to decay, and the offenſive ſmell to grow ſomewhat ſtrong: which ſeems to argue, that the diſpoſition, upon whoſe account our veal was luminous, may very well conſiſt both with its being, and not being, in a ſtate of putrefaction, and conſequently, is not likely to be derived meerly from the one, or the other. The fifth day, in the morning, looking upon it when I awaked, and before the curtains were opened, it ſeemed to ſhine better, than it had done the day preceding. The ſame night alſo, it was manifeſt enough, though not vivid, in the dark. When I awaked, the ſixth day in the morning, after the ſun was riſen, I could, within the curtains, perceive a glimmering light. But the ſeventh day, which was yeſterday, I could not, late at night, diſcern any light at all.

You ſaw too much in what a condition I was, when you did me the favour to viſit me, to expect, that I ſhould preſume to entertain you with any ſpeculations about the cauſe of theſe unuſual apparitions of light. It is true indeed, that in ſome notes, I formerly mentioned to you, I endeavoured to make it probable, that whether light depend upon a particular kind of impulſe, propagated through a tranſparent medium, or upon a diffuſion of extremely little parts from the luminous body, or upon the action of ſome other corporeal agent; whatever the efficient be, the effect is produced in a mechanical way. But though I had theſe papers by me, yet, to determine what peculiar kind of motions, or other operations, nature really employed in the production of a light, which ſeemed not clearly, by what I ſhall preſently note, referable either to the particular and ſettled conſtitution of the animals, whoſe fleſh ſhined, (as in our glow-worms, and ſome American flies,) or to that inteſtine and unuſual motion of the parts, that cauſes, or accompanies putrefaction in rotten wood, or fiſhes; ſince, upon the firſt and livelieſt appearance of the light, there was not any, (at leaſt, that could be taken notice of by the ſenſes:) To determine this, I ſay, it ſeemed to me ſo difficult a taſk, that I ſhall willingly leave the ſolution of ſuch abſtruſe phænomena, as ſome of ours, unattempted; eſpecially ſince I may, God permitting, make an hiſtorical mention of them the day after to-morrow, at the meeting of the Royal Society; where, I doubt not, much more, and more to the purpoſe, will be ſaid, and conſidered, than I have vanity to think myſelf capable of offering. Only, for the prevention of ſome needleſs conjectures, to which, without this previous advertiſement, one might upon plauſible grounds indulge, I ſhall, in the mean while add, and conclude with one obſervation more, which may poſſibly take off our thoughts from ſtriving to deduce the ſhining of our veal from the peculiar nouriſhment, or conſtitution, or properties, of that individual calf, whoſe fleſh, &c. was luminous. For, having ſeveral nights ſent purpoſely into the larder, to obſerve, whether any veal, ſince brought thither, or any other meat, did afford any light, a negative anſwer was always brought me back; ſave at one time, which happened to be within leſs than forty-eight hours of that, at which the luminouſneſs of the veal had been firſt taken notice of; for at this time there was, in the ſame larder, a conſpicuous light ſeen in a pullet, that hung up there, which having cauſed to be brought up into a darkened place in my chamber, in the night-time, I perceived four or five luminous places; which were not indeed near ſo large as thoſe of the veal, but were little leſs vived than they. All of theſe I took notice to be either upon, or near the rump; and that, which appeared moſt like a ſpark of fire, ſhone at the very tip of that part. Yet was not this fowl mortified, nor at all ill ſcented, but ſo freſh, that the next day I found it very good meat. But whether this may reaſonably lead to a ſuſpicion, that the peculiar conſtitution of the air in that larder, and at that time, may as well deſerve to be taken into conſideration, as the peculiar nature of the animals, whoſe fleſh did ſhine, is a queſtion, that I, who have ſcarce time to name it, muſt not preſume to do, any more than name. And therefore, as ſoon as I have begged your pardon for this tedious, though haſty ſcribble, I ſhall, without ceremony, ſubſcribe myſelf, &c.

A

NEW EXPERIMENT,

CONCERNING

An Effect of the varying Weight of the ATMOSPHERE upon ſome Bodies in the Water; ſuggeſting a Conjecture, that the very Alterations of the Air, in point of Weight, may have conſiderable Operations, even upon Men's Sickneſs or Health.

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THOUGH many things have, by ingenious men, been already obſerved, as to the power and operations of the atmoſphere's weight upon liquors, that are expoſed to it in Torricellian tubes, or other veſſels, cloſed at one end, and near the top, either empty or unfilled with any viſible body; yet men ſeem not to have much enquired, what effects the very variation of this weight of the atmoſphere may have on the liquors which it preſſes, in other veſſels than ſuch as baroſcopes and pumps. And yet when I remember, how much of air appears by our engine to be inviſibly harboured in the pores, not only of water, but of the blood, ſerum, urine, gall, and other juices of the human body; and that (as I have elſewhere experimentally ſhewn) the preſſure of the atmoſphere, and the ſpring of the air, work upon liquors, and on bodies immerſed in thoſe liquors, as well as upon ſolid ones, immediately expoſed to the air, I am prone to ſuſpect, that the very alterations of the atmoſphere, in point of weight, may, in ſome caſes, have ſome not contemptible operations, even upon men's ſickneſs or health; as when the ambient air, for inſtance, grows ſuddenly very much lighter than it was before, or than it was wont to be, the ſpirituous and aereal particles, that are plentifully harboured in the maſs of blood, will naturally ſwell that liquor, and ſo may diſtend the greater veſſels, and not a little alter the celerity and manner of the circulation of the blood by the capillary arteries and veins. By which alteration, that divers changes may happen in the body, will not ſeem improbable to thoſe, that know in general, how important a thing the manner of the circulation of the blood may be there, though, as to its particular effects, I leave them to the ſpeculation of phyſicians; and ſhall only add, that to keep this conjecture of mine (for I propoſe it as no other) from ſeeming as groundleſs as extravagant, I will annex an experiment, that you will not perhaps diſlike, juſt as I find it regiſtered among ſome of my looſe papers.

I cauſed to be blown, at the flame of a lamp, three ſmall round glaſs bubbles, about the bigneſs of hazel-nuts, and furniſhed each of them with a ſhort and ſlender ſtem, by whoſe means they were ſo nicely poiſed in water, that a very ſmall change of weight would make them either emerge, if they but lightly leaned on the bottom of the veſſel, or ſink, if they floated on the top of the water.

THIS being done at a time, when the atmoſphere was of a convenient weight, (and ſuch a ſeaſon is not ordinarily difficult to be choſen, within ſome reaſonable time, to him, that wants neither attention, nor a good baroſcope) I put them in a wide-mouthed glaſs, furniſhed with common water, and leaving them in a quiet place, where yet they were frequently in my eye, and were ſuffered to continue many weeks, or ſome months, I obſerved, as I expected, that ſometimes they would be at the top of the water, and remain there for divers days, or perhaps weeks; and ſometimes would fall to the bottom, and after having continued there for ſome time, longer or ſhorter, they would again emerge. And though ſometimes, eſpecially if I removed the veſſel, that contained them, to a ſouthern window, they would riſe to the top, or fall to the bottom of the water, according as the air was hot or cold; yet it was not difficult to diſtinguiſh theſe motions from thoſe produced by the varying gravity of the atmoſphere. For when the beams of the ſun, or heat of the ambient air, by rarifying the air included in the bubbles, made that air drive out ſome of the water, and conſequently made the whole bubble, conſiſting of glaſs, air, and water, ſomewhat lighter than a bulk of water equal to it, though the bubble did neceſſarily ſwim as long as the included air was thus rarified, yet when the abſence of the ſun, or any other cauſe, made the air loſe its adventitious warmth, there would enſue a condenſation of the air again, and thereupon an intruſion of more water (to ſucceed the air) into the glaſs, and conſequently a ſinking of the bubble; and this would commonly happen at night, if it did not happen ſooner. But when it was upon the account of the varying weight of the atmoſphere, that the bubbles either roſe or fell, it appeared by the baroſcope, that the atmoſphere was ſo heavy, or ſo light, that they ought to do ſo. Inſomuch, that I divers times predicted, whether I ſhould find the mercury in the baroſcope high or low, by obſerving the ſituation and poſture of the bubbles; and conſulting that inſtrument, it verified my conjectures. And though, whilſt the atmoſphere was not too conſiderably either light or heavy, the changes of the air, as to heat or cold, would, as I was ſaying, place the bubbles ſometimes at the top, and ſometimes at the bottom of the water, within the compaſs of a day; yet if the atmoſphere were either very heavy, or very light, the bubbles would continue at the bottom, or at the top of the water for many days together, in caſe the atmoſphere did not in all that time change its gravity. And I remember, that I did, for curioſity's ſake, when the quickſilver was high in the baroſcope, put the glaſs two or three days in a ſouth window about noon, and for a good while after, and that in ſun-ſhining weather; and yet even then the bubbles did not emerge, though it appeared by a good ſealed weather-glaſs, which I kept in the ſame window, that the ambient air was much warmer than at other times, when I had obſerved the bubbles to keep at the top of the water.

N. B. 1. It being very difficult to poiſe ſeveral bubbles preciſely, as well one as another, I thought it not ſtrange, that all the three bubbles did not conſtantly (though for the moſt part they did) riſe and fall together; but ſometimes two of them, and now and then, though ſeldom, one alone would ſink or emerge, when the change of the weight of the atmoſphere was not conſiderable enough to operate ſenſibly upon the reſt, and of ſuch inſtances, I have had opportunity to obſerve one or two within theſe laſt three days; and therefore it is not amiſs, to poiſe a greater number of bubbles together, that after trial made of all, the fitteſt may be choſen. Which advertiſement will appear the more proper, becauſe of what is to be added in the following note.

2. I have obſerved it ſometimes to happen, that a bubble, that floated when it was firſt poiſed, would, after a while, ſubſide, without any manifeſt cauſe; or if it were made to ſink by ſuch a cauſe, it would continue at the bottom of the water, though that cauſe were removed: which difficult phænomenon ſeeming to depend upon a kind of imbibition made of certain particles of an aereal nature, by the water, the conſideration of it belongs to another place, not to this; where it may ſuffice, that the experiment did ſometimes actually anſwer expectation, as that above-related did; wherein my main drift is to ſhew, that ſince, as the atmoſphere was heavier or lighter, it is capable to work upon bodies under water, ſo as to procure their ſinking, or their emerſion; the air, though a fluid a thouſand times lighter, muſt lean or preſs upon the water itſelf, by whoſe intervention it produces theſe effects; which confirms what I elſewhere teach, that the atmoſphere is incumbent, as a heavy body, upon the terraqueous globe.

3. **Besides** the other circumſtances, upon whoſe account this experiment may fail of ſucceſs, the ſeaſon of the year, wherein it is tried, may, for aught I know, be conſiderable. For which reaſon I ſhall here add this advertiſement, that I chuſe, but do not confine myſelf, to make my trials about the beginning of the ſpring, as a time, wherein notable alterations of the air, as well as to weight, as to other things, are the likelieſt to be frequent.

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ESSAYS
OF THE
STRANGE SUBTILTY,
GREAT EFFICACY,
DETERMINATE NATURE
OF
EFFLUVIUMS.

To which are annexed

NEW EXPERIMENTS to make FIRE and FLAME Ponderable:

TOGETHER WITH

A Diſcovery of the PERVIOUSNESS of GLASS.

An ADVERTISEMENT to the READER.

IT is hoped, the reader will not think it ſtrange, not to meet with, in the following papers, a more cloſe and uniform contexture of the paſſages, that make them up, if he be ſeaſonably informed of the riſe and occaſion of penning them, which was this. The author having many years ago written an eſſay about an experiment he made of nitre, by whoſe phænomena he endeavoured to exemplify ſome parts of the corpuſcular philoſophy, eſpecially the production of qualities; he afterwards threw together divers occuring thoughts and experiments, which he ſuppoſed might be employed by way of notes, to prove or illuſtrate thoſe doctrines, and eſpecially thoſe, that concerned the qualities of bodies; and among theſe obſerving thoſe, that are called occult, to be ſubjects uncultivated enough, (at leaſt, in the way, that ſeemed to him proper,) he propoſed to handle them more largely than moſt of the reſt; and in order to that deſign he judged it almoſt neceſſary to premiſe ſome conſiderations and experimental collections about the nature and power of effluviums, about the pores of bodies and figures of corpuſcles, and about the efficacy of ſuch local motions, as are wont either to be judged very faint, or to be paſſed by unheeded. For he had often looked upon theſe three doctrines, of effluvia, of pores and figures, and of unheeded motions, as the three principal keys to the philoſophy of occult qualities. But having hereupon made ſuch collections, as upon review appeared too large to paſs for notes on ſo ſhort a text, he was induced to draw them * into the form (they now appear in) of eſſays; but he would not put himſelf to the trouble of doing it, with care to keep them from retaining much of their firſt want of exact method and connection. Nor was the author ſolicitous to finiſh them up, in regard that his other ſtudies and occaſions made him perceive, that in what he had deſigned about occult qualities, he had cut himſelf out more work, than probably he ſhould, during many years, have opportunity to ſet upon in earneſt, and complete. And in this condition theſe papers lay for divers years, (as is well known to ſeveral, that ſaw them, or even tranſcribed ſome of them,) and might have continued to do ſo, if the author had not

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* And ſome that were publiſhed anno 1669, under The title of the Atmoſpheres of Conſiſtent Bodies.

been induced to let them come abroad, partly by conſidering, that though the ſubjects, (however he handled them) were as well important as curious, yet he did not find himſelf prevented by others in what he had to publiſh about them; and partly by the references he had made to them in ſome other papers, that he had promiſed his friends, wherein ſeveral things here delivered are vouched, and others ſuppoſed. And becauſe the notes concerning the poroſity of greater bodies, and the figurations of minute particles, together with the paper about unregarded motions, having been long laid aſide among other neglected papers, were ſome of them miſſing, and others ſo miſuſed, that they could not eaſily be made ready to accompany thoſe, that now come abroad; the author, that he might keep this book from having its dimenſions too diſproportionate, was content to add to the thickneſs of it, by ſubjoining one of thoſe little tracts, that lay by him, concerning flame, becauſe of the affinity betwixt the preceding doctrine about effluviums in general, and experiments, that ſhew, in particular, the ſubtilty and efficacy of thoſe of fire and flame. And though to that tract itſelf there belong another, deſigned to examine, whether the matter of what we call the ſun-beams, may be brought to be ponderable; yet ſuppoſing this hitherto cold and wet ſummer to be like to be as unfriendly to the trials to be made with burning-glaſſes, as of late years ſome other ſummers have proved, he was eaſily prevailed with, not to make thoſe experiments, that were ready, wait any longer for thoſe, that probably will not in a ſhort time be ſo; eſpecially ſince thoſe, that now come abroad, have no dependency upon the others.

OF THE STRANGE SUBTILTY OF EFFLUVIUMS.

CHAP. I.

WHETHER we ſuppoſe, with the antient and modern atomiſts, that all ſenſible bodies are made up of corpuſcles, not only inſenſible, but indiviſible; or whether we think with the Carteſians, and (as many of that party teach us) with *Ariſtotle*, that matter, like quantity, is indefinitely, if not infinitely diviſible; it will be conſonant enough to either doctrine, that the effluvia of bodies may conſiſt of particles extremely ſmall. For if we embrace the opinion of *Ariſtotle*, or *Des-Cartes*, there is no ſtop to be put to the ſubdiviſion of matter into fragments ſtill leſſer and leſſer. And though the Epicurean hypotheſis admit not of ſuch an interminate diviſion of matter, but will have it ſtop at certain ſolid corpuſcles, which, for their not being further diviſible, are called atoms, ἄτομοι; yet the aſſertors of theſe do juſtly think themſelves injured, when they are charged with taking the motes, or ſmall duſt, that fly up and down in the ſun-beams, for their atoms; ſince, according to theſe philoſophers, one of thoſe little grains of duſt, that is viſible only, when it plays in the ſun-beams, may be compoſed of a multitude of atoms, and exceed many thouſands of them in bulk. This the learned *Gaſſendus* in his notes on *Diogenes Laertius* makes probable by the inſtance of a ſmall mite, which, though ſcarce diſtinctly diſcernible by the naked eye, unleſs when it is in motion, does yet, in a good microſcope, appear to be a compleat animal, furniſhed with all neceſſary parts; which I can eaſily allow, having often in cheeſe-mites very diſtinctly ſeen the hair growing upon their legs. And to the former inſtance, I might add, what I have elſewhere told you of a ſort of animals far leſſer than cheeſe-mites themſelves, namely thoſe, that may be oftentimes ſeen in vinegar. But what has been already ſaid, may ſuffice for my preſent purpoſe, which is only to ſhew, that the wonderful minuteneſs I ſhall hereafter aſcribe to effluvia, is not inconſiſtent with the moſt received theories of naturaliſts. For otherwiſe, in this eſſay, the proofs I mean to employ, muſt be taken, not *à priori*, but *à poſteriori*. And the experiments and obſervations, I ſhall employ on this occaſion, will be chiefly thoſe, that are referrible to one of the following heads.

1. THE ſtrange extenſibility of ſome bodies, whilſt their parts yet remain tangible.
2. THE multitude of viſible corpuſcles, that may be afforded by a ſmall portion of matter.
3. THE ſmallneſs of the pores, at which the effluvia of ſome bodies will get in.
4. THE ſmall decrement of bulk, or weight, that a body may ſuffer by parting with great ſtore of effluvia.

5. The great quantity of ſpace, that may be filled, as to ſenſe, by a ſmall quantity of matter, when rarified or diſperſed.

But though to theſe diſtinct heads I ſhall deſign diſtinct chapters, yet you muſt not expect to find the inſtances ſolicitouſly marſhalled, but ſet down in the order they occurred to me; ſuch a liberty being allowable in a paper, where I pretend not to write treatiſes, but [*] notes.

CHAP. II.

Among many things, that are groſs enough to be the objects of our touch, and to be managed with our hands, there are ſome, that may help us to conceive a wonderful minuteneſs in the ſmall parts they conſiſt of.

I do not remember what *Cardan*, and ſince him, another writer, have delivered about the thinneſs and ſlenderneſs, to which gold may be brought. And therefore, without poſitively aſſenting to, or abſolutely rejecting what may have been ſaid about it by others, I ſhall only borrow, on this occaſion, what I have mentioned on [†] another, upon my own obſervation; namely, that ſilver, whoſe ductility and tractility are very much inferior to thoſe of gold, was, by my procuring, drawn out to ſo ſlender a wire, that, when we meaſured it, which was ſomewhat troubleſome to do, with a long and accurate meaſure, we found, that eight yards of it did not yet fully counterpoiſe one grain: ſo that we might add a grain more without making the ſcale, wherein it was put, manifeſtly preponderate, notwithſtanding the tenderneſs of the balance. Whence we concluded, that a ſingle grain of this wire amounted to twenty-ſeven foot, that is, three hundred and twenty-four inches. And ſince experience informs us, that half an Engliſh inch can, by diagonal lines, be divided into one hundred parts, great enough to be eaſily diſtinguiſhed, even for mechanical uſes, it follows, that a grain of this wire-drawn ſilver, may be divided into ſixty-four thouſand eight hundred parts, and yet each of theſe will be a true metalline, though but ſlender and ſhort, cylinder, which we may very well conceive to conſiſt yet of a multitude of minuter parts. For though I could procure no gilt wire near ſo ſlender as our newly-mentioned ſilver-wire; yet I tried, that ſome, which I had by me, was ſmall enough to make one grain of it fourteen foot long: at which rate, an ounce did amount to a full mile, conſiſting of one thouſand geometrical paces, of five foot a-piece, and ſeven hundred and twenty foot over and above. And if now it be permitted to ſuppoſe the wire to have been, as in probability it might have been, further drawn out to the ſame ſlenderneſs with the above-mentioned ſilver wire, the inſtance will ſtill be far more conſiderable; for in this caſe, each of thoſe little cylinders, of which ſixty-four thouſand eight hundred go to the making of one grain, will have a ſuperficial area, which, except at the baſis, will be covered with a caſe of gold; which is not only ſeparable from it by a mental operation, but perhaps alſo by a chemical one. For I remember, that from very ſlender gilt wire, though I could get none ſo ſlender as this of meer ſilver, I did more than once, for curioſity's ſake, ſo get out the ſilver, that the golden films, whilſt they were in a liquor that plumped them up, ſeemed to be ſolid wires of gold; but when the liquor was withdrawn, they appeared, as indeed they were, to be oblong, and extremely thin and double membranes of that metal, which, with an inſtrument, that had been delicate enough, might have been ripped open, and diſplayed, and been made capable of further diviſions and ſub-diviſions. To this I ſhall add, that each of the little ſilver cylinders I lately ſpake of, muſt not only have its little area, but its ſolidity; and yet I ſaw no reaſon to doubt, but that it might be very poſſible, if the artificer had been ſo ſkilful and willing, as I wiſhed, to have drawn the ſame quantity of metal to a much greater length, ſince even an animal ſubſtance is capable of being brought to a ſlenderneſs much ſurpaſſing that of our wire, ſuppoſing the truth of an obſervation of very credible perſons, critical enough in making experiments, which, for a confirmation, and an improvement of our preſent argument, I ſhall now ſubjoin. An ingenious gentlewoman of my acquaintance, wife to a learned phyſician, taking much pleaſure to keep ſilk-worms, had once the curioſity to draw out one of the oval caſes, (which the ſilk-worms ſpins, not, as it is commonly thought, out of its belly, but out of the mouth, whence I have taken pleaſure to draw it out with my fingers,) into all the ſilken wire it was made up of, which, to the great wonder, as well of her huſband, as herſelf, who both informed me of it, appeared to be, by meaſure, a great deal above three hundred yards, and yet weighed but two grains and a half: ſo that each cylindrically ſhaped grain of ſilk may well be reckoned to be at leaſt one hundred and twenty yards long.

Another way, I remember, I alſo employed to help men, by the extenſibility of gold, the better to conceive the minuteneſs of the parts of ſolid bodies.

We took ſix beaten leaves of gold, which we meaſured one by one with a ruler purpoſely made for nice experiments, and found them to have a greater equality in dimenſions, and to be nearer true ſquares, than could be well expected: the ſide of the ſquare was in each of them exactly enough three inches and $\frac{2}{8}$, or $\frac{1}{4}$, which number being reduced to a decimal fraction, viz. $\frac{325}{100}$, and multiplied by itſelf, affords $\frac{105625}{10000}$ for the area, or ſuperficial content of each ſquare leaf: and this multiplied by 6, the number of the leaves amounts to $\frac{633750}{10000}$ ſquare inches, for the area of the ſix leaves. Theſe, being carefully weighed in a pair of tender ſcales, amounted all of them to one grain and a quarter: and ſo one grain of this foliated gold was extended to ſomewhat above fifty inches; which differed but about a fifth part from an experiment of the like nature, that I remember I made many years ago in a pair of exact ſcales; and ſo ſmall a difference may very well be imputed to that of the pains and diligence of the gold-beaters, who do not always work with equal ſtrength and ſkill, nor upon equally fine and ductile gold.

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[*] This Eſſay was deſigned to be but a part of the author's notes upon his Eſſay about Salt-petre.

[†] In a paper about Improbable Truths.

Now if we recal to mind what I was lately ſaying, of the actual diviſibility of an inch into an hundred ſenſible parts, and ſuppoſe an inch ſo divided, to be applied to each ſide of a ſquare inch of the leaf-gold newly mentioned, it is manifeſt, that by ſubtle parallel lines, drawn between all the oppoſite points, a grain of gold muſt be diviſible into five hundred thouſand little ſquares, very minute indeed, but yet diſcernible by a ſufficient ſharp-ſighted eye. And if we ſuppoſe an inch to be divided into two hundred parts, as I lately told you it was in a ruler I employ, then, according to the newly-recited way, the number of the ſquares, into which a ſingle grain is capable of being divided, will amount to no leſs than two millions.

There is yet another way, that I took to ſhew, that the extenſibility, and conſequently the diviſibleneſs of gold, is probably far more wonderful, than by the lately mentioned trial it appears.

For this purpoſe, I went to a great refiner, whom I uſed to deal with for purified gold and ſilver, and enquired of him, how many grains of leaf-gold he was wont to allow to an ounce of ſilver, when it was to be drawn into gilt wire as ſlender as an hair. To this he anſwered me, that eight grains was the proportion he allowed to an ounce, when the wire was to be well gilt; but if it were to be more ſlightly gilt, ſix grains would ſerve the turn. And to the ſame purpoſe I was anſwered by a ſkilful wire-drawer. And I remember, that deſiring the refiner to ſhew me an ingot of ſilver, as he did at firſt gild it; he ſhewed me a good fair cylindrical bar, whereon the leaf-gold, that overlaid the ſurface, did not appear to be, by odds, ſo thick as fine Venetian paper; and yet comparing this with gilt wire, which I alſo deſired to ſee, the wire appeared to be the better gilt of the two; poſſibly, becauſe the gold, in paſſing through the various holes, was by the ſides of them not only extended, but poliſhed, which made it look more vividly than the unpoliſhed leaves, that gilded the ingot.

So that, if we ſuppoſe an ounce of the gilt wire formerly-mentioned, to have been gilt with ſix grains of leaf-gold, it will appear, by an eaſy calculation, that at this rate one ounce of gold, employed on gilding wire of that ſlenderneſs, would reach between ninety and an hundred miles. But if now we further ſuppoſe, as we lately did, that the ſlender ſilver wire, mentioned at the beginning of this chapter, were gilt; though we ſhould allow it to have (becauſe of its exceeding ſlenderneſs,) not, as the former, ſix grains, but eight grains of leaf-gold to an ounce of ſilver, it muſt be acknowledged, that an hollow cylinder, or ſheath of gold, weighing but eight grains, may be ſo ſtretched, that it will reach to no leſs than ſixty times as much in weight of ſilver wire, as it covers: [I ſaid ſixty times, for ſo often is eight contained in four hundred and eighty, the number of grains in an ounce;] and conſequently, a grain of that wire having been found to be twenty-ſeven foot long, the ounce of gold would reach to ſeven hundred ſeventy-ſeven thouſand ſix hundred foot, that is, an hundred fifty-five miles and above a half. And if we yet further ſuppoſe this ſuperficial, or hollow cylinder of gold, to be ſlit all along, and cut into as ſlender liſts or thongs as may be, we muſt not deny, that gold may be made to reach to a ſtupendious length. But we need not this laſt ſuppoſition, to make what preceded it an amazing thing; which yet, though it be indeed ſtupendous, and ſeem incredible, ought not at all to be judged impoſſible; being no more than what, upon the ſuppoſitions and obſervations above laid down, does evidently follow.

CHAP. III.

After what has been ſaid of the minuteneſs of tangible objects, it will be proper to ſubjoin ſome inſtances of the ſmallneſs of ſuch as yet continue viſible. But, in regard theſe corpuſcles are ſingly too little to have any common meaſure applied to any of them, we muſt make an eſtimate of their minuteneſs, by the number of thoſe, into which a ſmall portion or fragment of matter may be actually divided, the multitude of theſe being afforded by ſo inconſiderable a quantity of matter, ſufficiently declaring, that each of them, in particular, muſt be marvelouſly little.

Among the inſtances, where the ſmallneſs of bodies may be deduced from what is immediately the object of ſight, it may not be unfit to take notice of the evaporation of water, which though it be granted to conſiſt of groſs particles, in compariſon of the ſpirituous and odoriferous ones of divers other liquors, as of pure ſpirit of wine, eſſential oils of ſpices, &c. yet to ſhew, that a ſmall quantity of it may be diſperſed into a multitude of manifeſtly viſible corpuſcles, I thought upon, and more than once tried, the rarefaction of it into vapours by help of an æolipile, wherein, when I made the experiment the laſt time, I took the pains to regiſter the event as follows.

We put an ounce of common water into an æolipile, and having put it upon a chafing-diſh of coals, we obſerved the time, when the ſtreams of vapours began to be manifeſt. This ſtream was, for a good while, impetuous enough, as appeared by the noiſe it made, which would be much increaſed, if we applied to it, at a convenient diſtance, a kindled brand, in which it would blow up the fire very vehemently. The ſtream continued about a quarter of an hour, ſixteen minutes or better, but afterwards the wind had pauſes and guſts for two or three minutes before it quite ceaſed. And by reaſon of the ſhape of the æolipile, (which being framed chiefly for other purpoſes, was not ſo convenient for this) a great portion of the vapours condenſed in the upper part of it, and fell down in drops; ſo that ſuppoſing, that they alſo had come out in the form of wind, and the blaſt had not been intermitted toward the latter end, I gueſſed it might have continued uninterruptedly eighteen or twenty minutes. Note, that applying a meaſure to the ſmoke, that came out very viſible in a form almoſt conical, where it ſeemed to have an inch or more in diameter, it was diſtant from the hole of the æolipile about twenty inches; and five or ſix inches beyond that, though it were ſpread ſo much, as to have four or five inches in diameter, yet the not uniform, but ſtill-cohering clouds, which was the form, wherein the vapours appeared, were manifeſt and conſpicuous.

After the rarefaction of water, when it is turned into vapours, we may conſider that of fewel, when it is turned into flame; to which purpoſe, I might here propoſe ſeveral trials, as well of our own as others, about the prodigious expanſion of ſome inflammable bodies upon their being actually turned into flame. But in this place to mention all theſe, would perhaps too much intrench upon another paper; and therefore I ſhall here propoſe to your conſideration but one inſtance, and that very eaſy to be tried; of which I find this account among my *adverſaria*.

Having oftentimes burnt ſpirit of wine, and alſo oil in glaſs lamps, that for certain uſes were ſo made, that the ſurface of the liquor was ſtill circular, it was obvious to obſerve, how little the liquor would ſubſide by the waſte, that was made of it, in about half a quarter of an hour. And yet if we conſider, that the naked eye, after ſome exerciſe, may, as I have often tried, diſcern the motions of a pendulum, that ſwings faſt enough to divide a ſingle minute of an hour into two hundred and forty parts, and conſequently half a quarter of an hour into one thouſand eight hundred parts; if we alſo conſider, into how many parts of the time employed by a pendulum, the vibrations, ſlow enough to be diſcernible by the eye, may be mentally ſubdivided; and if we further conſider, that, without intermiſſion, the oil is preyed upon by an actual flame, and the particles of it do continually furniſh a conſiderable ſtream of ſhining matter, that with a ſtrange celerity is always flying away; we may very well conceive, that thoſe parts of flame, into which the oil is turned, are ſtupendouſly minute, ſince, though the waſting of the oil is in its progreſs too ſlow to be perceived by the eye, yet it is undoubted, that there is a continual decrement of the depth of the oil, the phyſical ſurfaces whereof are continually and ſucceſſively attenuated and turned into flame; and the ſtrange ſubtilty of the corpuſcles of flame would be much the ſtronglier argued, if we ſhould ſuppoſe, that inſtead of common oil the flame were nouriſhed by a fewel ſo much more compact and durable, as is that inflammable ſubſtance made of a metalline body, of whoſe laſtingneſs I have elſewhere made particular mention *, after having taught the way of preparing it.

Having in a pair of tender ſcales carefully weighed out half a grain of good gunpowder, we laid it on a piece of tile, and whelmed over it a veſſel of glaſs (elſewhere deſcribed, and often mentioned) with a braſs plate to cover the upper orifice of it. Then having fired the gunpowder, we obſerved, that the ſmoke of it did opacate, and, as to ſenſe, ſo fill the whole cavity of the glaſs, though its baſis were eight inches, its perpendicular height above twenty inches, and its figure far more capacious, than if it were conical; and this ſmoke, not containing itſelf within the veſſel, iſſued out at two or three little intervals, that were purpoſely left between the orifice of the veſſel and the plate, that lay upon it. This cover we then removed, that we might obſerve how long the ſmoke would continue to aſcend; which we found it would do for about half a quarter of an hour, and during near half that time, viz. the three firſt minutes, the continually aſcending ſmoke ſeemed to be, at its going out, of the ſame diameter with the orifice at which it iſſued; and it would aſcend ſometimes a foot, ſometimes half a yard, ſometimes two foot, or more, into the air, before it would diſperſe and vaniſh into it.

Now if we conſider, that the cavity of this round orifice was two inches in diameter, how many myriads of viſible corpuſcles may we eaſily conceive thronged out at ſo large an outlet, in the time above-mentioned, ſince they were continually thruſting one another forwards? and into ſo many viſible particles of ſmoke muſt we admit, that the half grain of powder was ſhattered, beſide thoſe multitudes, which, having been turned into actual flame, may probably be ſuppoſed to have ſuffered a comminution, that made them become inviſible. And though I ſhall not attempt ſo hopeleſs a work, as to compute the number of theſe ſmall particles; yet to make an eſtimate, whereby it would appear to be exceeding great, I thought fit to conſider, how great the proportion was between the ſpaces, that to the eye appeared all full of ſmoke, and the dimenſions of the powder, that was reſolved into that ſmoke. Cauſing then the glaſs to be filled with common water, we found it to contain above two and twenty pints of that liquor, and cauſing one of thoſe meaſures to be weighed, it was found to weigh ſo near a pound (of ſixteen ounces) that the computation of the whole water amounted to at leaſt one hundred and ſixty thouſand grains, and conſequently three hundred and twenty thouſand half grains. To which if we add, that this gunpowder would readily ſink to the bottom of water, as being (by reaſon of the ſalt-petre and brimſtone, that make up at leaſt ſix parts of ſeven of it) in ſpecie heavier than it, and in likelihood twice as heavy (for it is not eaſy to determine it exactly) we may probably gueſs the ſpace to which the ſmoke reached, to exceed five hundred thouſand times that, which contained the unfired powder; and this, though the ſmoke, being confined in the veſſel, was thereby kept from diffuſing itſelf ſo far, as by its ſtreaming out it ſeemed likely, that it would have done.

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* In ſome papers about Flame.

To theſe inſtances from inanimate bodies I ſhall ſubjoin one more taken from animals. Whereas then men have with reaſon wondered, that ſo ſmall a body as a cheeſe-mite, which by the naked eye is oftentimes not to be taken notice of, unleſs it move, (if even then it be ſo) ſhould, by the microſcope, appear to be an animal furniſhed with all neceſſary parts; whereas this, I ſay, has given juſt occaſion to conclude, that the corpuſcles, that make up the parts of ſo ſmall an animal, muſt themſelves be extremely ſmall; I think the argument may be much improved by the following conſideration. Thoſe, that have had the curioſity to open from time to time eggs, that are ſat upon by a hatching hen, cannot but have obſerved, how ſmall a proportion, in reference to the bulk of the whole egg, the chick bears; when that, which the excellent *Harvey* calls *punctum ſaliens*, diſcloſes the motion of the heart, and the colour of the blood; and that even about the ſeventh or eighth day, the whole chick now viſibly formed bears no great proportion to the whole egg, which is to ſupply it with aliment, not only for its nouriſhment, but ſpeedy growth for many days after.

To apply this now to the matter in hand; having ſeveral times obſerved, and ſhewn to others, that cheeſe-mites themſelves are generated of eggs, if we conceive, that in theſe eggs, as in ordinary ones, the animal at its firſt formation bears but a ſmall proportion to the bulk of the whole egg, the remaining part being to ſuffice for the food and growth of the embryo probably for a pretty while; ſince, if an ingenious perſon, that I deſired to watch them, did not miſinform me, they uſed to be about ten or twelve days in hatching; this whole egg itſelf will be allowed to be but little, in reference to the mite it came from; how extremely and unimaginibly minute may we ſuppoſe thoſe parts to be, that make up the alimental liquors, and even the ſpirits, that paſſing through the nerves, or analogous parts, ſerve to move the limbs and ſenſories of but, as it were, the model of ſuch an animal, as, when it reſts, would not, perhaps, itſelf, to the naked eye be ſo much as viſible; and in which we may preſume the nobler ſort of ſtabler parts to be of an amazing ſlenderneſs, if we conſider, that, though in other hairy animals, the optick, or ſome other of the larger nerves do, I know not how many times, in thickneſs and circuit, ſurpaſs a hair of the ſame animal; yet in a cheeſe-mite, though none of the largeſt of thoſe creatures, we have divers times manifeſtly ſeen, as is before intimated, ſingle hairs, that grow upon the legs.

ANOTHER way there is, that I employed to give men cauſe to think, that the inviſible effluvia of bodies, that wander through the air, may be ſtrangely minute; and this was, by ſhewing, how ſmall a fragment of matter may be reſolved into particles minute enough to aſſociate themſelves in ſuch numbers with a fluid ſo much more denſe than air, as water is, as to impart a determinate colour to the whole liquor. What I did with cochineal in proſecution of this deſign, my experiments about colours may inform you; but I ſhall now relate the ſucceſs of an attempt made another way, for which perhaps ſome of your friends, the chemiſts, will thank me; though I was not ſolicitous to carry on the experiment very far with gold, not becauſe I judged that leſs diviſible into a number of coloured particles, but becauſe I found, as I expected, that the paleneſs of the native colour of the gold may make it in the end leſs conſpicuous, though, if I had then had by me a menſtruum, as I ſometimes had, that would diſſolve gold blood-red, perhaps the experiment with gold would have ſurpaſſed that, which it is now time I ſhould begin to relate, as ſoon as I have hinted to you by the way, that, for variety's ſake, I made a trial with copper calcined *per ſe*, that I might not be accuſed of having omitted to employ a metal, whoſe body chemiſts ſuppoſe to be much opened by calcination. And though the event were notable, even in compariſon of that of the experiment made with cochineal, yet my conjectures inclined me much to prefer the way deſcribed in the following account.

WE carefully weighed out in a pair of tender ſcales one grain of copper not calcined, but barely filed; and becauſe, as we made choice of this metal for its yielding in moſt menſtruums a blue, which is a deep and conſpicuous colour, we alſo choſe to make a ſolution, not in aqua fortis, or aqua regis, but the ſpirit of ſal armoniack (as that is an urinous ſpirit,) having found by former trials, that this menſtruum would give a far deeper ſolution than either of the others. This lovely liquor, of which we uſed a good proportion, that all the copper might be thoroughly diſſolved, we put into a tall cylindrical glaſs of about four inches in diameter, and by degrees poured to it of diſtilled water, which is more proper in this caſe than common water, which has oftentimes an inconvenient ſaltiſhneſs, till we had almoſt filled the glaſs, and ſaw the colour grow ſomewhat pale, without being too dilute to be manifeſt; and then we warily poured this liquour into a conical glaſs, that it might be the more eaſy to fill the veſſel ſeveral times to the ſame height. This conical glaſs we filled to a certain mark four times conſecutively, weighing it, and the liquor too, as often in a pair of excellent ſcales purpoſely made for ſtatical experiments, and which, though ſtrong enough to weigh ſome pounds in each ſcale, would, when not too much loaden, turn with about one grain. Theſe ſeveral weights of the glaſs, together with the contained liquor, we added together, and then carefully weighing the empty glaſs again, we deducted four times its weight from the above-mentioned ſum, and thereby found the weight of the liquor alone, to be that, which reduced to grains, amounted to 28,534, ſo that a grain of copper, which is not full half ſo heavy in ſpecie as fine gold, communicated a tincture to 28,534 times its weight.

But now, if you pleaſe to take notice, that the ſcope of my experiment was to ſhew, into what a number of parts one grain of copper might be divided; you will allow me to conſider, as I did, that this multitude of parts muſt be eſtimated by the proportion, not ſo much in weight as in bulk, of the tinging metal to the tinged liquor; and conſequently, ſince that divers hydroſtatical trials have informed me, that the weight of copper to the weight of water of the ſame bulk is *proximè* as nine to one, a grain-weight of copper is in bigneſs but the ninth part of as much water as weighs a grain; and ſo the formerly-mentioned number of the grains of water muſt be multiplied by nine, to give us the proportion between the tinging and tinged bodies, that is, that a ſingle grain of copper, gave a blueneſs to above 256,806 parts of limpid water, each of them as big as it. Which, though it may ſeem ſtupendous, and ſcarce credible; yet I thought fit to proſecute the experiment ſomewhat farther, by pouring all the liquor out of the tall cylindrical glaſs into another clean veſſel, whence filling the conical glaſs twice, and emptying it as often into the ſame cylindrical glaſs, the third time I filled the conical glaſs with colourleſs diſtilled water, and pouring that alſo into the cylindrical glaſs, we found the mixed liquor to have yet a manifeſt, though but a pale blueneſs. And laſtly, throwing away what was in the cylindrical glaſs, we poured into it, out of the ſame conical glaſs, equal parts of diſtilled colourleſs water, and of the tincted liquor we had formerly ſet a-part in the clean veſſel; and found, that though the colour were very faint and dilute, yet an attentive eye could eaſily diſcern it to be blueiſh; and ſo it was judged by an intelligent ſtranger, that was brought in to look upon it, and was deſired to diſcover of what colour he thought it to be. Whereby it appears, that one grain of copper was able to impart a colour to above double the quantity of water above-mentioned.

This experiment I have allowed myſelf to be the longer and more particular in relating, both becauſe I know not, that any ſuch has been hitherto either made or attempted, and becauſe it will probably gratify your chemiſts, that love to have the tinctures of metals believed very diffuſive; and becauſe, if circumſtances were not added, it would ſeem to you as well incredible, as perhaps it does ſeem ſtupendous, that a portion of matter ſhould be able to impart a conſpicuous colour to above 256,806 times its bulk of water, and a manifeſt tincture to above 385,200, (for ſo it did, when the proportion of the tinged part to the whole mixture, made of it, and the untinged part, was as 2 to 31,) and a faint, but yet diſcernible and diſtinguiſhable colour, to above five hundred and thirteen thouſand ſix hundred and twenty times its bulk of water.

CHAP. IV.

It were eaſy for me (*Pyroph.*) to give you ſeveral inſtances, to ſhew, that the effluvia of liquors may get in at the pores of bodies, that are reputed of a cloſe texture; but I ſhall at preſent forbear to mention ſuch examples, not only becauſe they belong to another place*, where I take notice of them, but becauſe many ſuch would not ſeem ſo remarkable, nor be ſo conſiderable to our preſent purpoſe, as a few taken from bodies, that are not fluid.

And firſt, it is delivered by writers of good credit, that ſeveral perſons, (for the experiment does not hold in all) by barely holding for ſome time dried cantharides in their hands, have been put to much pain at the neck of the bladder, and have had ſome other parts miniſtering to the ſecretion of urine ſenſibly injured. That this is true, I am induced to believe, by what I have elſewhere related to you of the unwelcome experiment I had of the effect of cantharides applied but outwardly to my neck, and that unknown to me, upon the urinary paſſages; and that theſe operations are due to material effluxes, which, to get into the maſs of blood, muſt paſs through the pores of the ſkin, you will not, I preſume, put me to prove.

Scaliger Exercit. 186. relates, that in *Gaſcony*, his country, there are ſpiders of that virulency, that if a man treads upon them to cruſh them, their poiſon will paſs through the very ſoles of his ſhoes. Which ſtory, notwithſtanding the reputation of the author, I ſhould perhaps have left unmentioned, becauſe of a much ſtranger about ſpiders, which he rerelates in the ſame ſection, but that I met with one that is analogous in the diligent *Piſo*'s late hiſtory of *Braſil*; where, having ſpoken of another venemous fiſh of that country, and the antidotes he had ſucceſsfully uſed to cure the hurts it inflicts, he proceeds to that fiſh the natives call *Amoreatim*, of one kind whereof, called by the Portugals *Peize Sola*, his words are theſe; *Quæ mira ſanè efficacia non ſolum manum vel leviſſimo attactu, ſed & pedem, licet optimè calceatum, piſcatoris incautè piſciculum conterentis, paralyſi & ſtupore afficit, inſtar torpedinis Europææ, ſed minus durabili.* Lib. 5. cap. 14.

What I ſhall ere long have occaſion to tell you of the power of the *Torpedo*, and ſome other animals, to affect the hand and arm of him that ſtrikes them, ſeems applicable to the matter under conſideration: for, though their affecting the ſtriker at a diſtance may very well be aſcribed to the ſtupefactive, or other venemous exhalations, that expire (and perhaps are as it were darted) from the animal irritated by the ſtroke, and are breathed in together with the air they infect; yet their benumbing, or otherwiſe affecting the arm that ſtruck them, rather than any other part, ſeems to argue, that the poiſonous ſteams get in at the pores of the ſkin of the limb, and ſo ſtupify, or otherwiſe injure, the nervous and muſculous parts of it.

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* A Diſcourſe of pores of bodies, and figures of corpuſcles.

OTHER examples belonging to this ſection, may be referred hither from divers other places in theſe papers about occult qualities, and therefore I ſhall only add here, that moſt remarkable proof, That ſome emanations, even of ſolid bodies, may be ſubtil enough to get through the pores, even of the cloſeſt bodies; which is afforded us by the effluvia of the loadſtone, which are by magnetical writers ſaid to penetrate, without reſiſtance, all kind of bodies. And though I have not tried this in all ſorts, yet having tried it metals themſelves, I am apt to think, the general rule admits of very few exceptions, eſpecially, if that can be fully made out, which is affirmed about the perviouſneſs of glaſs to the effluxions of the loadſtone. For, not only glaſs is generally reputed to be as cloſe a body as any is, but (which weighs more with me) I have by trials purpoſely made, had occaſion to admire the cloſeneſs of very thin pieces of glaſs. But the reaſon, why I juſt now expreſſed myſelf with an If, was, becauſe I was not entirely ſatisfied with the proof wont to be acquieſced in, of the perviouſneſs of glaſs; namely, that in dials and ſea-compaſſes, that are covered with plates of glaſs, the needle may be readily moved to and fro by a loadſtone held over it. For theſe plates being commonly but faſtened on with wax, or at beſt with cement, a ſceptick may pretend, that the magnetical effluvia paſs not through the glaſs, but through that much more pervious matter, that is employed to ſecure the commiſſures, only from the acceſs of the air. To put then the matter paſt doubt, I cauſed ſome needles to be hermetically ſealed up in glaſs-pipes, which being laid upon the ſurface of water (whereon, by reaſon of the bigneſs of the cavities, they would lightly float,) the included needles did not only readily feel the virtue of an externally applied loadſtone, (though but a weak one) but complied with it ſo well, that I could eaſily, by the help of the needle, lead, without touching it, the whole pipe, this was ſhut up in, to what part of the ſurface of the water I pleaſed. And I alſo found, that by applying a better loadſtone to the upper part of a ſealed pipe, and a needle in it, I could make the needle leap up from the lower part, as near to the loadſtone, as the interpoſed glaſs would give it leave.

BUT I thought it would be more conſiderable, to manifeſt, that the magnetical effluvia, even of ſuch a dull body, as the globe of the earth, would alſo penetrate glaſs. And though this ſeem difficult to be tried, becauſe no ordinary loadſtone, nor any iron touched by it, was to be employed to work on the included iron; yet I thought fit to attempt it after this manner. I took a cylindrical piece of iron, of about the bigneſs of ones little finger, and between half a foot and a foot long, (for I had formerly obſerved, that the quantity of unexcited iron furthers its operation upon excited needles,) and having hermetically ſealed it up in a glaſs-pipe but very little longer than it, I ſuppoſed, that if I held it in a perpendicular poſture, the magnetical effluvia of the earth, penetrating the glaſs, would make the lower extream of the iron anſwerable to the north pole; and therefore having applied this to the point of the needle in a dial, or ſea-compaſs, that looked toward the north, (for authors mean not all the ſame thing by the northern pole of a needle, or loadſtone,) I preſumed it would, according to the laws magnetical, (elſewhere mentioned) drive it away, which accordingly it did. And having for farther trial inverted the included iron, (ſo that the end, which was formerly the lowermoſt, was now the uppermoſt) and held it in a perpendicular poſture, juſt under the ſame point of the needle, that extream of the iron-rod, which before had driven away this point, being by this inverſion become, in a manner, a ſouth-pole, did (according to the ſame laws) attract it: by which ſudden change of poles, merely upon the change of ſituation, it alſo appeared, that the iron owed its virtue only to the magnetiſm of the earth, not that of another loadſtone, which would not have been thus eaſily alterable. And this experiment I the more particularly relate, becauſe this is not the only place, where I have occaſion to make uſe of it.

CHAP. V.

ANOTHER proof of the great ſubtilty of effluviums may be taken from the ſmall decrement of weight or bulk, that a body may ſuffer by parting with great ſtore of ſuch emanations.

THAT bodies, which infuſed in liquors impregnate them with new qualities ſuitable to thoſe of the immerſed bodies, do ſo by imparting to them ſomewhat of their own ſubſtance, will, I preſume, be readily granted by thoſe, that conceive not, how one body ſhould communicate to another a ſolitary and naked quality, unaccompanied by any thing corporeal to ſupport and convey it. But I would not have you think, *Pyrophilus*, that the only matter of fact I have to countenance this notion, is that experiment, which has convinced divers chemiſts and phyſicians, otherwiſe not friends to the corpuſcular philoſophy, that medicines may operate without any conſumption of themſelves. For though divers of theſe, ſome of them learned men, have confidently written, that glaſs of antimony, and crocus metallorum, being either of them infuſed in a great proportion of wine, will make it vomitive; and if that liquor be poured off, and new be poured on, every new portion of ſuch liquor will be impregnated with the ſame virtue, and this though the liquor be changed a thouſand times, and yet the antimonial glaſs or crocus will continue the ſame, as well in weight as virtue; and though thence ſome of them, eſpecially chemiſts, argue, that ſome metals work without imparting any thing ſubſtantial, but only, as *Helmont* ſpeaks of ſome of his arcana, by irradiation: yet, I confeſs, I have ſome doubts, whether the experiment have been competently tried, and ſhall not fully acquieſce in what has been ſaid, till ſome ſkilful experimenter deliver it upon his own trial, and acquaint us too, with what inſtruments, and what circumſpection he made it. For beſides, that the ingenioufeſt phyſicians I have queſtioned about it, acknowledged the taſte, and ſometimes the colour of the wine, to be altered by the infuſed mineral, I could not acquieſce in the affirmation of an ordinary chemiſt, or apothecary, or even phyſician, if he ſhould barely aver, that he had weighed an antimonial medicine before it was put to infuſe, and after the infuſion ended, and obſerved no decrement of weight. For I have had too much experience (as I elſewhere mention) of the difficulty of making exact ſtatical trials; not to know, that ſuch ſcales, as are wont to be employed by chemiſts and apothecaries in weighing drugs, are by no means fit to make trials with the nicety, which that I am ſpeaking of requires: it being eaſy, even with the better ſort of ſuch unaccurate ſcales, eſpecially if they be not ſuſpended from ſome fixed thing, but held with the hand, to miſtake half a grain, or a grain; and perhaps a greater quantity, and at leaſt more, than by divers of the experiments of this eſſay appears neceſſary to be ſpent upon the impregnating of a conſiderable proportion of liquor, with corporeal effluxions. Beſides that if, when the beaten crocus, or glaſs, be taken out of the wine to be weighed again, the experimenter be not cautious enough to make allowance for the liquor, that will adhere to the medicament, it is plain, that he may take notice of no decrement of weight, though there may be really effluviums of the mineral amounting to ſeveral grains, imbibed by the liquor. And though he be aware of this, and dry the powder, yet it is not ſo eaſy, even for a ſkilful man, to be ſure, that none of the more viſcous particles of the liquor ſtick to the mineral, and being ſenſible upon the balance, though not to the eye or hand, repair the receſs of thoſe emetick corpuſcles, that diffuſed themſelves into the menſtruum. And the ſenſe of theſe difficulties put me upon the attempting to make ſo noble an experiment with excellent ſcales, and the care that it deſerves: but, after a long trial, an unlucky accident fruſtrated at laſt my endeavours. But though, till competent relators give us an account of this matter upon their own trial, and repeat the infuſion very much oftner, than, for aught I find, any man has yet done, I muſt not acquieſce in all, that is ſaid of the impregnation of wine, or other liquors by antimonial glaſs and crocus metallorum; yet, that after divers repeated infuſions, the mineral ſubſtance ſhould not be ſenſibly diminiſhed in bulk or virtue, may well ſuffice to make this inſtance, though not the only or chief, that may be brought for our purpoſe, yet a pertinent one to it. For, that there is a powerful emetick quality imparted to the liquor, is manifeſt by experience; and that the mineral does not impart this virtue, as it were, by irradiation, but by ſubſtantial effluxion, ſeems to me very probable; not only becauſe I conceive not, how this can be done otherwiſe, but becauſe, as it is noted above, the wine does oftentimes change colour by being kept a competent time upon the mineral, as if it drew thence a tincture; and even when it is not diſcoloured, I think it unſafe to conclude, that the menſtruum has not wrought upon it. For I have kept good ſpirit of vinegar, for a conſiderable time, upon finely powdered glaſs of antimony made *per ſe*, without finding the ſpirit to be all tinged, though it is known, that antimonial glaſs is ſoluble in ſpirit of vinegar, as mine afterwards appeared to be, by a longer digeſtion in the ſame liquor. But there may be a great number of minute particles diſſolved in the menſtruum before they be numerous enough to change the colour of it. And with this agrees very well what is obſerved, that though too great a quantity of the prepared antimony be put into the liquor, yet it will not be thereby made too ſtrongly emetick. For the wine, being a menſtruum, will, like other menſtruums, be impregnated but to a certain meaſure, without diſſolving the overplus of the matter, that is put into it; and Mars, which is a harder and heavier body than glaſs of antimony, is it ſelf in part ſoluble in good Rheniſh or other white wine, (and that in no long time,) and ſometimes even in water.

I do not therefore reject the emetick infuſion, as unfit to have a place in this chapter, but till the experiment have been a little more accurately made, I think it inferiour, as to our purpoſe, to ſome of the inſtances to be met with in the next chapter, and perhaps alſo to that mentioned by *Helmont*, and tried by more than one of my acquaintance, concerning the virtue of killing worms, that mercury imparts to the water or wine, wherein it has been long enough infuſed, or elſe for a while decocted. Though quickſilver given in ſubſtance is commended as an effectual medicine againſt worms, not only by many profeſſed * ſpagyriſts, but by divers † methodiſts of good note. And though ſome other things, chemical and philoſophical, keep me from being of their opinion, who think, that in this caſe the mercury impregnates the liquor, as it were, by irradiation, rather than in a corporeal manner; yet the eye does not perceive, that even limpid water takes any thing from clean and well-purged mercury, which we know, that divers corroſive liquors themſelves will not work upon.

To this inſtance I muſt add one, that is yet freer from exceptions, which is, that having for curioſity ſake ſuſpended in a pair of exact ſcales, that would turn with a very ſmall part of a grain, a piece of ambergreeſe bigger than a walnut, and weighing betwixt an hundred and ſix ſcore grains, I could not in three days and a half, that I had opportunity to make the trial, diſcover, even upon that balance, any decrement of weight in the ambergreeſe; though ſo rich a perfume, lying in the open air, was like in that time to have parted with good ſtore of odoriferous ſteams. And a while after ſuſpending a lump of aſſa fœtida five days and a half, I found it not to have ſuſtained any diſcernible loſs of weight, though, in ſpite of the unfavourable cold weather, it had about it a neighbouring atmoſphere repleniſhed with fœtid exhalations. And when twelve or fourteen hours after, perhaps upon ſome change of weather, I came to look upon it, though I found, that in that time the æquilibrium was ſomewhat altered, yet the whole lump had not loſt half a quarter of a grain; which induced me to think, that there may perhaps be ſteams diſcernible even by our noſtrils, that are far more ſubtil than the odorous exhalations of ſpices themſelves. For having, in very good ſcales, ſuſpended in the month of *March* an ounce of nutmegs, it loſt in about ſix days five grains and a half. And an ounce of cloves, in the ſame time, loſt ſeven grains and five-eighths.

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* As Quercetanus, Libavius, Zabata, Burggravius.

† As Vidius, Paræus, Cæſalpinus, &c.

You will perhaps wonder, why I do not prefer, to the inſtances I make mention of in this chapter, that, which may be afforded by the load-ſtone, that is acknowledged continually to emit multitudes of magnetical ſteams without decrement of weight. But though I have not thought fit to paſs this wholly under ſilence, yet I forbear to lay ſo much ſtreſs on it, not only becauſe my balances have not yet ſatisfied me about the effluvia of load-ſtones, (for I take them not all to be equally diffuſive of their particles,) but becauſe I foreſee it may be doubted, whether load-ſtones, like odorous bodies, do furniſh afreſh of their own, all the corpuſcles, that from time to time iſſue from them; or, whether they be not continually repaired, partly by the return of the magnetical particles to one pole, that ſallied out of the other; and partly by the continued paſſage of magnetical matter, ſupplied by the earth, or other mundane bodies, which make the pores or channels of the load-ſtone their conſtant thorough-fares.

I doubt not but it will make it more probable, that a ſmall quantity of matter being ſcattered into inviſible effluvia, may be exceedingly rarified and expanded, if it can be made appear, that this little portion of matter ſhall, for a conſiderable time, emit multitudes of viſible parts, and that in ſo cloſe an order among themſelves, as to ſeem in their aggregate but one entire liquor, endowed with a ſtream-like motion, and a diſtinct ſuperficies, wherein no interruption is to be ſeen, even by an eye placed near it. To deviſe this experiment, I was induced, by conſidering, that hitherto all the total diſſolutions, that have been made of pigments, have been in liquors naturally cold, and conſiſting probably of much leſs ſubtil, and certainly of much leſs agitated parts, than that fluid aggregate of ſhining matter, that we call flame; whereas I argued, that if one could totally diſſolve a body compoſed of parts ſo minute, as thoſe of a metal, into actual flame, and huſband its flame ſo, as that it ſhould not immoderately waſte, I ſhould thereby diſſolve the metal in a far more ſubtil menſtruum than our common water, or aqua fortis, or aqua regis, or any other known menſtruum I have yet employed. And conſequently, the attenuation and expanſion of the metal in this truly igneous menſtruum would much ſurpaſs, not only what happens in ordinary metalline ſolutions, but poſſibly alſo what I have noted in the third chapter of this eſſay, about the ſtrange diffuſion of copper diſſolved in ſpirit of urine and water. In proſecution of this deſign, I ſo prepared one ſingle grain of that metal, by a way, that I elſewhere teach, that it was diſſolved in about a ſpoonful of an appropriated menſtruum. And then having cauſed a ſmall glaſs lamp to be purpoſely blown to contain this liquor, and fitted it with a ſocket and wick, we lighted the lamp, which, without conſuming the wick, burnt with a flame large enough, and very hot, and ſeemed to be all the while of a greeniſh blue, as if it were but a finer and ſhining ſolution of copper. And yet this one grain of prepared metal tinged the flame, that was from moment to moment produced, during no leſs than half an hour and ſix minutes. And now if we conſider, that in this flame there was an uninterrupted ſucceſſion of multitudes of coloured particles newly extricated, and flying off in every of thoſe many parts wherein a minute of time may either actually or mentally be divided; and if we conſider flame as a light and very agitated body, paſſing with a ſtream upwards through the air, and if we alſo conſider the quantity of liquor, that would (as I ſhall by and by tell you) run through a pipe of a much leſſer diameter than that flame, within the compaſs of the fore-mentioned time: what a quantity of the ſtreaming fluid, we call flame, if it could have been preſerved, and collected into one body, may we ſuppoſe, would appear to have iſſued out of one grain of copper in the ſpace of thirty-ſix minutes; and what a multitude of metalline corpuſcles may we ſuppoſe to have been ſupplied for the tinging of that flame, during ſo long a time? ſince a cylindrical ſtream of water falling but through a very ſhort pipe of glaſs, conſtantly ſupplied with liquors, did paſs at ſuch a rate, that though the aqueous cylinder ſeemed more ſlender by half, or perhaps by two-thirds, or better, than the flame, yet we eſtimated, by the help of a minute-watch, and a good pair of ſcales that, if I had had conveniencies to let it run long enough, the water effluxed in thirty-ſix minutes, the time of the flame's duration, would have amounted to above nine gallons, or, reckoning a pint of water to contain a pound of ſixteen ounces, ſeventy-two pounds.

CHAP. VI.

THE laſt ſort of inſtances I ſhall propoſe to ſhew the ſtrange ſubtilty of effluvia, is of ſuch as diſcover the great quantity of ſpace, that may, by a ſmall quantity of matter, when rarified or diſperſed, be either filled as to ſenſe, or, at leaſt, made (as they ſpeak) the ſphere of its activity.

To manifeſt this truth, and thereby as well confirm the foregoing chapter, as make out what is deſigned in this, I ſhall endeavour to ſhew, and help your imagination to conceive, how great a ſpace may be impregnated with the effluxions of a body, oftentimes without any ſenſible, and oftener without any conſiderable decrement in bulk, or weight, of the body that affords them. And in order to this, though I ſhall not pretend to determine preciſely how little the ſubſtances, I am to inſtance in, would waſte upon the baſance, becauſe you will very eaſily ſee, they are not that way to be examined; yet I preſume, you will as eaſily grant, that the decrement of weight would be but inconſiderable, ſince, of ſuch light ſubſtances, the loſs even of bulk is ſo; which laſt clauſe I ſhall now attempt to make good, by ſetting down ſome obſervations, partly borrowed from the writings of approved phyſicians, and partly, that my friends and I have made about the durable evaporation of ſuch ſmall particles of the effluxions of animals, as are actually not to be diſcerned by the eye to have any of thoſe things ſticking to them, which are ſo very long in flying ſucceſſively away.

It is wont to be ſomewhat ſurprizing to men of letters, when they firſt go a hawking with good ſpaniels, to obſerve, with how great ſagacity thoſe dogs will take notice of, and diſtinguiſh by the ſcent, the places where partridges, quails, &c. have lately been. But I have much more wondered at the quick ſcent of an excellent ſetting-dog, who, by his way of ranging the fields, and his other motions, eſpecially of his head, would not only intimate to us the kinds of game, whoſe ſcent he chanced to light on, but would diſcover to us where partridges have been, though perhaps without ſtaying in that place, ſeveral hours before, and aſſiſt us to gueſs how long they had been gone before we came.

I have had ſtrange anſwers given me in *Ireland*, by thoſe, who make a gain, if not an entire livelihood, by killing of wolves in that country, (where they are paid ſo much for every head they bring in) about the ſagacity of that peculiar race of dogs they employ in hunting them; but not truſting much to thoſe relators, I ſhall add, that a very ſober and diſcreet gentleman of my acquaintance, who has often occaſion to employ blood-hounds, aſſures me, that if a man have but paſſed over a field, the ſcent will lie, as they ſpeak, ſo as to be perceptible enough to a good dog of that ſort for ſeveral hours after. And an ingenious hunter aſſures me, that he has obſerved, that the ſcent of a flying, and heated deer, will ſometimes continue upon the ground from one day to the next following.

And now we may conſider theſe three things; firſt, that the ſubſtance left upon the graſs, or ground, by the tranſient tread of a partridge, hare, or other animal, that does but paſs along his way, does probably communicate to the graſs, or ground, but ſome of thoſe effluxions, that tranſpire out of his feet, which being ſmall enough to eſcape the diſcernment of the eye, may probably not amount to one grain in weight, or perhaps not to the tenth part of it. Next, that the parts of fluid bodies, as ſuch, are perpetually in motion, and ſo are the inviſible particles, that ſwim in them, as may appear by the diſſolution of ſalt, or ſugar, in water, and the wandering of aqueous vapours through the air, even when the eye perceives them not. And thirdly, that though the atmoſphere of one of theſe ſmall parcels of the exhaling matter we are ſpeaking of may oftentimes be exceeding vaſt, in compariſon of the emittent body, as may be gueſſed by the diſtance, at which ſome ſetters, or blood-hounds, will find the ſcent of a partridge, or deer; yet in places expoſed to the fre eair, or wind, it is very likely, that theſe ſteams are aſſiduouſly carried away from their fountain, to maintain the fore-mentioned atmoſphere for ſix, eight, or more hours, that is, as long as the ſcent has been obſerved to lie, there will be requiſite a continual recruit of ſteams ſucceeding one another: and, that ſo very ſmall a portion of matter, as that, which we were ſaying the *fomes* of theſe ſteams may be judged to be, being ſenſibly to impregnate an atmoſphere incomparably greater than itſelf, and ſupply it with almoſt continual recruits, we cannot but think, that the ſteams it parts with, muſt be of an extreme, and ſcarce conceivable minuteneſs.

And we may further conſider, that the ſubſtances, which emit theſe ſteams, being ſuch as newly belonged to animals, and were, for the moſt part, tranſpired through the pores of their feet, muſt be in likelihood a far more evaporable and diſſipable kind of bodies, than minerals or aduſt vegetables, ſuch as gunpowder is made of; ſo that if the grains of gunpowder emit effluviums capable of being, by ſome animals, perceived at a diſtance by their ſmell, one may probably ſuppoſe, that the ſmall grains of this powder may hold out very many times longer to ſupply an atmoſphere with odorable ſteams, than the corpuſcles left on the ground by tranſient animals.

Now though it be generally agreed on, that very few birds have any thing near ſo quick a ſenſe of ſmelling, as ſetting-dogs, or blood-hounds, yet, that the odour of gunpowder, eſpecially when aſſiſted by the ſteams of the caput mortuum of powder formerly fired in the ſame gun, may by fowls be ſmelled at a notable diſtance, particularly when the wind blew from me towards them, I often perſuaded myſelf I obſerved, eſpecially as to crows, when I went a ſhooting; and was confirmed in that opinion, both by the common tradition, and by ſober and ingenious perſons much exerciſed in the killing of wild-fowl, and of ſome four-footed beaſts.

I had forgotten to take notice of one obſervation of the experienced *Julius Palmarius*: whence we may learn, that beaſts may leave upon the vegetables, that have touched their bodies for any time, ſuch corpuſcles, as, though unheeded by other animals, may, when eaten by them, produce in them ſuch diſeaſes as the infected animals had. For this author writes, in his uſeful tract *De morbis contagioſis*, that he obſerved horſes, beeves, ſheep, and other animals, to run mad upon the eating of ſome of the ſtraw on which ſome mad ſwine had lain.

AND now to reſume and proſecute our former diſcourſe, you may take notice, that the effluvia, mentioned to have been ſmelt by animals, are, though inviſible, yet big enough to be the objects of ſenſe; ſo that it is not improbable, that among the ſteams, that no ſenſe can immediately perceive, there ſhould be ſome far more ſubtil than theſe, and conſequently capable of furniſhing an atmoſphere much longer, without quite exhauſting the effluviating matter, that afforded them

Lib. VI. Obſer. 22. FORESTUS, an uſeful author, recites an example of peſtilential contagion long preſerved in a cobweb.

ALEXANDER BENEDICTUS writes alſo, that at *Venice* a flock-bed did for many years harbour a peſtiferous malignity to that degree, that when afterwards it came to be beaten, it preſently infected the by-ſtanders with the plague.

Lib. IV. de Feb. cap. 3. AND the learned *Sennertus* himſelf relates, that in the year one thouſand five hundred and forty-two, there did in the city of *Uratiſlavia*, vulgarly *Breſlaw*, where he afterwards practiſed phyſick, die of the plague, in leſs than ſix months, little leſs than ſix thouſand men, and that from that time, the peſtilential contagion was kept folded up in a linen cloath about fourteen years, and at the end of that time being diſplayed in another city, it began a plague there, which infected alſo the neighbouring towns, and other places.

Lib III. Con. 17. TRINCAVELLA makes mention of a yet laſtinger contagion, which occaſioned the death of ten thouſand perſons, that lay lurking in certain ropes, with which, at *Juſtinopolis*, thoſe, that died of the plague, had been let down into the graves.

BUT though none of theſe relations ſhould, to ſome criticks, appear ſcarce credible, it may be objected, that all theſe things, wherein this contagion reſided, were kept cloſe ſhut up, or at leaſt were not expoſed to the air. Wherefore having only intimated, that the exception, which I think is not irrational, would, though never ſo true, but leſſen the wonder of theſe ſtrange relations, without rendering them unfit for our preſent purpoſe, I ſhall add, that though it is the opinion of divers learned phyſicians, that the matter harbouring contagion cannot laſt above twenty, or a few more days, if the body it adheres to be expoſed to the free air and the wind; and though I am not forward to deny, that their judgment may hold in ordinary caſes; yet I muſt not deny neither, that a contagion may ſometimes happen to be much more tenacious, and obſtinate: of which I ſhall give but that one, almoſt recent inſtance, obſerved by the learned *Diemerbroek*, in his own apothecary, who having but removed with his foot, from one ſide to the other of a little arbour in his garden, ſome ſtraw, that had lain under the pallet, on which near eight months before a bed had lain, wherein a ſervant of the apothecary's, that recovered, had been ſick of the plague; the infectious ſteams preſently invaded the lower part of his leg, and produced a pungent pain and bliſter, which turned to a peſtilential carbuncle, that could ſcarce be cured in a fortnight after, though, during that time, the patient were neither feveriſh, nor, as to the reſt of his body, ill at eaſe. This memorable inſtance, together with ſome others of the like kind, that our author obſerved in the ſame city of *Nimmeguen*, obtained, not to ſay, extorted, even from him, this confeſſion; which I add, becauſe it contains ſome conſiderable, and not yet mentioned circumſtances of the recited caſe: *Hoc exemplo medicorum doctrina de contagio in fomite latente ſatis confirmatur. Mirum tamen eſt, hoc contagium tanto tempore in prædicto ſtramine potuiſſe ſubſiſtere, utpote quod tota hyeme ventis & pluviis,* (he adds in another place) *nivibus & frigori, expoſitum fuiſſet.*

Lib. IV. de Peſte.

AND now I will ſhut up this chapter with an inſtance, that ſome will think, perhaps, no leſs ſtrange than any of the reſt; which is, that though they, that are ſkilful in the perfuming of gloves, are wont to imbue them with but an inconſiderable quantity of odoriferous matter, yet I have by me a pair of *Spaniſh* gloves, which I had by the favour of your fair and virtuous ſiſter (*F.*) that were ſo ſkilfully perfumed, that partly by her, partly by thoſe, that preſented them her as a rarety, and partly by me, who have kept them ſeveral years, they have been kept about eight or nine and twenty years, if not thirty, and they are ſo well ſcented, that they may, for aught I know, continue fragrant divers years longer. Which inſtance if you pleaſe to reflect upon, and conſider, that ſuch gloves cannot have been carried from one place to another, or ſo much as uncovered, as they muſt often have been, in the free air, without diffuſing from themſelves a fragrant atmoſphere, we cannot but conclude thoſe odorous ſteams to be unimaginably ſubtile, that could for ſo long a time iſſue out, in ſuch ſwarms, from a little perfumed matter lodged in the pores of a glove, and yet leave it richly ſtocked with particles of the ſame nature; though, eſpecially by reaſon of ſome removes, in which I took not the gloves along with me, I forgot ever ſince I had them, to keep them ſo much as ſhut up in a box.

OF

OF THE GREAT EFFICACY OF EFFLUVIUMS.

CHAP. I.

THEY, that are wont, in the eſtimates they make of natural things, to truſt too much to the negative informations of their ſenſes, without ſufficiently conſulting their reaſon, have commonly but a very little and ſlight opinion of the power and efficacy of effluviums; and imagine, that ſuch minute corpuſcles (if they grant, that there are ſuch,) as are not, for the moſt part of them, capable to work upon the tendereſt and quickeſt of ſenſes the ſight, cannot have any conſiderable operation upon other bodies. But I take this to be an error, which, as it very little becomes philoſophers, ſo it has done no little prejudice to philoſophy it ſelf, and perhaps to phyſick too. And therefore though the nature of my deſign at preſent did not require it, yet the importance of the ſubject would invite me to ſhew, that this is as ill-grounded as prejudicial a ſuppoſition.

AND indeed if we conſider the ſubject attentively, we may obſerve, that though it be true, that, *cæteris paribus*, the greatneſs of bodies doth, in moſt caſes, contribute to that of their operation upon others, yet matter or body being, in its own preciſe nature, an unactive or moveleſs ſubject, one part of the maſs acts upon another, but upon the account of its local motion, whoſe operations are facilitated and otherwiſe diverſified by the ſhape ſize, ſituation and texture both of the agent and of the patient. And therefore if corpuſcles, though very minute, be numerous enough, and having a competent degree of motion, even theſe ſmall particles, eſpecially if fitly ſhaped, when they chance to meet with a body, which the congruity of its texture diſpoſes to admit them at its pores, and receive their either friendly or hoſtile impreſſions, may perform ſuch things in the patient, as viſible and much groſſer bodies, but leſs conveniently ſhaped and moved, would be utterly unable on the ſame body to effect.

AND that you may with the leſs difficulty allow me to ſay, that the effluviums of bodies, as minute as they are, may perform conſiderable things, give me leave to obſerve to you, that there are at leaſt ſix ways, by which the effluviums of a body may notably operate upon another; namely, 1. By the great number of emitted corpuſcles. 2. By their penetrating and pervading nature. 3. By their celerity, and other modifications of their motion. 4. By the congruity and incongruity of their bulk and ſhape to the pores of the bodies they are to act upon. 5. By the motions of one part upon another, that they excite or occaſion in the body they work upon, according to its ſtructure. And 6. by the fitneſs and power they have to make themſelves be aſſiſted, in their working, by the mere catholic agents of the univerſe. And though it may perhaps be ſufficiently proved, that there are ſeveral caſes wherein a body, that emits particles, may act notably upon another body, by this or that ſingle way, of thoſe I have been naming; yet uſually the great matters are performed by the aſſociation of two, three, or more of them, concurring to produce the ſame effect. Upon which ſcore, when I ſhall in the following paper refer an inſtance, or a phænomenon, to any one of the forementioned heads, I deſire to be underſtood as looking upon that but as the head, to which it chiefly relates, without excluding the reſt.

CHAP. II.

TAKING thoſe things for granted, that have, I hope, been ſufficiently proved in the former tract about the ſubtilty of effluviums, I ſuppoſe it will readily be allowed, that the emanations of a body may be extremely minute; whence it may be rightly inferred, that a ſmall portion of matter may emit great multitudes of them.

NOW, that the great number of agents may in many caſes compenſate their littleneſs, eſpecially where they act, or reſiſt *per modum unius*, as they ſpeak, men would perhaps the more eaſily grant, if they took notice to this purpoſe of ſome familiar inſtances.

WE ſee, that not only leſſer land-floods, that overflow the neighbouring fields, but thoſe terrible inundations, that ſometimes drown whole countries, are made by bodies ſingly ſo ſmall and inconſiderable as drops of rain, when they continue to fall in thoſe multitudes we call ſhowers.

So the aggregates of ſuch minute bodies as grains of ſand, being heaped together in ſufficient numbers, make banks, wherewith greateſt ſhips are ſometimes ſplit, nay, and ſerve in moſt places for bounds to the ſea it ſelf.

AND though a ſingle corn of gun-powder, or two or three together, are not of force to do much miſchief, yet two or three barrels of thoſe corns, taking fire together, are able to blow up ſhips and houſes, and perform prodigious things.

But inſtead of multiplying ſuch Inſtances, afforded by bodies of ſmall indeed, but yet viſible bulk, I ſhall (as ſoon as I have intimated, that the above-mentioned drops of rain themſelves conſiſt of convening multitudes of vapours moſt commonly inviſible in their aſcent) endeavour to make out what was propoſed, by two or three inſtances drawn from the operations of inviſible particles.

And firſt, we ſee, that though aqueous vapours be looked upon as the fainteſt and leaſt active effluviums, that we know of; yet when multitudes of them are in rainy weather diſperſed thorough the air, and are thereby qualified to work on the bodies expoſed to it, their operations are very conſiderable, not only in the diſſolution of ſalts, as ſea-ſalt, ſalt of tartar, *&c.* and in the putrefactive changes they produce in many bodies, but in the intumeſcence they cauſe in oak and other ſolid woods; as appears by the difficulty we often find in and before rainy weather, to ſhut and open doors, boxes, and other wooden pieces of work, that were before fit enough for the cavities they had been adjuſted to.

I might here urge, that though the ſtrings of viols and other muſical inſtruments are ſometimes ſtrong enough to ſuſtain conſiderable weights, yet if they be left ſcrewed to their full tenſion, (as it frequently happens) they are oftentimes, by the ſupervening of moiſt weather, made to break, not without impetuoſity and noiſe. But it may ſuit better with my preſent aim, if I mention on this occaſion, (what I elſewhere more fully take notice of,) being deſirous to try, what a multitude even of aqueous ſteams may do, I cauſed a rope, that was long, but not thick, and was in part ſuſtained by a pully, to have a weight of lead ſo faſtened to the end of it, as not to touch the ground, and after the weight had leiſure allowed it to ſtretch the cord as far as it could, I obſerved, that in the moiſt weather the wateriſh particles, that did inviſibly abound in the air, did ſo much work upon and ſhorten the rope, as to make it lift up the hanging weight, which was, if I miſ-remember not, about an hundred pounds.

The inviſible ſteams, iſſuing out of the walls of a newly plaiſtered or whited room, are not ſenſibly prejudicial to thoſe, that do but tranſiently viſit it, or make but a very ſhort ſtay in it, though there be a charcole-fire in the chimney; but we have many inſtances of perſons, that by lying for a night in ſuch rooms, have been the next morning, or ſooner, found dead in their beds, being ſuffocated by the multitude of the noxious vapours emitted during all that time.

And here I think it proper to obſerve, that it may much aſſiſt us to take notice of the multitude of effluvia, and make us expect great matters from them, to conſider, that they are not emitted from the body, that affords them all at once, as hail-ſhot out of a gun, but iſſue from it, as the vaporous winds do out of an æolipile well heated, or waters out of a ſpring-head in continued ſtreams, wherein freſh parts ſtill ſucceed one another; ſo that though as many effluxions of a body, as can be ſent out at one time, were numerous enough to act but upon its ſuperficial parts, yet the emanation of the next minute may get in a little farther, and each ſmalleſt portion of time ſupplying freſh recruits, and perhaps urging on the ſteams already entered, the particles may at length get into a multitude of the pores of the invaded body, and penetrate it to the very innermoſt parts.

CHAP. III.

I COME now to ſhew, in the ſecond place, that the ſubtile and penetrating nature of effluviums may, in many caſes, co-operate with their multitude in producing notable effects; and that there are effluviums of a very piercing nature, though we ſhall not now enquire upon what account they are ſo, we may evince by ſeveral examples. For not only the inviſible ſteams of good aqua-fortis and ſpirit of nitre do uſually in a ſhort time, and in the cold, ſo penetrate the corks, wherewith the glaſſes, that contained them, were ſtopped, as to reduce them into a yellow pap; but alſo the emanations of mercury have been ſometimes found in the form of coagulated, or even of running mercury in the heads or very bones of thoſe gilders, or venereal patients, that have too long, or too unadviſedly, been expoſed to the fumes of it, though they never took quickſilver in its groſs ſubſtance. Chemiſts too often find in their laboratories, that the ſteams of ſulphur, antimony, arſenick, and divers other minerals, are able to make thoſe ſtagger, or perhaps ſtrike them down, that without a competent warineſs unlute the veſſels, wherein they had been diſtilled or ſublimed; of which I have known divers ſad examples. And of the penetrancy, even of animal ſteams, we may eaſily be perſuaded, if we conſider, how ſoon in many plagues the contagious, though inviſible, exhalations are able to reach the heart, or infect other internal parts; though in divers of theſe caſes the blood helps to convey the infection, yet ſtill the morbifick particles muſt get into the body, before they can infect the maſs of blood. And in thoſe ſtupefactions, that are cauſed at a diſtance by the torpedo, the parts mainly affected ſeem to be the nervous ones of the hand and arm, which are of the moſt retired and beſt fenced parts of thoſe members. And there is a ſpirit of ſal armoniack, that I make to ſmell to, whoſe inviſible ſteams, unexcited by heat, are of ſo piercing a nature, that not only they will powerfully affect the eyes and noſtrils, and throats, and ſometimes the ſtomachs too (yet without proving vomitive) of the patients they invade, but alſo when a great cold has ſo clogged the organs of ſmelling, that neither ſweet nor ſtinking odours would at all affect them, theſe piercing ſteams have not only in a few minutes both made themſelves a way, and, which is more, ſo opened the paſſages, that ſoon after the patient has been able to ſmell other things alſo. And by the ſame penetrating ſpirit, a perſon of quality was, ſome time ſince, reſtored to a power of ſmelling, which he had loſt for divers years, (if he ever had it equally with other men.) I could eaſily ſubjoin examples of this kind, but they belong to other places. And here I ſhall only add, that the ſteams of water itſelf, aſſiſted by warmth, are capable of diſſolving the texture of even hard and ſolid bodies, that are not ſuſpected to be ſaline; as appears by the philoſophical calcination (as chemiſts call it) wherein ſolid pieces of harts-horn are brought to be eaſily friable into powder, by being hung over waters, whilſt their ſteams riſe in diſtillation and without the help of furnaces. The exhalations, that uſually ſwim every night in the air, and almoſt every night fall to the ground in the form of dews (which makes them be judged aqueous) are in many places of the torrid zone of ſo penetrating a nature, that, as eye-witneſſes have informed me, they would, in a very ſhort time, make knives ruſt in their ſheaths, and ſwords in their ſcabbards, nay, and watches in their caſes, if they did not conſtantly carry them in their pockets. And I have known even in *England* divers hard bodies, into which the vapours ſwimming in the air have inſinuated themſelves ſo far, as to make them friable throughout. But of the penetration of effluviums, I have given, in ſeveral places, ſo many inſtances, that it is not neceſſary to add any here. And therefore to ſhew, that, as I intimated at the beginning of this chapter, the penetrancy and the multitude of effluviums may much aſſiſt each other, I ſhall now ſubjoin; that we muſt not for the moſt part look upon effluviums, as ſwarms of corpuſcles, that only beat againſt the outſides of the bodies they invade, but as corpuſcles, which by reaſon of their great and frequently recruited numbers, and by the extreme ſmallneſs of their parts, inſinuate themſelves in multitudes into the minute pores of the bodies they invade, and often penetrate to the innermoſt of them; ſo that, though each ſingle corpuſcle, and its diſtinct action, be inconſiderable, in reſpect of the multitude of parts, that compoſe the body to be wrought on; yet a vaſt multitude of theſe little agents working together upon a correſpondent number of the ſmall parts of the body they pervade, they may well be able to have powerful effects upon the body, that thoſe parts conſtitute; as, in the caſe mentioned in the former chapter, the rope would not probably have been enabled to raiſe ſo great a weight, though a vehement wind had blown againſt it, to make it loſe its perpendicular ſtreightneſs, but a vaſt multitude of watery particles, getting by degrees into the pores of the rope, might, like an innumerable company of little wedges, ſo widen the pores, as to make the thrids or ſplinters of hemp, the rope was made up of, ſwell, and that ſo forcibly, that the depending weight could not hinder the ſhortening of the rope, and therefore muſt of neceſſity be raiſed thereby. And I have more than once known ſolid, and even heavy mineral bodies, burſt in pieces by the moiſture of the air, though we kept them within-doors carefully ſheltered from the rain.

CHAP. IV.

THAT the celerity of the motion of very minute bodies, eſpecially conjoined to their multitudes, may perform very notable things, may be argued from the wonderful effects of fired gunpowder, *aurum fulminans*, of flames, that inviſibly touch the bodies they work on, and alſo whirlwinds, and thoſe ſtreams of inviſible exhalations and other aereal particles we call winds. But becauſe inſtances of this ſort ſuit not ſo well with the main ſcope of this tract, I ſhall not inſiſt on them, but ſubjoin ſome others, which, though leſs notable in themſelves, will be more congruous to my preſent deſign. That the corpuſcles, whereof odours conſiſt, ſwim to and fro in the air, as in a fluid vehicle, will by moſt, I preſume, be granted, and may be eaſily proved. But I have elſewhere ſhewn, that the motion of the effluviums of ſome ſufficiently odorous bodies has too little celerity to make a ſenſible impreſſion on the organs of ſmelling, unleſs thoſe ſteams be aſſiſted to beat more forcibly upon the noſtrils by the air, which hurries them along with it, when it enters the noſtrils in the form of a ſtream, in the act of inſpiration. And I have by familiar obſervation of hunters, fowlers, and partly of my own, made manifeſt, that ſetting-dogs, hounds, crows, and ſome other animals, will be much more affected with ſcents, or the odorous effluvia of partridges, hares, gunpowder, &c. when the wind blows from the object towards the ſenſory, than when it ſits the contrary way, which way ſoever the noſtrils of the animal be obverted, ſo the air be imbued with the odorous ſteams: and conſequently the difference ſeems to proceed from this, that when the noſtrils are obverted to the wind, the current of the air drives the ſteams forcibly upon the ſenſory, which otherwiſe it does not.

THAT there is a briſkneſs of motion requiſite, and more than ordinarily conducive to electrical attractions, may be argued from the neceſſity, that we uſually find by rubbing amber, jett, and other electrical bodies, to make them emit thoſe ſteams, by which it is highly probable their action is performed: and though I have elſewhere ſhewn, that this precedent rubbing is not always neceſſary to excite all electrical bodies; yet in thoſe, that I made to attract without it, it would operate much more vigorouſly after attrition; which I conceive makes a reciprocal motion amongſt the more ſtable parts, and does thereby, as it were, diſcharge and ſhoot out the attracting corpuſcles; whoſe real emiſſion, though it may be probably argued from what has been already ſaid, ſeems more ſtrongly provable by an obſervation, that I made many years ago, and which I have been lately informed to have been long ſince made by the very learned *Fabri*. The obſervation was this; that if, when we took a vigorouſly excited electrick, we did at a certain nick of time (which circumſtances may much vary, but was uſually almoſt as ſoon as the body was well rubbed) place it at a juſt diſtance from a ſuſpended hair, or other light body, or perhaps from ſome light powder; the hair, &c. would not be attracted to the electrick, but driven away from it, as it ſeemed, by the briſkly moving ſteams, that iſſue out of the amber, or other light body.

This argument I could confirm by another phænomenon or two, of affinity with this, if I ſhould not borrow too much of what I have elſewhere noted about the hiſtory of electricity.

I know a certain ſubſtance, which, though made by diſtillation, does in the cold emit but a very mild and inoffenſive ſmell, but when the veſſel, that holds it, is heated, though no ſeparation of conſtituent principles appear to be thereby made, (the body being in all uſual trials homogeneous) the effluviums will be ſo altered, that I remember a virtuoſo, that, to ſatiſfy his curioſity, would needs be ſmelling to it, when it was heated, complained to me, that he thought the ſteams would have killed him, and that the effluviums of ſpirit of ſal armoniack itſelf were nothing near ſo ſtrong and piercing as thoſe.

And even among ſolid bodies, I know ſome, which, though abounding much in a ſubſtance, wherein ſome rank ſmells principally reſide, yet (if they were not chafed) were ſcarce at all ſenſibly odorous; but upon the rubbing of them a little one againſt the other, the attrition making them, as it were, dart out their emiſſions, would in a minute or two make them ſtink egregiouſly.

And as the celerity of motion may thus give a vigour to the emanations of bodies, ſo there may be other modifications of motion, that may contribute to the ſame thing, and are not to be wholly neglected in this place. For as we ſee, that greater bodies do operate differingly, according to ſuch and ſuch modifications; as there is a great difference between the effects of a dart, or javelin, ſo thrown, as that its point be always forwards, and the ſame weapon, if it be ſo thrown, that during its progreſſive motion the extremes turn about the center of gravity, or ſome inward parts, as it happens, when boys throw ſticks to beat down fruit from the tops of trees; ſo there is little doubt to be made, but, that in corpuſcles themſelves, it is not all one, as to their effects, whether they move with, or without rotation, and whether in ſuch or ſuch a line, and whether with, or without undulation, trembling, or ſuch a kind of conſecution; and in ſhort, whether the motion have, or have not this or that particular modification; which, how much it may diverſify the effects of the bodies moved, may appear by the motion, that the aerial particles are put into by muſical inſtruments. For though the effects of harmony, diſcord, and peculiar ſounds, be ſometimes very great, not only in human bodies, but, as we ſhall ſhew in the following tract, in organical ones too; the whole efficacy of muſick, and of ſounds, that are not extraordinarily loud and different, ſeems, as far as it is aſcribable to ſonorous bodies, to depend upon the different manners of motion, whereinto that air is put, that makes the immediate impreſſion on our organs of hearing.

CHAP. V.

I Should now proceed to ſhew, how the celerity and other modes, that diverſify the motion of effluviums, may be aſſiſted to make them operative by their determinate ſizes and figures, and the congruity, or incongruity, which they may have upon that ſcore, with the pores of the groſſer bodies they are to work on: but I think it not fit to entrench upon the ſubject of another * tract, where the relation between the figures of corpuſcles, and the pores of groſſer bodies, is amply enough treated of. And therefore I ſhall only, in this place, take notice of thoſe effects of lightning, which ſeem referrible, partly to the celerity and manner of appulſe, and partly to the diſtinct ſizes and ſhapes of the corpuſcles, that compoſe the deſtructive matter, and to the peculiar relation between the particles of that matter, and the ſtructure of the bodies they invade. I know, that many ſtrange things, that are delivered about the effects of what the Latins call *fulmen*, which our Engliſh word lightning does not adæquately render, are but fabulous; but there are but too many, that are not ſo; ſome of which I have been an eye-witneſs of, within leſs than a quarter of an hour after, that the things happened. And though it be very difficult to explicate particularly many of theſe true phænomena, yet it ſeems warrantable enough to argue from them, that there may be agents ſo qualified, and ſo ſwiftly moved, that notwithſtanding their being ſo exceedingly minute, as they muſt be, to make up a flame, which is a fluid body, they muſt, in an imperceptible time, pervade ſolid bodies, and traverſing ſome of them, without violating their texture, burn, break, melt, and produce other very great changes in other bodies, that are fitted to be wrought on by them. And of this, I muſt not forget to mention this remarkable inſtance; that a perſon, curious enough to collect many rarities, bringing me one day into the ſtudy, where he kept the choiceſt of them, I ſaw there, among other things, a fine pair of drinking-glaſſes, that were ſomewhat ſlender, but extraordinarily tall; they ſeemed to have been deſigned to reſemble one another, and made for ſome drinking entertainment. But before I ſaw them, that reſemblance was much leſſened by the lightning, that fell between them in ſo ſtrange a manner, that, without breaking either of them, that I could perceive, it altered a little the figure of one of them, near the lower part of the cavity; but the other was ſo bent, near the ſame place, as to make it ſtand quite awry, and give it a poſture, that I beheld not without ſome amazement. And I cannot yet but look upon it as a very ſtrange thing, and no leſs conſiderable to our preſent purpoſe, that nature ſhould, in the free air, make of exhalations, and that ſuch as probably, when they aſcended, were inviſible, ſuch an aggregate of corpuſcles, as ſhould, without breaking ſuch frail bodies as glaſſes, be able in its paſſage thorough them, that is, in the twinkling of an eye, to melt them; which to do is wont, even in our reverberatory furnaces, to coſt the active flames a pretty deal of time.

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* Of the Pores of Bodies, and Figures of Corpuſcles.

AND this calls into my memory, that upon a time, hearing not far off from me ſuch a clap of thunder, as made me judge and ſay, that queſtionleſs ſome of the neighbouring places were thunder-ſtruck, I ſent preſently to make enquiry; which having juſtified my conjecture, I forthwith repaired to the houſe, where the miſchief was done by ſomething, which thoſe, that pretended to have ſeen it coming thither, affirmed to be like a flame moved very obliquely. To omit the hurt, that ſeemed to have been done by a wind, that accompanied it, or was perhaps produced by it, to divers perſons and cattle; that, which makes me here mention it, was, that obſerving narrowly what had happened in an upper room, where it firſt fell, I ſaw, that it had, in more than one place, melted the lead in its paſſage, though that poſſibly outlaſted not the twinkling of an eye, without breaking to pieces the glaſs-caſements, or burning, that I took notice of, either the bed, or hangings, or any other combuſtible houſhould-ſtuff; though, near the window, it had thrown down a good quantity of ſolid ſubſtance of the wall, through which it ſeemed to have made its paſſage in or out. And that, which made me the leſs ſcruple to mention this accident, is, that having curiouſly pryed into the effects of the fulmen, not only in that little upper room, but in other parts of the houſe, beneath whoſe lowermoſt parts it ſeemed to have ended its extravagant courſe, I could not but conclude, that if ſo be it were the ſame fulmen, it muſt have more than once gone in and out of the houſe, and that the line of its motion was neither ſtrait, nor yet reducible to any curve, or mixed line, that I had met with among mathematicians; but that, as I then told ſome of my friends, it moved to and fro in an extravagant manner, not unlike the irregular and wriggling motion of thoſe fired ſquibs, that boys are wont to make by ramming gunpowder into quills. But about thunder, more perhaps elſewhere. I ſhall here only add, that whereas it is a known tradition, which my own obſervations heedfully made ſeem now and then to confirm, that vehement thunder, if beer be not very ſtrong, will uſually, (for I do not ſay always,) ſour it in a day or two; if this degeneration be not one of the conſequences of the great and peculiar kinds of the concuſſions of the air, that happens in loud thunder (in which caſe, the phænomenon will belong to the next diſcourſe,) the effect may probably be imputed to ſome ſubtil exhalations diffuſed thorough the air, which, penetrating the pores of the wooden veſſels, whoſe contexture is not very cloſe, imbue the liquor with a kind of acetous ferment; which conjecture I ſhould think much confirmed by a trial, it ſuggeſted to me, if I had made it often enough to rely upon it. For conſidering, that the pores of glaſs are ſtrait enough to be impervous (for aught I have yet obſerved) to the ſteams, or ſpirituous parts of ſulphur, as well as to other odorous exhalations, I thought it worth trying, whether there be any ſulphureous ſteams, or other corpuſcles, diffuſed thorough the air in time of thunder, that would not be too groſs to get in at ſuch minute pores as thoſe of glaſs. And accordingly, having hermetically ſealed up both beer and ale apart, I kept them in ſummer time, till there happened a great thunder, a day or two after which, the beer, which we drank, that was good before, being generally complained of, as ſoured by the thunder, I ſuffered my liquors to continue, at leaſt a day or two longer, that the ſouring ſteams, if any ſuch there were, might have time enough to operate upon them, and then breaking the glaſſes, I found not, that the liquors had been ſoured, though we had purpoſely forborn to fill the glaſſes, to facilitate the degeneration of the liquors. Perhaps it will be pardonable, on this occaſion, to mention a practice, which is uſual in ſome places, where I have been, and particularly employed by a great lady, that is a great houſekeeper, and is very curious and expert in divers phyſical obſervations; for, talking with her about the remedies of the ſouring of beer, and other drinks, by thunder, which is ſometimes no ſmall prejudice to her, ſhe affirmed to me, that ſhe uſually found the practice, I was mentioning, ſucceed; and, that before the then laſt great thunder, of which I had obſerved the effects upon beer, ſhe preſerved hers by putting, at a convenient diſtance, under the barrels, chaffing-diſhes of coals, when ſhe perceived, that the thunder was like to begin, which practice, if it conſtantly ſucceed, may put one a conſidering, whether the fire do not, by rarifying the air, and diſcuſſing the ſulphureous, or other ſteams, by altering them, or by uniting with them the exhalations of the coals, or by ſome ſuch kind of way, render ineffectual theſe ſouring corpuſcles, which perhaps require a determinate bulk and ſhape, beſides their being crowded very many of them together, to have their full operation on barrelled liquors. But theſe things are but meer conjectures, and therefore I proceed.

CHAP. VI.

THE fifth way, whereby effluviums may perform notable things, is the motion of one part upon another, that they may excite, or occaſion in the body they work on, according to its ſtructure.

I ſhall, in the following tract, have occaſion to ſay ſomething of the motions, into which the internal parts of inanimate bodies may put one another; but the examples now produced are deſigned to manifeſt the efficacy, that effluviums may, on the newly-mentioned accounts, have on organical and living bodies. To which inſtances, it would yet be proper to premiſe, that even inanimate and ſolid bodies may be of ſuch a ſtructure, as to be very much alterable by the appropriated effluviums of other bodies, as may be inſtanced in the power, that I have known ſome vigorous load-ſtones to have, of taking away, in a trice, the attractive virtue of an excited needle, or giving a verticity directly contrary to the former, without ſo much as touching it.

And we may pertinently take notice of the attractive virtue of the load-ſtone, as that, which may afford us an eminent example of the great power of a multitude of inviſible effluviums, even from bodies, that are not great, upon bodies, that are inorganical or liveleſs: for taking it for granted, what both the Epicureans, Carteſians, and almoſt all other corpuſcularian philoſophers agree in, that magnetiſm is performed by corporeal emiſſions, we may conſider, that theſe paſſing unreſiſtedly thorough the pores of all ſolid bodies, and even glaſs itſelf, which neither the ſubtileſt odours, nor electrical exhalations, are obſerved to do, ſeem to be almoſt incredibly minute, and much ſmaller than any other effluviums, though themſelves too ſmall to be viſible; and yet theſe ſo incomparably little magnetical effluxions proceeding from vigorous load-ſtones, will be able to take up conſiderable quantities of ſo ponderous a body as iron; inſomuch, that I have ſeen a load-ſtone, not very great, that would keep ſuſpended a weight of iron, that I could hardly lift up to it with one arm; and I have ſeen a little one, with which I could take up above eighty times its weight. And theſe effluvia do not only for a moment faſten the iron to the ſtone, but keep the metal ſuſpended as long as one pleaſes.

This being premiſed, I come now to obſerve, that the chief effects of effluvia belonging to the fifth head are wrought upon animals, which, by virtue of their curious and elaborate ſtructure, have their parts ſo connected, and otherwiſe contrived, that the motions or changes, that are produced in one, may have, by the conſent of parts, a manifeſt operation upon others, although perhaps very diſtant from it, and ſo framed, as to declare their being affected by actions, that ſeem to have no affinity at all with the agents, that work upon the part firſt affected.

I have ſhewn at large, in another * treatiſe, that a human body ought not to be looked upon meerly as an aggregate of bones, fleſh, and other conſiſtent parts, but as a moſt curious, and a living engine, ſome of whoſe parts, though ſo nicely framed, as to be very eaſily affected by external agents, are yet capable of having great operations upon the other parts of the body they help to compoſe. Wherefore, without now repeating what is there already delivered, I ſhall proceed to deliver ſuch effects, as are wrought on human bodies by theſe effluviums, without any immediate contact of the bodies, that emit them.

And firſt, not to mention light, becauſe its being, or not being a corporeal thing, is much diſputed, even among the moderns; it is plain, that our organs of ſmelling are ſenſibly affected by ſuch minute particles of matter as the fineſt odours conſiſt of. Nor do they always affect us preciſely as odours, ſince we ſee, that many perſons, both men and women, are by ſmells, either ſweet or ſtinking, put into troubleſome head-achs.

If it were not almoſt ordinary, it would be more than almoſt incredible, that the ſmell of a pleaſing perfume ſhould preſently produce, in a human body, that immediately before was well and ſtrong, ſuch faintneſſes, ſwoons, loſs of ſenſible reſpiration, intumeſcence of the abdomen, ſeeming epilepſies, and really convulſive motions of the limbs, and I know not how many other frightful ſymptoms, that, by the unſkilful, are often taken for the effects of witchcraft, and would impoſe upon phyſicians themſelves, if their own, or their predeceſſors experience, did not furniſh them with examples of the like phænomena produced by natural means. Thoſe ſymptoms manifeſt, what the conſent of parts may do in a human body, ſince even morbifick odours, if I may ſo call them, by immediately affecting the organs of ſmelling, affect ſo many other parts of the *genus nervoſum*, as oftentimes to produce convulſive motions, even in the extreme parts of the hands and feet.

Nor is the efficacy of effluviums confined to produce hyſterical fits, ſince theſe inviſible particles may be able, and ſometimes as ſuddenly, as perfumes are wont to excite them, to appeaſe them; as I have very frequently, though not with never-failing ſucceſs, tried, by holding a ſpirit, I uſually make of ſal armoniack, under the noſtrils of hyſterical perſons. My remedy did not only often recover, in a trice, thoſe, whoſe fits were but ordinary, but did, more than once, ſomewhat to the wonder of the by-ſtanders, relieve, within a minute or two, perſons of differing ages, and conſtitutions, that were ſuddenly fallen down by fits, that the by-ſtanders judged epileptical, but I, hyſterical.

I attribute the good and evil operations of the fore-mentioned ſteams, rather in general to the conſent of the parts, that make up the *genus nervoſum*, than to any hidden ſympathy, or antipathy, betwixt them and the womb, not only for other reaſons, not proper to be inſiſted on here, but becauſe I have known odours have notable effects even upon men. I know a very eminent perſon, a traveller, and a man of a ſtrong conſtitution, but conſiderably ſanguine, who is put into violent head-achs by the ſmell of muſk. And I remember, that one day being with him, and a great many other men of note, about a publick affair, a man, that had a parcel of muſk about him, having an occaſion to make an application to us, this perſon was ſo diſordered by the ſmell, which to moſt of us was delightful, that, in ſpite of his civility, he was reduced to make us an apology, and ſend the perfumed man out of the room; notwithſtanding whoſe receſs, this perſon complained to me, a good while after, of a violent pain in his head, which I perceived had ſomewhat unfitted him for the tranſaction of the affair, whereof he was to be the chief manager. I know another perſon, whoſe happy muſe hath juſtly made him many admirers, that is ſubject to the head-ach upon ſo mild a ſmell, as that of damaſk roſes, and ſometimes even of red roſes; inſomuch, that walking one day with him in a garden, whoſe alleys were very large, ſo that he might eaſily keep himſelf at a diſtance from the buſhes, which bore many of them red roſes; he abruptly broke off the diſcourſe we were engaged in, to complain of the harm the perfume did his head, and deſired me to paſs into a walk, that had no roſes growing near it.

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* The Uſefulneſs of Experimental Philoſophy.

If it were not for the ſex of the perſon, I could relate an inſtance, that would be much more conſiderable, of the operation of roſes. For I know a diſcreet lady, to whom their ſmell is not unpleaſing, (for ſhe anſwered me, that it was not ſo at all) but ſo hurtful, that it preſently makes her ſick, and would make her ſwoon, if not ſeaſonably prevented: and ſhe told me, that being once at a court, in which ſhe was a maid of honour, though ſhe herſelf did not know whence it came, ſhe found herſelf extremely ill on a ſudden, and ready to ſink down for faintneſs; but being then in diſcourſe with a perſon, whoſe high quality ſhe payed her profound reſpect to, her civility, that kept her from complaining, or withdrawing, might have been dangerous, if not fatal to her, had not the princeſs, who was ſpeaking with her, and who knew her antipathy to roſes, taken notice, that her face grew ſtrangely pale, and was covered with a cold ſweat. For thereby preſently gueſſing what might be the cauſe, which the ſick lady herſelf did not, ſhe aſked aloud, whether ſome body had not brought roſes (which were then in ſeaſon) into the bed-chamber, which queſtion occaſioned a ſpeedy withdrawing of a lady, that ſtood at a diſtance off, and had about her roſes, which were not ſeen by the patient, who was by this means preſerved from falling into a ſwoon, though not from being for a while very much diſcompoſed.

But this you may tell me was the caſe of a woman, who complained her malady affected her heart, not her head. Wherefore returning to what I was ſpeaking of before I mentioned her, I ſhall proceed to tell you, that as odours may thus give men the head-ach, ſo I have often found the ſmell of rectified ſpirit of ſal armoniack to free men, as well as women, from the fits of that diſtemper; and that ſometimes in ſo few minutes, that the perſons relieved could ſcarcely imagine, they could ſo quickly be ſo.

To which I ſhall not add the trials, that I have ſucceſsfully made upon myſelf, becauſe being, thanks be to God, very ſeldom troubled with that diſtemper, the occaſions I have had of making them have not been many. And though I have not always found ſo ſlight a remedy to work the deſired cure, yet that it does it often, even in men, is ſufficient to ſhew the efficacy of ſanative effluviums.

Now to manifeſt, that ſteams do not operate only upon hyſterical women, or perſons ſubject to the head-ach, I will add ſome inſtances of the effects they may produce upon other perſons, and parts.

It is but too well known an obſervation, that women with child have been often made to miſcarry by the ſtink of an ill-extinguiſhed candle, though perhaps the ſmoke aſcending from the ſnuff were diſſipated into the inviſible corpuſcles, a good while before it arrived at the noſtrils of the unhappy woman; and what violent and ſtraining motions abortions are frequently accompanied with, is ſufficiently known already.

I think I have elſewhere mentioned, that a gentleman of my acquaintance, a proper and luſty man, will be put into the fits of vomiting by the ſmell of coffee, boiled in water. I ſhall therefore rather mention, that I know a phyſician, who having been, for a long time, when he was young, frequently compelled to take electuarium lenitivum, one of the gentleſt and leaſt unpleaſant laxatives of the ſhops, conceived ſuch a diſlike of it, that ſtill, as himſelf has complained to me, if he ſmell to it, as he ſometimes happens to do in apothecaries ſhops, it will work (now and then for ſeveral times) upwards and downwards with him.

I know another very ingenious perſon of the ſame faculty, that has been a traveller by ſea and land, who has complained to me, that the ſmell of the greaſe of the wheels of a hackney-coach, though it do but paſs by him, is wont to make him ſick, and ready to vomit.

Every body knows, that ſmoke is apt to make mens eyes water, and excite, in the organs of reſpiration, that troubleſome and vehement commotion we call coughing. But we need not have recourſe at all to viſible fumes, for the production of the like effects; ſince we have often obſerved them, and repeated ſneezings to boot, to proceed from the inviſible ſteams of ſpirit of ſal armoniack, when vials containing that liquor, though they were perhaps but very ſmall, were approached too haſtily, or, perhaps, too near to the noſtrils.

And becauſe in moſt of the foregoing inſtances, the chief effects ſeem to be wrought, by the conſent of parts, on the *genus nervoſum*, and the action of one of them upon the other, and thereby upon ſeveral other parts of the body, I will ſubjoin a remarkable inſtance of the operation of a mild and grateful odour upon the humours themſelves, and that in a man.

A famous apothecary, who is a very tall and big man, ſeveral times told me, that though he was once a great lover of roſes, yet having had occaſion to employ great quantities of them at a time, he was ſo altered by their ſteams, that now, if he come among the roſe-buſhes, the ſmell does much diſcompoſe him. And the odour of roſes, (I mean incarnate-roſes, which we commonly call damaſk-roſes, though they be not the true ones) makes ſuch a colliquation of humours in his head, that it ſets him a coughing, and makes him run at the noſe, and gives him a ſore throat; and by an affluence of humours makes his eyes ſore, inſomuch, that during the ſeaſon of roſes, when quantities of them are brought into his houſe, he is obliged, for the moſt part, to abſent himſelf from home.

CHAP. VII.

ONE may ſhew, on this occaſion, that as there might be conſiderable things performed by effluviums, as they make one part of a living engine work upon another by virtue of its ſtructure, ſo the action of ſuch inviſible agents may, in divers caſes, be much promoted by the fabrick and laws of the univerſe itſelf, upon this account, that, by the operation of effluvia upon particular bodies, they may diſpoſe and qualify thoſe bodies to be wrought upon, which before they were not fit to be, by light, magnetiſms, the atmoſphere, gravity, or ſome other of the more catholick agents of nature, as the world is now conſtituted. But not to injure another tract, I ſhall conclude this, when I ſhall have taken notice, that in the inſtances hitherto produced, there has been a viſible local diſtance between the body that emits ſtreams, and that on which they work. But if I thought it neceſſary, it were not difficult to ſhew, that one might well enough refer to the title of this tract divers effects of bodies, that are applied immediately to ours; ſuch as are blood-ſtones, cornelians, nephritick ſtones, lapis Malacenſis, and ſome amulets, and other ſolid ſubſtances, applied by phyſicians outwardly to our bodies. For in theſe applications the groſs body touches but the ſkin, and the great effects, which I elſewhere relate myſelf to have ſometimes (though not often, much leſs always) obſerved to have followed upon this external contact, or near application, may reaſonably be derived from the ſubtil emanations, that paſs thorough the pores of the ſkin to the inward parts of the body: as is evident in thoſe, who by holding cantharides in their hands, or having them applied to ſome remote external part, have grievous pains produced in their urinary parts, as it has happened to me, as well as to many others. And to the inſinuation of theſe minute corpuſcles, that get in at the pores of the ſkin, ſeems to be due the efficacy of ſome medicines, that purge, vomit, reſolve the humours, or otherwiſe notably alter the body, being but externally applied; of which I could here give ſeveral inſtances, but that they belong more properly to another place, and are not neceſſary in this, where it may ſuffice to name the notorious power, that mercurial ointments, or fumes, either together, or apart, have of producing copious ſalivations, to ſhew in general, that both the ſteams, and the emanations of outwardly-applied medicinal bodies, may have ſome great effects on human ones.

OF THE
DETERMINATE NATURE
OF
EFFLUVIUMS.

CHAP. I.

THE effluviums of bodies, *Pyrophilus*, being for the moſt part inviſible, have been wont to be ſo little conſidered by vulgar philoſophers, that ſcarce vouchſafing to take notice of their exiſtence, it is no wonder, that men have not been ſolicitous to diſcover their diſtinct natures and differences. Only *Ariſtotle*, and, upon his account, the ſchools, have been pleaſed to think, that the two grand parts of our globe do ſometimes emit two kinds of exhalations, or ſteams; the earthy part affording thoſe, that are hot and dry, which they name fumes, and very often, ſimply, exhalations; and the aqueous part, others, that are (not as many of his diſciples miſtake him to have taught, cold and moiſt, but) hot and moiſt [*], which they uſually call vapours, to diſcriminate them from the fumes, or exhalations, though otherwiſe, in common acceptation, thoſe appellations are very frequently confounded.

Lib. I. Meteor. cap. 3, & 4.

BUT, though the *Ariſtotelians* have thus perfunctorily handled this ſubject, it would not become corpuſcularian philoſophers, who attribute ſo much as they do to the inſenſible particles of matter, to acquieſce in ſo ſlight and jejune an account of the emanations of bodies. And ſince we have already ſhewn, that beſides the greater and more ſimple maſſes of terreſtrial and aqueous matter newly mentioned, there are very many mixed bodies, that emit effluviums, which make, as it were, little atmoſpheres about divers of them, it will be congruous to our doctrine and deſign, to add in this place, that beſides the ſlight and obvious differences, taken notice of by *Ariſtotle*, the ſteams of bodies may be almoſt as various, as the bodies themſelves, that

[*] Cap 3. Ἔστι γὰρ ἀτμίδος μὲν φύσις, ὑγρὸν καὶ θερμόν.

that emit them; and that therefore we ought not to look upon them barely under the general and confuſed notion of ſmoke, or vapours, but may probably conceive them to have their diſtinct and determinate natures, oftentimes, though not always, ſuitable to that of the bodies from whence they proceed.

And indeed, the newly-mentioned diviſions of the ſchools gives us ſo ſlight an account of the emanations of bodies, that, methinks, it looks like ſuch another, as if one ſhould divide animals into thoſe, that are horned, and thoſe, that have two feet: for, beſides that the diſtinction is taken from a difference, that is not the conſiderableſt, there are divers animals, as many four-footed beaſts, and fiſhes, that are not compriſed in it; and each member of the diviſion comprehends I know not how many diſtinct ſorts of animals, whoſe differences from one another are many times more conſiderable, than thoſe, that conſtitute the two ſupreme genus's, the one having bulls and goats, and rhinoceros's, and deer, and elks, and certain ſea-monſters, whoſe horns I have ſeen; and the other genus compriſing alſo a greater variety, namely, a great part of four-footed beaſts, and, beſides men, all the birds (for aught we know) whether of land or water. And as it would give us but a very ſlender information of the nature of an elk, or an unicorn, to know, that it is an horned beaſt; or of the nature of a man, an eagle, or a nightingale, to be told, that it is an hornleſs beaſt; ſo it will but very little inſtruct a man in the nature of the ſteams of quickſilver, or of opium, to be told, that they are vapours hot, or rather cold, and moiſt; or of the ſteams of amber, or cantharides, or cinnamon, or tobacco, to be told, that they are hot and dry. For, beſides that there may be effluviums, which, even by their elementary qualities, are not of either of theſe two ſupreme genus's, (for they may be cold and dry, or cold and moiſt,) theſe qualities are often far from being the nobleſt, and conſequently thoſe, that deſerve to be moſt conſidered in the effluviums of this, or that body; as we ſhall by and by have occaſion to manifeſt.

CHAP. II.

AND here it may not be improper to mention an experiment, that, I remember, I divers years ſince employed to illuſtrate the ſubject of our preſent diſcourſe.

I conſidered then, that fluid bodies may be of very unequal denſity, and gravity, as is evident in quickſilver, water, and pure ſpirit of wine; which, notwithſtanding their great difference in ſpecifick gravity, may yet agree in the conditions requiſite to fluid bodies. Therefore preſuming, that by what I could make appear viſible in one, what happens analogically in the other, may be ocularly illuſtrated, I took ſome ounces of roch-allom, and as much of fine ſalt-petre. I took ſome ounces of each, becauſe, if the quantity of the ingredients be too ſmall, the concoagulated grains will be ſo too, and the ſucceſs will not be ſo conſpicuous. Theſe being diſſolved together in fair water, the filtrated ſolution was ſet to evaporate in an open-mouthed glaſs, and being then left to ſhoot in a cool place, there were faſtned to the ſides, and other parts of the glaſs, ſeveral ſmall cryſtals, ſome octoedrical, which is the figure proper to roch allom, and others of the priſmatical ſhape of pure ſalt-petre; beſides ſome other ſaline concretions, whoſe being diſtinctly of neither of theſe two ſhapes, argued them to be concoagulations of both the ſalts. And this we did, by uſing ſuch a degree of celerity in evaporating the liquor, as was proper for ſuch an effect. For, by another degree, which is to be employed, when one would recover the ſalts more diſtinctly and manifeſtly, the matter may, as I found by trial, be ſo ordered, that the aluminous ſalt may, for the moſt part, be firſt coagulated by itſelf, and then, from the remaining liquor, curiouſly ſhaped cryſtals of nitre may be copiouſly obtained.

Trials like this we alſo made with other ſalts, and particularly with ſea-ſalt, and with allom and vitriol; the phænomena of which you may meet with in their due places. For the recited experiment may, I hope, alone ſerve to aſſiſt the imagination to conceive, how the particles of bodies may ſwim to and fro in a fluid, (which the air is,) and though they be little enough to be inviſible, may, many of them, retain their diſtinct and determinate natures, and their aptneſs to cohere upon occaſion; and others may, by their various occurſions and coalitions, unite into leſſer corpuſcles, or greater bodies, differing from the more ſimple particles, that compoſed them, and yet not of indeterminate, though compounded figures.

CHAP. III.

THESE things being premiſ'd, we may now proceed to the particular inſtances of the determinate nature of effluviums; and theſe we may not inconveniently reduce to the three following heads, to each of which we ſhall aſſign a diſtinct chapter; the firſt of theſe I ſhall briefly treat of in this third chapter, and treat ſomewhat more largely of the others in the two following.

In the firſt place then, that the effluviums of many bodies retain a determinate nature oftentimes in an inviſible ſmallneſs, and oftener in ſuch a ſize, as makes them little enough to fly or ſwim in the air; may appear by this, that theſe effluvia being, by condenſation, or otherwiſe, re-united, they appear to be of the ſame nature with the body, that emitted them. Thus in moiſt weather, the vapours of water, that wander inviſibly through the air, meeting with marble-walls, or pavements, or other bodies, by their coldneſs and other qualifications, fit to condenſe and retain them, appear again in the form of drops of water; and the ſame vapours return to the viſible form of water, when they fall out of the air in dews, or rains.

Quick-ſilver it ſelf, if it be made to aſcend in diſtillation with a convenient degree of fire, will almoſt all be found again in the receiver in the form of running mercury. Which ſtrange and piercing fluid is in ſome caſes ſo diſpoſed to be ſtripped of its diſguiſes, and re-appear in its own form, that divers artificers, and eſpecially gilders, have found, to their coſt, that the fumes of it need not be, as in diſtillation, included in cloſe veſſels to return to their priſtine nature, mercury having been ſeveral times found in the heads, and other parts of ſuch people, who have, in tract of time, been killed by it, and ſometimes made to diſcover itſelf during the lives of thoſe, that dealed ſo much in it; of which I elſewhere give ſome inſtances. Wherefore I ſhall only obſerve at preſent, that it is a common practice, both among gilders, and ſome chemiſts, that, when they have occaſion to make an amalgam, or force away the mercury from one by the fire, they keep gold in their mouths, which by the mercurial fumes, that wander through the air, will now and then, by that time it is taken out of their mouths, be turned white almoſt, as if it had been ſilvered over.

A maſs of purified brimſtone being ſublimed, the aſcending fumes will condenſe into what the chemiſts call *flores Sulphuris*, which is true ſulphur of the ſame nature with that, formerly expoſed to ſublimation; and may readily, by melting, be reduced into ſuch another maſs.

AND to give you another like example of dry bodies; I tried, that by ſubliming good camphire in cloſe veſſels, it would all, as to ſenſe, be raiſed into the upper veſſel, or part of the ſubliming-glaſs, in the form of dry camphire, as it was before.

NAY, though a body be not by nature, but art, compounded of ſuch differing bodies, as a metal and another mineral, and two or three ſalts; yet, if upon purification of the mixture from its groſſer parts, the remaining and finer parts be minute enough and fitly ſhaped, the whole liquor will aſcend, and yet in the receiver altogether recover its priſtine form of a tranſparent fluid, compoſed of differing ſaline and mineral parts. This is evident in the diſtillation of what chemiſts call butter, or oil of antimony, very well rectified. For this liquor will paſs into the receiver diaphanous and fluid, though, beſides the particles of the ſublimate, (which is it ſelf a factitious compounded body) it abounds with antimonial corpuſcles, carried over and kept inviſible by the corroding ſalts; whatever *Angelus Sala*, and thoſe chemiſts, that follow him, have affirmed to the contrary, as might be eaſily here proved, if this were a fit place to do it in.

I found by enquiring of an ingenious perſon, that had an intereſt in a tin-mine, that I was not deceived in gueſſing, that tin it ſelf, though a metal, whoſe ore is of a very difficult fuſion, and which I have by it ſelf kept long upon the cupel without finding it to fly away, would yet retain its metalline nature in the form of fumes or flowers. For this experienced gentleman anſwered me, that divers times they would take great ſtore of a whitiſh ſublimate from the upper part of the furnaces or chimnies, where they brought their ore to fuſion, or wrought further upon it; and that this ſublimate, though perhaps elevated to the height of an ordinary man, would, when melted down, afford at once many pounds of very good tin. On which occaſion I ſhall add, that I have my ſelf, more than once, raiſed this metal in the form of white corpuſcles by the help of an additament, that did ſcarce weigh half ſo much as it.

CHAP. IV.

THE ſecond way, by which we may diſcover the determine nature of effluviums, is, by the difference, that may ſometimes be obſerved in their ſenſible qualities. For theſe effluviums, that are endowed with them, proceed from the ſame ſort of bodies, and yet thoſe afforded by one kind of bodies being in many caſes manifeſtly differing from thoſe, that fly off from another, this evident diſparity in their exhalations argues their retaining diſtinct natures, according to thoſe of the reſpective bodies whence they proceed.

I will not now ſtay to examine, whether in the ſteams, that are made viſibly to aſcend from the terreſtrial globe by thoſe grand agents and uſual raiſers of them, the ſun, and the agitation of the air, the eye can manifeſtly diſtinguiſh the diverſity of colours: but in ſome productions of art, ſuch different colours may be diſcovered in the exhalations, even without the application of any external heat to raiſe them. For, when ſpirit of nitre, for example, has been well rectified, I have often obſerved, that even in the cold the fumes would play in the unfilled part of the ſtopped vials it was kept in, and appear in it of a reddiſh colour, and if thoſe veſſels were opened, the ſame fumes would copiouſly aſcend into the air, in the form of a reddiſh or orange-tawny ſmoke. ſpirit, or oil of ſalt alſo, if it be very well dephlegmed, though it will ſcarce in the cold viſibly aſcend in the empty part of a vial, whilſt it is kept well ſtopped; yet, if the free air be allowed acceſs to it, it will, in caſe it be ſufficiently rectified, fly up in the form of a whitiſh fume. But this is inconſiderable, in compariſon of what happens in a volatile tincture of ſulphur I have elſewhere taught you to make with quick-lime. For, not only upon a ſlight occaſion, the vacant part of the vial will be filled with white fumes, though the glaſs be well ſtopped; but upon the opening the vial theſe fumes will copiouſly paſs out at the neck, and aſcend into the air in the form of a ſmoke, more white, than perhaps you ever ſaw any. And both this and that of the ſpirit of ſaltpetre do, by their operation, as well as ſmell, diſcloſe what they are; the latter being of a nitrous nature, (as is confeſſed) and the former, of a ſulphureous: in ſo much, that having, for curioſity's ſake, in a fitly ſhaped glaſs, caught a competent quantity of the aſcending white fumes, I found them to have convened into bodies tranſparent and geometrically figured, wherein it was eaſy to diſcover, by their ſenſible qualities, that there were ſtore of ſulphureous particles mixed with the ſaline ones. That the liquors of vegetables, diſtilled *in balneo*, or in water, are not wont to retain any thing of the colour of the bodies, that afforded them, is a thing eaſy to be obſerved in diſtillations made without retorts or the violence of the fire. But it may be worth while to make trial, whether the eſſential oil of wormwood aſcend coloured like the plant whence it is firſt drawn over with water in the alembic, or rectified *in balneo*. For I forgot to take notice of it, when upon ſome particularities, I obſerved in that plant, my curioſity led me to find, that not only in the firſt diſtillation in a copper alembic, tinned on the inſide, the oil came over green, but by a rectification purpoſely made in a glaſs veſſel, the purified liquor was not deprived of that colour.

The mention of theſe eſſential oils, as chemiſts call thoſe, that are drawn in alembics, leads me to tell you, that, though theſe liquors be but effluvia of the vegetables they are diſtilled from, condenſed again in the receiver into liquors; yet, as ſubtile as they are, many of them retain the genuine taſte of the bodies, whence the heat elevated them; as you will eaſily find, if you will taſte a few drops of the eſſential oil of cinnamon, for example, or of wormwood diſſolved by the intervention of ſugar, or ſpirit of wine, in a convenient quantity of water, wine, or beer. For, by this means you have the natural taſte of this ſpice or herb. And wormwood is a plant, whoſe effluvia do ſo retain the nature of the body, that parts with them, that I muſt not forbear to alledge here an obſervation of mine, that may ſhew you, that it is poſſible, though not uſual, that even without the help of the fire, the expirations of a body may communicate its taſte. For, among other things, that I had occaſion to obſerve about ſome quantity of wormwood laid up together, I remember, I took notice, and made others do the like, that coming into a room, where it was kept, not only the organs of ſmelling were powerfully wrought upon by the corpuſcles, that ſwarmed in the air, but alſo the mouth was ſenſibly affected with a bitter taſte. Perhaps you will ſcarce think it worth while, that after this inſtance I ſhould add, that I found the expirations of amber, kept a while in pure ſpirit of wine, taſte upon the tongue like amber it ſelf, when I chewed it between my teeth. But I chooſe to mention this inſtance, becauſe it will connect thoſe lately mentioned with another ſort, very pertinent to our preſent purpoſe. For, the expirations, that I have obtained from amber, both with pure ſpirit of wine, and a more peircing menſtruum, did manifeſtly retain in both thoſe liquors a peculiar ſmell, with which I found it to affect the noſtrils, when, for trial's ſake, I excited the electrical faculty of amber by rubbing. And as for odours, it is plain, that the eſſential oils of chemiſts, well drawn, do many of them retain the peculiar and genuine ſcent of the ſpices or herbs, that afforded them. And that theſe odours do really conſiſt of, or reſide in certain inviſible corpuſcles, that fly off from the viſible bodies, that are ſaid to be endowed with ſuch ſmells, I have elſewhere proved at large; and it may ſufficiently appear from their ſticking to divers of the bodies they meet with and their laſting adheſion to them.

Other examples may be given of the ſettled difference of effluviums directly perceivable by human organs of ſenſe, as dull as they are; which laſt expreſſion I add, becauſe I ſcarce doubt, but that, if our ſenſories were ſufficiently ſubtile and tender, they might immediately perceive in the ſize, ſhape, motion, and perhaps colour too of ſome now inviſible effluviums, as diſtinguiſhable differences, as our naked eyes in their preſent conſtitution ſee, between the differing ſorts of birds, by their appearances, and their manner of flying in the air, as hawks, and partridges, and ſparrows, and ſwallows. To make this probable I will not urge, that in fine white ſand, whoſe grains by the unaſſiſted eye are not wont to be diſtinguiſhed by any ſenſible quality, I have often obſerved in an excellent miſcroſcope, a notable diſparity as to bulk, figure, and ſometimes as to colour: and that in ſmall cheeſe-mites, which the naked eye can very ſcarcely diſcern, ſo far is it from diſcovering any difference between them, one may (as was noted in the laſt eſſay) plainly ſee, beſides an obvious difference in point of bigneſs, many particular parts, on whoſe accounts, the ſtructure of thoſe moving points may difference them from each other. And I have ſometimes ſeen a very evident diſparity, even in point of ſhape, between the very eggs of theſe living atoms, as a poet would perhaps ſtile them. But theſe kinds of proofs, (as I was ſaying) I ſhall forbear to inſiſt on, that I may proceed to countenance my conjecture by the effects of the effluviums, that are properly ſo called, upon animals.

And firſt, though the touch be reckoned one of the moſt dull of the five ſenſes, and be reputed to be far leſs quick in men, than in divers other animals; yet the groſs organs of that may, in men themſelves, even by accident, be ſo diſpoſed, as to be ſuſceptible of impreſſions from effluvia: of this, in another paper, I give ſome inſtances. And I know not, whether divers of the preſages of weather, to be obſerved in ſome animals, and the aches, and other pains, that in many crazy and wounded men, are wont to fore-run great changes of weather, do not often (for I do not ſay always) proceed, at leaſt in part, from inviſible, and yet incongruous effluxions, which, either from the ſubterraneal parts, or from ſome bodies above ground, do copiouſly impregnate the air. And, on this occaſion, it will not be impertinent to mention here what an experienced phyſician, being (if I much miſremember not) the learned *Diemerbroek*, relates concerning himſelf, who having been infected with the plague by a patient, that lay very ill of it, though by God's bleſſing, which he particularly acknowledges, upon a ſlight, but ſeaſonable remedy, he was very quickly cured, and that without the breaking of any tumor; yet it left ſuch a change in ſome parts of his body, that he ſubjoins this memorable paſſage, *Ab illo periculo ad contagioſos mihi appropinquanti in emunctoriis ſucceſſit dolor, vix fallax peſtis indicium.*

About Coſmical Suſpicions

Two or three other obſervations of the like nature you meet with in another of my papers. And I ſhall now add, that I know an ingenious gentlewoman (wife to a famous phyſician) who was of a very curious and delicate complexion, that has ſeveral times aſſured me, that ſhe can very readily diſcover, whether a perſon, that comes to viſit her in winter, came from ſome place, where there is any conſiderable quantity of ſnow; and this ſhe does, as ſhe tells me, not by feeling any unuſual cold (for if the ground be frozen, but not covered with ſnow, the effect ſucceeds not,) but from ſome peculiar impreſſion, which ſhe thinks, ſhe receives by the organs of ſmelling. I might add, that I know alſo, as I may have formerly told you, a very ingenious phyſician, who falling into an odd kind of fever, had his ſenſe of hearing thereby made ſo very nice and tender, that he very plainly heard ſoft whiſpers, that were made at a conſiderable diſtance off, and which were not in the leaſt perceived by the healthy by-ſtanders, nor would have been by him before his ſickneſs. Which ſickneſs I mention as the thing, that gave his organs of hearing this preter-natural quickneſs, becauſe, when the fever had quite left him, he was able to hear but at the rate of other men. And I might tell you too, that I know a gentleman of eminent parts and note, who, during a diſtemper he had in his eyes, had his organs of ſight brought to be ſo tender, that both his friends, and himſelf alſo, have aſſured me, that when he waked in the night, he could for a while plainly ſee and diſtinguiſh colours, as well as other objects, diſcernible by the eye, as was more than once tried, by pinning ribbands, or the like bodies, of ſeveral colours, to the inſide of his curtains in the dark. For if he were awakened in the night, he would be able to tell his bed-fellow, where thoſe bodies were placed, and what colour each of them was of.

I have mentioned theſe inſtances only to ſhew you, that if our ſenſories were more delicate and quick, they would be ſufficiently affected by objects, that, as they are generally conſtituted, make no impreſſions at all upon them. For otherwiſe I know, that the ſpecies (as they call them) both of ſounds and colours are not held by many of the moderns, (from whom in that I diſſent not,) to be ſo much corporeal effluxions, trajected through the medium, as peculiar kinds of local motion conveyed by it. Therefore, I ſhall now confirm the conjecture I would countenance, by the diſcrimination made by the organs of other animals, of ſuch effluvia, as to us men are not only inviſible, but inſenſible. And therefore, partly to ſtrengthen what I delivered, and partly to confirm what I am now diſcourſing of, it will not be impertinent to ſubjoin two or three relations, that I had from perſons of very good credit, whom I thought likely to make me no unſatisfactory returns to my queſtions, about things they were very well verſed in.

A perſon of quality, to whom I am near allied, related to me, that to make a trial, whether a young blood-hound was well inſtructed, (or as the huntſmen call it, made) he cauſed one of his ſervants, who had not killed, or ſo much as touched any of his deer, to walk to a country-town, four mile off, and then to a market-town, three miles diſtant from thence; which done, this nobleman did, a competent while after, put the blood-hound upon the ſcent of the man, and cauſed him to be followed by a ſervant or two, the maſter himſelf thinking it alſo fit to go after them to ſee the event; which was, that the dog, without ever ſeeing the man he was to purſue, followed him by the ſcent to the above-mentioned places, notwithſtanding the multitude of market people, that went along in the ſame way, and of travellers, that had occaſion to croſs it. And when the blood-hound came to the chief market-town, he paſſed through the ſtreets, without taking notice of any of the people there, and left not, till he had gone to the houſe, where the man, he ſought, reſted himſelf, and found him in an upper room, to the wonder of thoſe that followed him. The particulars of this narrative, the nobleman's wife, a perſon of great veracity, that happened to be with him when the trial was made, confirmed to me.

ENQUIRING of a ſtudious perſon, that was keeper of a red-deer park, and verſed in making blood-hounds, in how long time, after a man or deer had paſſed by a graſſy place, one of thoſe dogs would be able to follow him by the ſcent? he told me, that it would be ſix or ſeven hours: whereupon an ingenious gentleman, that chanced to be preſent, and lived near that park, aſſured us both, that he had old dogs of ſo good a ſcent, that if a buck had the day before paſſed in a wood, they will, when they come where the ſcent lies, though at ſuch a diſtance of time after, preſently find the ſcent and run directly to that part of the wood where the buck is. He alſo told me, that though an old blood-hound will not ſo eaſily fix on the ſcent of a ſingle deer, that preſently hides himſelf in a whole herd, yet if the deer be chaſed a little till he be heated, the dog will go nigh to ſingle him out, though the whole herd alſo be chaſed. The above-named gentleman alſo affirmed, that he could eaſily diſtinguiſh, whether his hounds were in chaſe of a hare, or a fox, by their way of running, and their holding up their noſe higher than ordinary, when they purſue a fox, whoſe ſcent is more ſtrong. Theſe relations will not be judged incredible by him, that reflects on ſome of the inſtances, that have already, in the foregoing eſſay, been given of the ſtrange ſubtilty of effluvia: to which I ſhall now add, that I remember, that to try, whether I could, in ſome meaſure, make art imitate nature, I prepared a body of a vegetable ſubſtance, which, though it were actually cold, and both to the eye and touch dry, did for a while emit ſuch determinate and piercing, though inviſible, exhalations, that having, for trial's ſake, applied to it a clear metalline plate, and that of none of the very ſofteſt kind neither, for about one minute of an hour, I found, that, though there had been no immediate contact between them, I having purpoſely interpoſed a piece of paper to hinder it, yet there was imprinted on the ſurface of the plate a conſpicuous ſtain of that peculiar colour, that the body, with whoſe ſteams I had imbued the vegetable ſubſtance, was fitted to give a plate of that mixed metal. And though it be true, that in ſome circumſtances, the lately-mentioned inſtances about blood-hounds have a conſiderable advantage of this I have now recited, yet that advantage is much leſſened, not to ſay countervailed, by ſome circumſtances of our experiment. For, not to repeat, that the emittent body was firm and cold, the effect produced by the effluvium, that guided the ſetting-dog, was wrought upon the ſenſory of a living and warm animal; and ſuch an one, whoſe organs of ſmelling are of an extraordinary tender conſtitution above thoſe of men, and other animals, and probably the impreſſion was but tranſient; whereas, in our caſe, the inviſible ſteams of the vegetable ſubſtance wrought upon a body, which was of ſo ſtrong and inorganical a texture, as a compounded metal, though it were fenced by being lapped up in paper, notwithſtanding which theſe ſteams invaded it in ſuch numbers, and ſo notably, as to make their operation on it manifeſt to the eye, and conſiderably permanent too; ſince, coming to look upon the plate after the third day, I found the induced colour yet conſpicuous, and not like ſuddenly to vaniſh.

HITHERTO, in this chapter, I have argued from the conſtant and ſettled difference of the ſenſible qualities of effluviums, that they do not always loſe their diſtinct natures, when they ſeem to have loſt themſelves by vaniſhing into air. But before I diſmiſs this ſubject, I muſt conſider an objection, which I know may be made againſt the opinion we have been countenancing. For it may be alledged, that there may be many caſes, wherein the effluviums of bodies are, in their paſſage through the air, ſenſibly altered, or do affect the organs of ſenſe, otherwiſe than each kind of them apart would do: nor is this difficulty altogether irrational. For it ſeems conſonant enough to experience, that ſome ſuch caſes ſhould be admitted; and therefore, in the foregoing diſcourſe I have, where I thought it neceſſary, forborn to expreſs myſelf in ſuch general and abſolute terms, as otherwiſe I might have done. But as for ſuch caſes, as I have inſiſted upon, and many more I ſhall now repreſent, that the objected alterations need not hinder, but that effluviums, at their firſt parting from the bodies, whence they take wing, if I may ſo ſpeak, may retain as much of the nature of thoſe bodies, as we have aſcribed to them; ſince the ſubſequent change may very probably be deduced from the combinations, or coalitions, of divers ſteams aſſociating themſelves in the air, and acting upon the ſenſory, either altogether and conjointly, or at leaſt ſo near it, that the ſenſe cannot perceive their operations as diſtinct. This I ſhall elucidate, but not pretend to prove, by what happens in ſounds and taſtes. For if, by way of inſtance, in a muſical inſtrument, two ſtrings tuned to an eighth, be touched together, they will ſtrike the ear with a ſound, that will be judged one, as well as pleaſing, though each of the trembling ſtrings make a diſtinct noiſe, and the one vibrates as faſt again as the other. And if, into oil of tartars *per deliquium*, you drop a due proportion of ſpirit of nitre, and exhale the ſuperfluous moiſture, the acid and alcalizate corpuſcles, that were ſo ſmall, as to ſwim inviſibly in thoſe liquors, will convene into nitrous concretions, whoſe taſte will be compounded of, but very differing from, both the taſtes of the acid and tartareous particles; which particles may yet, for the moſt part, by a ſkilful diſtillation, be divorced again. And ſo, if to a ſtrong ſolution of pot-aſhes, or ſalt of tartar, you put as much in weight of ſal armoniack, as there is of either of thoſe fixed ſalts contained in the liquor; you may, beſides a ſubtil urinous ſpirit, that will eaſily come over in the diſtillation, obtain a dry *caput mortuum*, which is almoſt totally a compounded ſalt, differing enough from either of the ingredients, eſpecially the alcalizate, as well in taſte, as in ſome other qualities: This ſalt, freed from its fæces, being that diuretick ſalt, I ſeveral years ago gave quantities of to ſome chemiſts and phyſicians, from the moſt of whom I received great thanks, accompanied with the more acceptable accounts of the very happy ſucceſs they had employed it with, though uſually but in a ſmall doſe, as from ſix, eight, or ten grains, to a ſcruple. But this being mentioned only upon the by, I ſhall proceed to tell you, that ſince I intimated to you already, that I would mention examples of ſounds and taſtes, only to illuſtrate what I have been delivering; I ſhall now add ſome inſtances by way of proof, of the coalition and reſulting change of ſteams in the air. It is eaſily obſervable in ſome noſegays, where the differing flowers happen to be conveniently mixed, that in the ſmell afforded by it, at a due diſtance, the odours of the particular flowers are not perceived, but the organ is affected by their joint action, which makes on it a confuſed, but delightful impreſſion. And ſo, when in a ball of pomander, or a perfumed ſkin, muſk, and amber, and civet, and other ſweets, are ſkilfully mixed, the coalition of the diſtinct effluvia of the ingredients, that aſſociate themſelves in their paſſage through the air, produce in the ſenſory one grateful perfume, reſulting from all thoſe odours. But if you take ſpirit of fermented urine, and ſpirit of wine, both of them phlegmatick, and mix them together, they will incorporate like wine and water, or any other ſuch liquors, without affording any dry concretions. But if you expoſe them in a convenient veſſel but to the mild heat of a bath, or lamp, the aſcending particles will aſſociate themſelves, and adhere to the upper part of the glaſs in the form of a white, but tender ſublimate, conſiſting both of urinous and vinous ſpirits, aſſociated into a mixture, which differs from either of the liquors, not only in conſiſtence, taſte, and ſmell, but in ſome conſiderable operations performable by this odd mixture; which this is not the place to take further notice of. And if ſpirit of ſalt, and ſpirit of nitre be, by diſtillation, elevated in the form of fumes, ſo ordered, as to convene into one liquor in the receiver, this liquor will readily diſſolve crude gold, though neither the ſpirit of nitre alone, nor that of ſalt, would do ſo.

AND that you may have an ocular proof of the poſſibility of the diſtinctneſs and ſubſequent commixture of ſteams in the air, I ſhall now add an experiment, which I long ſince deviſed for that purpoſe, and which I ſoon after ſhewed to many curious perſons, moſt of whom appeared ſomewhat ſurprized at it. The experiment was; that I took two ſmall vials, the one filled with ſpirit of ſalt, but not very ſtrong, the other with ſpirit of fermented urine, or of ſal armoniack very well rectified; theſe vials being placed at ſome diſtance, and not being ſtopped, each liquor afforded its own ſmell, at a pretty diſtance, by the ſteams it emitted into the air, but yet theſe ſteams were inviſible. But when theſe vials, (which ſhould be of the ſame ſize) came to be approached very near to each other, though not ſo, as to touch; as when the two liquors are put together in the form of liquors, they will notably act upon one another; ſo their reſpective effluviums meeting in the air, would, anſwerably to the littleneſs of their bulk, do the like, and, by their mutual occurſions, become manifeſtly viſible, and appear moving in the air like a little portion of ſmoke, or of a miſt, which would quickly ceaſe, if either of the vials were removed half a foot, or a foot from the other. And I remember, that, to add to the oddneſs of the phænomenon, I ſometimes made a drop of the ſpirit of ſalt hang at the bottom of a little ſtick of glaſs, or ſome other convenient body, and held this drop thus ſuſpended in the orifice of a vial, that had ſpirit of ſal armoniack in it, and was furniſhed with a ſomewhat long neck; for by this means, it happened as I expected, that the aſcending urinous particles, though inviſible before, invading plentifully the acid ones of the drop, produced a notable ſmoke, which, if the drop were held a little above the neck of the glaſs, would moſt commonly fly upwards to the height of a foot, or half a yard: but if the drop were held ſomewhat deep within the cavity of the neck, a good part of the produced ſmoke would oftentimes fall into the cavity of the vial, which was left in great part empty, ſometimes in the form of drops, but uſually in the form of a ſlender and ſomewhat winding ſtream, of a white colour, that ſeemed to flow down juſt like a liquor from the depending drop, till it had reached the ſpirit of ſal armoniack; upon whoſe ſurface it would ſpread itſelf like a miſt. But this only upon the by. As for the main experiment itſelf, it may be, as I have found, ſucceſsfully tried with other liquors than theſe; but it is not neceſſary, in this place, to give an account of ſuch trials; though perhaps, if I had leiſure, it might be worth while to conſider, whether theſe coalitions of differing ſorts of ſteams in the air, and the changes reſulting thence of their particular precedent quantities, may not aſſiſt us to inveſtigate the cauſes of divers ſudden clouds and miſts, and ſome other meteorological phænomena, and alſo of divers changes, that happen in the air, in reference to the coming in and ceaſing of ſeveral either epidemical, or contagious diſeaſes, and particularly the plague, that ſeem to depend upon ſome occult temperature and alterations of the air, which may be copiouſly impregnated by the differing ſubterraneal (not to add here, ſideral) effluviums, that not unfrequently aſcend into it, or otherwiſe invade it, with peſtiferous, or other morbifick corpuſcles, and ſometimes with others of a contrary nature, and ſometimes too, perhaps, neither the one ſort of ſteams, which may be ſuppoſed to have imbued the air, is in itſelf deleterious; nor the other ſalutary, but becomes ſo upon their caſual coalition in the air. You will perhaps think this conjecture of the reſultancy of peſtilential ſteams the leſs improbable, if I here add that odd obſervation, which was frequently made in the formerly-mentioned plague at *Nimmeguen*, by a phyſician ſo judicious as *Diemerbroek*, whoſe words are theſe; *Illud notatu dignum ſæpiſſimè obſervavimus, nempe in illis ædibus, in quibus nulla adhuc peſtis erat, ſi linteamina ſordida aquâ & ſapone noſtrate (ut in Belgio moris eſt) illic lavarentur, eo ipſo die, vel interdum poſtridie, duos tres-ve ſimul peſte correptos fuiſſe, ipſique ægri teſtabantur fœtorem aquæ ſaponatæ illis primam & maximam alterationem intuliſſe. Hoc ipſum quoque in meo ipſius hoſpitio infelix experientia docuit, in quo poſt lota linteamina ſtatim gravem alterationem perceperunt plerique domeſtici, & proximè ſequenti nocte tres peſte correptæ, ac brevi poſt mortuæ fuere.* I omit the inſtances he further ſets down to confirm this odd phænomenon, of which, though perhaps ſome other cauſe may be divided, yet, that I lately aſſigned, ſeems at leaſt a probable one, if not the moſt probable; ſince, as it is manifeſt by daily experience, that the ſmell occaſioned by the waſhing of foul linen with the ſoap commonly uſed in the *Netherlands*, produces not the plague; ſo, by our learned author's obſervation it appears, either, that there were not yet any peſtilential effluxions in the air of thoſe places, which, on the occaſions of thoſe waſhings, became infected, or at leaſt, that by the addition of the fetid effluvia of the ſoapy water, thoſe morbifick particles, that were diſperſed through the air before, had not the power to introduce a malignant conſtitution into the air, and to act as truly peſtilential, till they were enabled to do ſo, by being aſſociated with the ill-ſcented effluvia of the ſoap.

Tract. de Peſte, Lib. II. cap. 3.

WHETHER alſo ſalutary, and, if I may ſo call them, alexipharmical corpuſcles may not be produced in the air by coalition, might be very well worth our enquiry: eſpecially if we had a competent hiſtorical account of the yearly ceaſing of the plague at *Grand Cayro*. For, as I have elſewhere noted out of the learned *Proſper Alpinus*, who practiſed phyſick there; and as I have alſo been informed by ſome of my acquaintance, who viſited that vaſt city, that almoſt in the midſt of ſummer, as ſoon as the river begins to riſe[*], the plague has its malignity ſuddenly checked, even as to thoſe, that are already infected, and ſoon after ceaſes; ſo if other circumſtances contradict not, one might gueſs, that this ſtrange phænomenon may be chiefly occaſioned by ſome nitrous, or other corpuſcles, that accompany the overflowing nile, and by aſſociating themſelves with what *Hippocrates* ſomewhere calls νοτερὰς ἀποῤῥοίας, diſable them to produce their wonted pernicious effects. To which hypotheſis ſuits well what is delivered by more than one traveller into *Egypt*, and more particular by our ingenious countryman Mr. *George Sandys*, who not only takes notice, that about the time of the overflowing of *Nilus*, whoſe abounding with nitre has been obſerved even by the antients, there is a certain moiſtening emanation diffuſed thorough the air. To prove, ſays he[†], ſpeaking of the overflowing of *Nilus*, that it proceedeth from a natural cauſe, this one, though ſtrange, yet true experiment will ſuffice. Take of the earth of *Egypt* adjoining to the river, and preſerve it carefully, that it neither come to be wet nor waſted, weigh it daily, and you ſhall find it neither more nor leſs heavy until the ſeventeenth of *June*, at which day it beginneth to grow more ponderous, and augmenteth with the augmentation of the river, whereby they have an infallible knowledge of the ſtate of the deluge, proceeding without doubt from the humidity of the air, which having a recourſe through all paſſable places, and mixing therewith, increaſeth the ſame, as it increaſeth in moiſture.

THAT theſe ſanative ſteams perform their effects merely becauſe they are moiſt, I preſume naturaliſts will ſcarce pretend; but that they may be of ſuch a nature, as by their coalition with the morbifick corpuſcles, to increaſe their bulk and alter their figure, or precipitate them out of the air, or clog their agility, or pervert their motions, and, in a word, deſtroy all, or ſome, at leaſt, of thoſe mechanical affections, which made thoſe corpuſcles peſtilential: that, I ſay, theſe antidotal vapours (if I may ſo call them) may have theſe effects upon thoſe, that formerly were morbifick, and that ſo there may reſult from the aſſociation of two ſorts of particles, whereof one was of a highly noxious nature, a harmleſs mixture, might here be made probable by ſeveral things; but that I hope, what I have lately recited about the coalitions of the effluvia of ſpirit of ſalt, and of urine, (liquors known to be highly contrary to each other) is not already forgotten by you.

AND the experiment, with which I am to conclude this eſſay, will, perhaps, make you think it poſſible, that the peſtiferous ſteams, that have already paſſed out of the air, and invaded, but not too much vitiated, the bodies of men, may have their malignity much debilitated by the ſupervening of theſe antidotal particles. For in that experiment you will find, that the ſteams emitted into the air from the liquor there deſcribed, though that were actually cold, were able to reach, and manifeſtly to operate, (and that probably by way of precipitation,) upon corpuſcles, that were fenced from them by the interpoſition of other bodies, not more porous than thoſe of living men. Whether the fume of ſulphur, which by many is extolled to prevent the infection of the air, do, by its acid, or other particles, diſarm, if I may ſo ſpeak, the peſtilential ones, I have not now time to enquire: no more than whether in *Ireland*, and ſome few other countries, that breed or brook no poiſonous animals, that hoſtility may proceed, at leaſt, in great part, from the peculiar nature of the ſoil, which both from its ſuperficial and deeper parts, conſtantly ſupplies the air with corpuſcles deſtructive to venemous animals. And ſome other particulars, that may be pertinently enough conſidered here, you may find treated on in other papers. And therefore at preſent I ſhall only intimate, in a word, that having purpoſely made a viſible and laſting ſtain on a ſolid body barely by cold effluvia, I did, by the inviſible and cold ſteams of another body, make, in two or three minutes, a viſible change in the colour of that ſtain.

AND as for the other part of the conjecture, viz. that meteors may ſometimes be produced by the occurſions of ſubterraneal effluvia, ſome of them of one determinate nature, and ſome of another, I think I could, to countenance it, give you divers inſtances of the plentiful impregnation of the air at ſome times, and in ſome places, with ſteams of very differing natures, and ſuch as are not ſo likely to be attracted by the heat of the ſun, as to be ſent up from the ſubterraneal regions, and ſometimes from minerals themſelves. But for inſtances of this kind, I ſhall, for brevity ſake, refer you to another paper[‖], where I have purpoſely treated of this ſubject, and particularly ſhewn, that though uſually the effluxions, that come from under-ground, are ill-ſcented, yet they are not always ſo; and alſo, that ſulphureous exhalations, even from cold, and, for the moſt part, aqueous liquors, may retain their determinate nature in the air, and act accordingly upon ſolid bodies themſelves, to whoſe conſtitution thoſe effluvia chance to be proportionate.

BUT one memorable ſtory, not mentioned in that diſcourſe, is too much to our preſent purpoſe to be here omitted, eſpecially having met with it in ſo approved an author as the experienced *Agricola*, who having mentioned out of antient hiſtorians the raining of white and red liquors, which they took (erroneouſly I doubt not) for milk and blood, ſubjoins, [§] *Ut autem majorem fidem habeamus annalium monumentis facit*

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[*] The plague, which here miſerably rageth, upon the firſt of the flood doth inſtantly ceaſe; in ſo much, as when five hundred die at *Gayro* the day before, which is nothing rare (for the ſound keep company with the ſick, holding death fatal, and, to avoid them, irreligion,) not one doth die the day following; ſays Mr. *Sandys* in his travels, Lib. II.

[†] Mr. *Sandys* in the book above-cited.

[‖] An Eſſay of Subterraneal Exhalations.

[§] *Agric.* de nat. eorum, quæ effluunt è terra, Lib. XII. pag. 236.

facit res illa decantata, quæ patrum memoriâ (in another place he ſpecifies the year of our Lord) *in* Suevia *accidit; aer enim ille ſtillavit guttas, quæ lineas veſtes crucibus rubris quaſi ſanguineis imbuebant.* Which I the rather mention, becauſe it does not only prove what I alledge it for; but may keep what is lately and very credibly reported to have happened in divers places of the kingdom of *Naples*, ſoon after the fiery eruption of *Veſuvius*, from being judged a phænomenon either altogether fabulous, (as doubtleſs many have thought it,) or a prodigy without all example, as is preſumed, even by thoſe, that think it not miraculous. And to this I add, that it will be the leſs improbable, that the more agile corpuſcles of ſubterraneal ſalts, ſulphurs and bitumens, may be raiſed into the air, and keep diſtinct natures there, if ſo fixed a body, as common earth itſelf, can be brought to ſwim in the air. And yet of this the worthy writer newly quoted gives us, beſides what annals relate, this teſtimony upon his own knowledge: [†] *Certè hîc Kempnicii undecimum abhinc annum menſe Septembri effluxerunt imbres, ſic cum terra lutea commiſti, ut eâ paſſim plateas ſcilicet ſtratas viderem conſperſas.*

AND to ſhew you, that in ſome caſes the particles even of vegetable bodies may not ſo ſoon periſh in the air, as they vaniſh there, but may retain diſtinct natures at a greater diſtance, than one would think, from the bodies, that copiouſly emit them; I ſhall add, that having deſired an ingenious gentleman, that went on a conſiderable employment to the *Eaſt-Indies*, to make ſome obſervations for me in his voyage, he ſent me, among other things, this remark: that having ſailed along the coaſt of *Ceylon*, (famous for cinnamon-trees and well-ſcented gums,) though they coaſted it almoſt a whole day, the wind, that then chanced to blow from the ſhore, brought them a manifeſtly odoriferous air from the iſland, though they kept off many miles (perhaps twenty or twenty-five) from the ſhore. Nor ſhould this be thought incredible, becauſe the diffuſion ſeems ſo diſproportionate to that of other bodies diſſolved by fluids; as, for inſtance, though ſalt be an active body, and reſoluble into abundance of minute particles, yet one part of ſalt will ſcarce be taſtable in an hundred parts of water. For ſenſibly to affect ſo groſs an organ, as that of our taſte, there is uſually required in ſapid particles a bigneſs far exceeding that, which is neceſſary to the making bodies fit objects for the ſenſe of ſmelling, and which is here mainly to be conſidered, there is a great difference between the power a body has to impregnate ſo thin and fine a fluid as air, whoſe parts are ſo rare and lax, and that, which it has to impregnate liquors, ſuch as water or wine, whoſe parts are ſo conſtipated as to make it, not only viſible and tangible, but ponderous. On which occaſion I remember, that having had a curioſity to try how far a ſapid body could be diluted, without ceaſing to be ſo, I found by trial, that one drop of good chemical, and, as artiſts call it, eſſential oil of cinnamon, being duly mixed, by the help of ſugar, with wine, retained the determinate taſte of cinnamon, though it were diffuſed into near a quart of wine. So that, making a moderate eſtimate, I concluded, that upon the common ſuppoſition, according to which a drop is reckoned for a grain, one part of oil had given the ſpecifick taſte of the ſpice, it was drawn from, to near fourteen thouſand parts of wine. By comparing which experiment, with what I noted about the proportion of ſalt requiſite to make water taſte of it, you will eaſily perceive, that there may be a very great difference, in point of diffuſiveneſs, between the little particles, that make bodies ſapid: which may ſerve to confirm, both ſome part of the firſt chapter of the foregoing eſſay of the ſubtilty of effluvia, and what I was lately ſaying, to ſhew it poſſible, that antimonial glaſs might impart ſtore of ſteams to the emetick wine, without appearing, upon common ſcales, to have loſt of its weight; ſince we ſee, that one drop of ſo light a body as oil, may communicate not inſenſible effluvia, but taſtable corpuſcles to near a quart of liquor. But this is not all, for which I mention our experiment: for I muſt now add, that beſides the almoſt innumerable ſapid parts of a ſpicy drop communicated to the wine, it thence diffuſed a vaſt number of odorous particles into the air, which both I, and others, perceived to be imbued with the diſtinct ſcent of cinnamon, and which, perhaps, the liquor would have been found able to have aromatized for I know not how long a time, if I had had leiſure to proſecute the obſervation.

CHAP. V.

THE third and laſt way I ſhall mention of ſhewing the determinate nature of effluviums, is to be taken from the conſideration of their effects upon other bodies, than the organs of our ſenſes; (for of their operations upon theſe, we have already ſpoken in the foregoing chapter.) For the effects, that certain bodies produce on others by their effluviums, being conſtant and determinate, and oftentimes very different from thoſe, which other agents, by their emiſſions, work upon the ſame, and other ſubjects, the diſtinct nature of the corpuſcles emitted may be thence ſufficiently gathered.

WE may, from the foregoing tract of the ſubtilty of effluvia, borrow ſome inſtances very pertinent to this place. For the temporary benumbedneſs, or ſtupefaction, for example, produced in the fiſherman's foot by the [*] effluvia of the fiſh *Amoreatim*, mentioned by the ingenious *Piſo*, manifeſts, that thoſe ſtupefying emanations retained a peculiar and venomous nature during their whole paſſage through the ſhoe, ſtocking, and ſkin, interpoſed betwixt the fiſh, and the nervous part of the foot benumbed by it. And though there are very few other bodies in the world, that are minute enough to paſs through the pores of glaſs, it is apparent, by the experiment there recited of the oblong iron hermetically ſealed up in a glaſs-pipe, that the magnetical effluvia of the earth may retain their peculiar and wonderful nature in a ſmallneſs, that qualifies them to paſs freely through the pores of glaſs itſelf.

[†] *Agric.* de nat. eorum, quæ è terra effluunt, Lib. XII. pag, 263.

[*] See the Eſſay of the Subtilty of Effluviums, Chap. IV.

But that I may neither repeat what you have already met with in the foregoing tract, nor anticipate what I have to ſay in the next; I will employ in this chapter ſome inſtances, that may be ſpared from both.

That divers bodies of a venemous nature may exerciſe ſome ſuch operations upon others, by their effluviums tranſmitted through the air, as they are wont to do in their groſs ſubſtance, is a truth, whereof though I have not met with many, yet I have met with ſome examples among phyſicians.

Lib. VI. parte 7. cap. 1. The learned *Sennertus* obſerves, as a known thing, that the apprentices of apothecaries have been caſt into profound ſleeps, when in diſtilling opiat and hypnotick liquors they have received in at their noſtrils the vapours exhaling from thoſe bodies.

It is recorded by the * writers about poiſons, that the root and juice of mandragora caſts thoſe, that take it, into a deep ſopor not unlike a lethargy. And though the apples of the ſame plant be thought to be much leſs malignant; yet *Levinus Lemnius* relates, that it happened to him more than once, that having laid ſome mandrake-apples in his ſtudy, he was by their ſteams made ſo ſleepy, that he could hardly recover himſelf; but the apples being taken away he regained alacrity, and threw off all drowſineſs.

Among all poiſons, there is ſcarce any, whoſe phænomena are in my opinion more ſtrange than thoſe, that proceed from a mad dog; and yet even this poiſon, which ſeems to require corpuſcles of ſo odd and determinate a nature, is recorded by phyſicians to have been conveyed by exhalations. *Aretæus* writes, as a learned modern quotes him, *Quòd à rabido cane, qui in faciem, dum ſpiritus adducitur, tantummodò inſpiraverit, & nullo modo momorderit, in rabiem homo agatur*. And as there are relations, among phyſicians, of animals, that have become rabioſi by having eaten of the parts or excrements of rabid animals; ſo Lib. III. Acutor. Morbor. *Cælius Aurelianus*, who writes, that ſome have been made to run mad, not by being bitten, but wounded only with the claws of a mad dog, tells us alſo of a man, that fell into a hydrophobia (which is wont to be a high degree of the rabies, and by ſome of the ancienter writers was employed to ſignify that diſeaſe) without being bitten by a mad dog, but infected *ſolo odore ex rabido cane attracto*. By which odours, in this, and other narratives of poiſons, I underſtand not a bare ſcholaſtick ſpecies, but a ſwarm of effluvia, which moſt commonly are all, or at leaſt ſome of them, odorous. And though it may juſtly ſeem ſtrange to many, that the venom of a mad dog ſhould be communicated otherwiſe than by biting, which is ſuppoſed to be the only way he can infect by, it may appear leſs improbable, becauſe *Matthæus de Gradibus* names a perſon, who, he ſays, proved infected after many days, by only having put his hand into the mouth of a mad dog, who did not bite him. And the formerly mentioned *Matthiolus* relates, that he ſaw two, that were made rabid without any wound by the ſlabber of a mad dog, with which they had the misfortune to be beſmeared.

Lib. VI. Part 6. cap. 2. Sennertus himſelf affirms of a painter of his acquaintance, that, when he had opened a box, in which he had long kept included realgar, a noxious mineral, ſometimes uſed by painters, and not unknown to chemiſts, and had unfortunately ſnuffed in the ſteams of it, he was ſeized with a giddineſs in his head and fainting fits, his whole face alſo ſwelling, though by taking of antidotes he eſcaped the danger.

Divers other examples we have met with in the writings of phyſicians, which I forbear to add to theſe, becauſe, I confeſs, I very much doubt the truth of them, though the deliverers of ſome of them be men of note. But the probability of moſt of the things already cited out of credible authors may be ſtrengthened by what I ſhall now ſubjoin, as a further proof of the diſtinct nature of effluvia; of which it will be a very conſiderable proof, if medicines, which are of a milder and more familiar nature and operation than poiſons, ſhall yet be able in ſome caſes to retain, in their inviſible particles ſwimming in the air, the ſame, though not ſo great, power of purging, which is known to belong to them when their groſs body is taken in at the mouth. Of this I have elſewhere, on another occaſion, given ſome examples. To which I ſhall now add, that I know a doctor of phyſick, that is uſually purged by the odours or exhalations of a certain electuary, whoſe cathartick operation, when it is taken in ſubſtance, is wont to be but languid. And another doctor of my acquaintance cauſing good ſtore of the root of black hellebore to be long pounded in a mortar, moſt of thoſe, that were in the room, and eſpecially the party that pounded it, were thereby purged, and ſome of them ſtrongly enough. And the learned *Sennertus* ſomewhere affirms, that ſome will be purged by the very odour of colocynthis. And it is not to be paſſed by unregarded, that in the caſes I have alledged, exhalations, that are endowed with occult qualities, (for thoſe of cathartick medicines are reckoned among ſuch) aſcend into the air, without being forced from the bodies they belonged to by an external heat.

And if I would in this place alledge examples of the operations of ſuch effluvia, as do not paſs into the air, but yet operate only by the contact of the external parts of the body, I could give inſtances, not only of the purgative, but the emetick qualities of ſome medicines, exerted without their being taken in at the mouth, or injected with inſtruments.

There are alſo other ſorts of examples, than thoſe hitherto mentioned, that argue a determinate nature in the effluxions of ſome bodies emitted into the air. Approved writers tell us, that the ſhadow of a walnut-tree, with the leaves on it, is very hurtful to the head; and ſome inſtances they give us, of great miſchief it has ſometimes done. And though the ſhadow, as ſuch, is not likely to be guilty of ſuch bad effects; yet the effluvia of the neighbouring plant may be noxious enough to the head. For I, that was not at all prepoſſeſſed with an opinion, that it was ſo, and therefore, without ſcruple, reſorted to the ſhade of walnut-trees in a hot country, was, by experience, forced to think it might give others the head-ach, ſince it did to me, who, thanks be to God, both was, and am ſtill very little ſubject to that diſtemper. And this brings into my mind an obſervation, that I have met with among ſome ingenious travellers into the *Weſt-Indies*, who obſerve in general, and, of late, a countryman of our own affirms it in particular, of the poiſonous manchinello-tree, that birds will not only forbear to eat of the fruit of venemous plants, but, as to ſome of them, will not ſo much as light on the trees: which I therefore mention, becauſe, probably, nature inſtructs them to avoid ſuch trees by ſome noxious ſmell, or other emanation, that offends the approaching birds. And I remember, that ſome of our navigators give it for a rule to thoſe, that happen to land in unknown iſlands, or coaſts, that they may venture to eat of thoſe parts of fruits which they can perceive the birds, like kind taſters, to have been pecking at before.

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* In Explicatione Herbarum Biblicarum, cap. 2.

NICOLAUS FLORENTINUS (cited by *Sennertus*) tells us of a certain Lombard, that having in a houſe, that he named, at *Florence*, burned a great black ſpider at the flame of a candle, ſo unwarily, that he drew in the ſteams of it at his noſtrils, preſently began to be much diſordered, and fell into a fainting fit, and, for the whole night, had his heart much diſaffected, his pulſe being ſo weak, that one could ſcarce perceive he had any; though afterwards he was cured by treacle, diamoſc, and the powder of zedoary, mixed together.

AND I remember, that being ſome years ago in *Ireland*, I gathered a certain plant (peculiar to ſome parts of that country) which the natives call Maccu-buy, becauſe of ſtrange traditions, that go about it; the chief of which I found, by trial, not to be true: but yet being ſatisfied, that its operations were odd, and violent enough, I was willing to gratify the chief phyſician of the country, who was deſirous I ſhould propoſe to him ſome ways of correcting it; and whilſt I was ſpeaking of one, that required the pounding of it, he told me on that occaſion, that intending to make an extract of it with vinegar, he cauſed his man to beat it well in a mortar, which the man ſoon repented he had begun to do: and the doctor himſelf, though at a pretty diſtance off, was ſo wrought upon by the corpuſcles, that iſſued out into the air, that his head, and particularly his face, ſwelled to an enormous and diſfiguring bulk, and continued tumid for no inconſiderable time after.

I have not leiſure to ſubjoin many more inſtances, to ſhew the determinate nature of effluviums, ſmall enough to wander through the air; nor perhaps will it be neceſſary, if you pleaſe but to conſider theſe two things. The firſt, that many odoriferous bodies, as amber, muſk, civet, &c. as they will, by the adheſion of their whole ſubſtance, perfume ſkins, linen, &c. ſo they will, in time, perfume ſome bodies diſpoſed to admit their action, though kept at a diſtance from them. And the other is, that in peſtilential fevers, and divers other contagious ſickneſſes, as the plague, ſmall-pox, or meaſles, the ſame determinate diſeaſe is communicable to ſound perſons, not only by the immediate contact of the infected party, but without it, by the contagious ſteams, that exhale from his body into the air. And having ſaid this, and deſired you to reflect upon it, I ſhall conclude this chapter with an experiment, that, poſſibly, will not a little confirm a great part of it.

CONSIDERING then with myſelf, how I might beſt deviſe a way of ſhewing to the very eye, that effluvia, elevated without the help of heat, and wandering in the air, may both retain their own nature, and, upon determinate bodies, produce effects, that a vulgar philoſopher would aſcribe to occult qualities; I remembred, that I had found by trials (made to other purpoſes) that volatile and ſulphureous ſalts would ſo work upon ſome acid ones, ſublimed with mercury, as to produce an odd diverſity of colours, but chiefly an inky one; on which account, I judged it likely, that my aim would be anſwered by the following experiment.

I took an ounce, or better, of ſuch a volatile tincture of ſulphur, as I have elſewhere * taught you to make of quick-lime, ſulphur, and ſal armoniack, and ſtopped it up in a vial capable of containing at leaſt twice as much; then taking a paper, whereon ſomething had been written with inviſible ink, I laid it down ſix inches off of the vial, which being unſtopped, began, upon the acceſs of the fire, to emit white fumes into it, and by theſe, what was written upon the paper, notwithſtanding its diſtance from the liquor, quickly became very legible, though not quite ſo ſuddenly, as if a paper, written with the ſame clear liquor, were held at the like diſtance directly over the orifice of the vial. And having cauſed ſeveral pieces of clean paper to be written on, with a new pen dipped in the clear ſolution of ſublimate, made in water, it was pleaſant to ſee, how divers of the letters of ſeveral of theſe papers, being placed within ſome convenient diſtance of the vial, would be made plainly legible, and ſome of them more, ſome leſs blackiſh, according to their diſtances from the ſmoking liquor, and other circumſtances. But it was more ſurprizing to ſee, that when I held, or laid ſome of

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* The liquor here mentioned is, for the main, the ſame with that deſcribed by the author in his book of Colours.

theſe papers, though with the written ſide upwards, juſt upon, or over the orifice of the vial, though the contained liquor did not, by ſome inches, reach ſo high, yet the latent letters would become not only legible, but conſpicuous, in about a quarter of a minute of an hour, meaſured by a good watch fit for the purpoſe, as more than one trial aſſured me. And as it may be obſerved, that in ſome circumſtances the ſmoking liquor, and the ſolution of ſublimate, will make an odd precipitate, almoſt of a ſilveriſh colour, ſo in one, or two of our trials, we found a like colour produced, by the ſteams of that liquor, in ſome of the colourleſs ink. Nor is it ſo neceſſary to employ a viſibly ſmoking liquor, for the denigrating of inviſible ink at a diſtance. For I have, to that purpoſe, with good ſucceſs, though not equal to that I have recited, employed a couple of liquors, wherein there was neither ſulphur, nor ſal armoniack, nor ſublimate. What other trials I made with our volatile tincture of ſulphur, it is not neceſſary here to relate; only one experiment, which you will poſſibly think odd enough, I ſhall not omit; becauſe it will not only confirm the precedent trials, but alſo much of the foregoing eſſay, by ſhewing the great ſubtilty, and penetrating power of effluviums, that ſeem rather to iſſue out very faintly, than to be darted out with any briſkneſs.

Causing then ſomething to be written with diſſolved ſublimate upon a piece of paper, we folded the paper with the written ſide inwards, and then incloſed this in the midſt of ſix ſheets of paper, laid one upon another, not placed one within another, and folded up in the form of an ordinary letter, or packet, to be ſealed, that the edges of the encloſing paper, being inſerted one within the other, the fumes might not get into this written paper, but by penetrating through the leaves themſelves: this done, that ſide of the packet, on which there was no commiſſure, and on which, were it to be ſent away, the ſuperſcription ſhould be written, was laid upon the orifice of the vial, which (as was before intimated) was ſome inches higher than the ſurface of the liquor, and left there about ten minutes; after which, taking off the folded papers, and opening them, we found, that the ſteams had pervaded all the leaves, in which the written paper had been encloſed. For, though the leaves did not appear ſtained or altered, yet the formerly latent characters appeared conſpicuous. I have not time to diſcourſe, whether, and how far, this experiment may aſſiſt us to explain ſome odd effects of thunder, or of that ſtrange phænomenon, (glanced at in the foregoing chapter,) which is ſaid to have happened lately in the kingdom of *Naples*, after the great eruption of *Veſuvius*, which is ſaid to have been followed by the appearing of the croſſes formerly mentioned, ſome of which have been found on the innermoſt parts of linen, that had been carefully folded up. But of theſe, and the like things, I ſay, I have now no time to diſcourſe, whether any thing derivable from our experiment may be pertinently applied to their explication. For which reaſon, I ſhall add no more, than that afterwards, for further trial, we took a printed book, that chanced to be at hand, and which we judged the fitteſt for our purpoſe, becauſe the leaves being broad, they might the better preſerve a ſmall paper to be placed in the midſt of them, from being acceſſible to the exhalations ſide-wiſe, and having put the deſigned paper into this book, and held it to the orifice of the vial, though there were no leſs than twelve leaves between them, yet thoſe letters, that happened to be the moſt rightly placed, were made inky in the ſhort ſpace of three minutes, at the utmoſt; though this liquor had been ſo long kept, and ſo often unſtopped to try concluſions with it, that it had probably loſt a good part of the moſt ſpirituous and piercing particles.

NEW EXPERIMENTS
TO MAKE
FIRE AND FLAME
STABLE AND PONDERABLE.

A PREFACE; ſhewing the Motive, Deſign, and Parts of the enſuing TRACT.

THE inducements, which put me upon the attempt, expreſſed in the title of this eſſay, were chiefly theſe:

FIRST, I conſidered, that the interſtellar part of the univerſe, conſiſting of air and æther, or fluids analogous to one of them, is diaphanous; and that the æther is, as it were, a vaſt ocean, wherein the luminous globes, that here and there, like fiſhes, ſwim by their own motion, or like bodies in whirlpools are carried about by the ambient, are but very thinly diſperſed, and conſequently, that the proportion, that the fixed ſtars, and planetary bodies, bear to the diaphanous part of the world, is exceeding ſmall, and ſcarce conſiderable, though we ſhould admit the ſun, and fixed ſtars, to be opacous bodies, upon the account of their terminating our ſight: which diffident expreſſion I employ, becauſe I have elſewhere ſhewn, by two or three experiments, purpoſely deviſed, that a body may appear opacous to our eyes, and yet allow free paſſage to the beams of light.

I further conſidered, that there being ſo vaſt a diſproportion between the diaphanous part of the world, and the globes, about which it is every way diffuſed, and with which it is ſometimes in great portions mingled, as in the water, which, together with the earth, makes up the globe we inhabit; and the nature of a diaphanous body's being ſuch, that when the ſun, or any other luminous body, illuſtrates them, that, which we call light, does ſo penetrate, and mix itſelf *per minima*, with them, that there is no ſenſible part of the tranſparent body unenlightened; I thought it worth the enquiry, whether a thing, ſo vaſtly diffuſed as light is, were ſomething corporeal, or not? and whether, in caſe it be, it may be ſubjected to ſome other of our ſenſes, beſides our ſight, whereby we may examine, whether it hath any affinity with other corporeal beings, that we are acquainted with here below?

I did not not all this while forget, that the Peripateticks make light a meer quality, and that *Carteſius* ingeniouſly endeavours to explicate it, by a modification of motion in an ætherial matter: but I remembered too, that the Atomiſts of old, and of late the learned *Gaſſendus*, and many other philoſophers, aſſert light to be corporeal; and, that ſome years ſince, though I declined to paſs my judgment about the queſtion, yet I had employed arguments, that appeared plauſible enough to ſhew, that it was not abſurd to ſuppoſe, that the ſun, which is the fixed ſtar moſt known to us, might be a fiery body. And therefore doubting, whether the corporeity of light would be in haſte determined by meer ratiocinations, I thought it very well worth the endeavouring to try, whether I could do any thing towards clearing the diſpute of it by experiments; eſpecially being perſuaded, that, though ſuch an attempt ſhould be ineffectual, it would but leave the controverſy in its former ſtate, without prejudicing either of the contending hypotheſes; and yet, if it ſhould prove ſucceſsful, the conſequences of it would be very great and uſeful towards the explicating of divers phænomena in divers parts of natural philoſophy, as in chemiſtry, botanicks, and (if there be any ſuch) the allowable part of aſtrology. (Nor perhaps would it be impoſſible, by the help of ſlight theorical alterations, to reconcile the experiments, I deſigned, to either of the above-mentioned hypotheſes, and ſo, as to the explication of light, to one another.)

To compaſs then, what I aimed at, I thought it was fit, in the firſt place, to try what I could do by the union of the ſun-beams, they being on all hands confeſſed to be portions (as I may ſo ſpeak) of true and celeſtial light: and then I thought fit to try, what could be obtained from flame; not only, becauſe that is acknowledged to be a luminary, but becauſe I hoped the difficulties, I foreſaw in the other trials, might be, in ſome meaſure, avoided in thoſe made with flame; and if both ſorts of them ſhould ſucceed, the latter and former would ſerve to confirm each other. According to the method I propoſed of handling theſe two ſubjects, I ſhould begin with ſome account of what I attempted to perform in the ſun-beams. But the truth is, that when I chanced to fall upon the enquiry, that occaſioned this paper, beſides that the time of the year itſelf was not over-favourable, the weather proved ſo extraordinary dark, and unſeaſonable, that it was wondered at; ſo that, though I was furniſhed with good burning-glaſſes, and did ſeveral times begin to make trials upon divers bodies, as lead, quick-ſilver, antimony, &c. yet the frequent interpoſition of clouds, and miſts, did ſo disfavour my attempts, that, however they were not all alike defeated, yet I could not proſecute the greateſt part of them to my own ſatisfaction. And therefore, being unwilling to build on them as yet, I ſhall reſerve an account of them for another opportunity; and now proceed to the mention of that ſort of experiments, which depending leſs on caſualties, it was more in my power to bring to an iſſue.

I know, I might have ſaved both you, and myſelf, ſome time and pains, by omitting ſeveral of theſe trials, and by a more compendious way of delivering the reſt. But I rather choſe the courſe I have taken; partly, becauſe the novelty, and improbabilities of the truth I deliver, ſeem to require, that it be made out by a good number of trials; partly, becauſe I thought it might not be altogether uſeleſs to you, and your friends, to ſee upon what inducements the ſeveral ſteps were made in this enquiry; partly, becauſe I was willing to contribute ſomething towards the hiſtory, that now, perhaps, will be thought fit to be made of the increment, or decrement, that particular bodies may receive by being expoſed to the fire; and partly, in fine, becauſe the incongruity of the doctrine here aſſerted to the opinions of the ſchools, and the general prepoſſeſſions of mankind, made me think it fit, by a conſiderable variety, as well as number of experiments, to obviate, as far as may be, the differing objections, and evaſions, wherewith a truth, ſo paradoxical, may expect to be encountered.

NEW EXPERIMENTS, &c.

THOUGH there be among the following trials, a diverſity, that invites me, as to rank them into four or five differing ſorts, ſo to aſſign them as many diſtinct ſections; yet, for the conveniency of making the references, there will be occaſion to make betwixt them, I ſhall wave the diſtinction, and ſet them down in one continued ſeries.

AND becauſe I am willing to comply with my haſte, as well as to deal frankly, and without ceremony, with you, I ſhall venture to ſubjoin the naked tranſcrips of my experiments, as I had in an artleſs manner ſet them down, with many others, for my own remembrance, among my *adverſaria*, without ſo much as retrenching ſome circumſtances, that relate leſs to my preſent argument, than to ſome other purpoſes.

I ſhall then begin with the mention of a couple of experiments, which, though they might conveniently enough be referred to another paper, yet I ſhall here ſet them down, becauſe it ſeems very proper to endeavour to ſhew in the firſt place, that flame itſelf may be, as it were, incorporated with cloſe and ſolid bodies, ſo as to increaſe their bulk and weight.

TRIALS of the Firſt Sort.

EXPERIMENT I.

[A PIECE of copper-plate not near ſo thick as a half-crown, and weighing two drachms and twenty-five grains, was ſo placed, with its broad part horizontal, in a crucible, whoſe bottom had a little hole in it, for fumes to get out at, that it could not be removed from its poſition, nor be eaſily made to drop down, or loſe its level to the horizon, though the crucible were turned upſide down: then about an ounce and half of common ſulphur being put into a taller and broader crucible, that, wherein the copper ſtuck, was inverted into the orifice of it, that the ſulphur being kindled, the flame, but not the melted brimſtone in ſubſtance, might reach the plate, and have ſome vent beyond it at the above-mentioned hole. This brimſtone burned about two hours, in which time it ſeemed all to have been reſolved into flame, no flowers of ſulphur appearing to have ſublimed into the inſide of the upper crucible; and though the copper-plate were at a conſiderable diſtance from the ignited ſulphur, yet the flame ſeemed to have really penetrated it, and to have made it viſibly ſwell, or grow thicker; which appeared to be done by a real acceſſion of ſubſtance; ſince, after we had wiped off ſome little adhering ſordes, and with them divers particles of copper, that ſtuck cloſe to them, the plate was found to weigh near two and thirty grains more than at firſt, and conſequently to have increaſed its former weight by above a fifth part.]

EXPERIMENT II.

[HAVING, by refining one ounce of ſterling ſilver with ſalt-petre, according to our way, reduced it to ſeven drachms or ſomewhat leſs; we took a piece of the thus purified ſilver, that weighed one drachm wanting two grains, and having ordered it as the copper-plate had been in the former experiment, after the flame of above one ounce and a quarter of ſulphur, (that quantity chancing to be ſuitable to the capacity of the crucible) had, for about an hour and a half, beat upon it, the ſilver-plate ſeemed to the eye ſomewhat ſwelled, and the lower ſurface of it, that was next the flame, was brought to a great ſmoothneſs, the weight being increaſed to one drachm five grains and three quarters; which increaſe of weight falling ſo ſhort of that, which was gained by the copper, I leave it to you to conſider, whether the difference may be attributed to the cloſeneſs and compactneſs of the ſilver, argued by its being heavier in ſpecie than copper; or to the greater congruity of the pores of copper to be wrought on by the fiery menſtruum; or to ſome other cauſe.]

IF you ſhould here aſk me, by what rational inducements I could be led to entertain ſo extravagant an expectation, as, that ſuch a light and ſubtile body as flame ſhould be able to give an augmentation of weight to ſuch ponderous bodies as minerals and metals; I ſhall now, to avoid making anticipations here, or needleſs repetitions hereafter, return you only this anſwer; that the expectation you wonder at, may juſtly be entertained upon the ſame, or ſuch like inducements, as you may eaſily diſcover in another paper, entitled *Corollarium Paradoxum*. For, ſuppoſing upon the grounds there laid, that flame may act upon ſome bodies as a menſtruum, it ſeems no way incredible, that, as almoſt all other menſtruums, ſo flame ſhould have ſome of its own particles united with thoſe of the bodies expoſed to its action: and the generality of thoſe particles being, (as it is ſhewn in the paradox about the fewel of flames,) either ſaline, or of ſome ſuch piercing and terreſtrial nature, it is no wonder, that being wedged into the pores, or being brought to adhere very faſt to the little parts of the bodies expoſed to their action, the acceſſion of ſo many little bodies, that want not gravity, ſhould, becauſe of their multitude, be conſiderable upon a balance, whereon one or two, or but few of theſe corpuſcles, would have no viſible effect.

I could here, if it were expedient, mention ſome odd ſcruples about the preceding experiments, and ſome alſo of the ſubſequent; but, leſt you ſhould, with ſome other of my friends, upbraid me with being too jealous and ſceptical, I will not trouble you with them; but proceed to the next ſort of trials, wherein, though the matter were not always manifeſtly beaten on by a ſhining flame; yet it was wrought on by that, which would be called flame, by thoſe, who take not that word ſtrictly, but in a latitude, and which this igneous ſubſtance may more properly be ſtiled, than it can be called common fire, this being viſibly harboured in burning coals, or other groſs materials, from which our metals were fenced. And I have elſewhere ſhewn, by experiment, that viſibility is not in all caſes neceſſary to actual flame, particularly, when the eye receives a predominant impreſſion from another light.

TRIALS of the Second Sort.

EXPERIMENT III.

INTO a crucible, whoſe ſides had been purpoſely taken down to make it very ſhallow, was put one ounce of copper-plates; and this being put into our cupelling-furnace, and kept there two hours, and then being taken out, we weighed the copper (which had not been melted) having firſt blown off all the aſhes, and we found it to weigh one ounce and thirty grains.

EXPERIMENT IV.

[SUPPOSING that copper, being reduced to filings, and thereby gaining more of ſuperficies in proportion to its bulk, would be more expoſed to the action of the fire, than when it is in places, as it was formerly, we took an ounce of that metal in filings, and putting them upon a very ſhallow crucible, and under a muffler, we kept them there about three hours, (whilſt other things, that required ſo long a time were cupelling;) and afterwards taking them off, we found them of a very dark colour, not melted, but caked together in one lump, and encreaſed in weight (the aſhes and duſt being blown off) no leſs, than about forty-nine grains. Part of which increment, above that obtained by the copper-plates in the former experiment, may not improbably be due to the longer time, that in this experiment the filed copper was kept in the fire.]

EXPERIMENT V.

[BEING willing to ſee, whether calcined hart's-horn, that I did not find eaſy to be wrought on by corroſive menſtruums, would retain any thing of the flame, or fire, to which it ſhould be expoſed; we weighed out one ounce of ſmall lumps of hart's-horn, that had been burnt till they appeared white, and having put them into a crucible, and kept them in a cupelling-furnace for two hours, whilſt ſome metals were driving off there by the violence of the fire; we found, that when they were taken out, they had loſt ſix or ſeven grains of their former weight; perhaps either becauſe, notwithſtanding the external whiteneſs of the lumps, the internal parts of ſome of them might not be ſo exquiſitely calcined, but retain ſome oleaginous or other volatile ſubſtance; or becauſe, having omitted to ignite them well before they were weighed, they may have ſince their firſt calcination imbibed ſome moiſt particles of the air. Which conjecture ſeemed the likelier, becauſe having kept them a while in the ſcales they were weighed in, they did within two or three hours make it ſomewhat preponderate. On which occaſion I ſhall add, that, at the ſame time, with the hart's-horn we put in one ounce of well-heated brick, and kept that likewiſe in the furnace for above two hours; at the end of which, weighing it whilſt it continued hot, we did not find it to have either ſenſibly got or loſt; but, ſome time after, it ſeemed upon the ballance to have imbibed ſome, though but very little, moiſture from the air.]

EXPERIMENT VI.

[UPON a good cupel we put one ounce of Engliſh tin of the better ſort, and having placed it in the furnace under a muffler, though it preſently melted, yet it did not forſake its place, but remained upon the concave ſurface of the cupel, till at the end of about two hours, it appeared to have been well calcined; and then being taken out and weighed by it ſelf, the ounce of metal was found to have gained no leſs than a drachm.]

EXPERIMENT VII.

[AN ounce of lead was put upon the cupel, made of calcined hart's-horn, and placed under the muffler, after that the cupel was firſt made hot, and then weighed. This lead did not enter into the cupel, but was turned into a pretty kind of litharge on the top of it, and broke the cupel, whereby ſome part of the cupel was loſt in the furnace, and yet the reſt, together with the litharge, weighed ſeven grains more than the ounce of lead and the heated cupel did, when they were put in.]

BUT becauſe, though this trial ſhewed, that ſome weight was gained either by the metal or cupel, or both, yet it did not by this appear, what either of them acquired; it ſeemed fit to ſubjoin a further trial.

EXPERIMENT VIII.

[WE took a cupel about two ounces in weight, made of about ten parts of bone-aſhes, and one of charcoal-aſhes, made up together with ale. This was by itſelf put in a cupelling-furnace, under a muffler; and the laborant, well verſed in weighing, was ordered to take it out, when it was throughly and highly heated, and to weigh it whilſt it was in that condition, I being then preſent: this being done, it was forthwith placed again under the muffler, where ſome metalline bodies were cupelling, and kept there for about two hours; at the end of which time it was taken out red-hot, and preſently put into the ſame ballance, as before, which was already faſtened to a gibbet; where having cauſed the adhering aſhes to be blown off, I found, that whereas, when it was firſt taken from under the muffler, we had but two ounces and two grains, now the ſame weight being put into the oppoſite ſcale, it had gained very near one and twenty grains. And here note, that it was not without ſome cauſe, that I was careful to have the cupel weighed red-hot. For I had a ſuſpicion, that, notwithſtanding the dryneſs of the bone, it might receive ſome little alteration of weight by imbibing ſome little particles wandering in the air; which ſuſpicion the event juſtified. For leaving the cupel counterpoiſed to cool in the balance, in a ſhort time it began ſenſibly to preponderate; and ſuffering it to continue there nine or ten hours, till we had occaſion to uſe the balance, I found it at the end of that time to be about three grains heavier than before.]

THIS was not the only trial we made about the augmenting the weight of cupels; but this being the faireſt, and exempt from thoſe miſchances, from which the other were not altogether free; I ſhall content myſelf to have ſet down this: in the mention of which I thought fit to take notice of the increaſe of the weight of the cupel after it had lain in the ſcales, and alſo that we weighed it at firſt, whilſt it was throughly hot, becauſe thoſe circumſtances, as not being ſuſpected, may eaſily be left unthought on, even by ſkilful experimenters; and yet the weighing of the cupel, when it had been well nealed, and the not weighing it ſoon enough after it is taken from the fire, may keep thoſe, that ſhall reiterate this experiment, from making it cautiouſly and accurately enough. For if the former circumſtance be omitted, that, which the cupel may ſeem to have loſt of its ſubſtance, was nothing but the adventitious moiſture of the air; and if the latter circumſtance be neglected, the weight it may ſeem to have gained from the fire, was indeed due to the wateriſh particles of the air. I could wiſh alſo, that trial were made, whether the ſucceſs would be the ſame in cupels made in differing ſorts of bone-aſhes, and other materials, wont to be employed for that purpoſe. For that I had not opportunity to do.

EXPERIMENT IX.

IRON being a metal, that experience had informed me will more eaſily be wrought on by fluids, that have particles of a ſaline nature in them, than is commonly believed; it was not unreaſonable to expect, that flame would have a greater operation on it, (eſpecially if it were before-hand reduced to ſmall parts) than on any of the bodies hitherto deſcribed. Which ſuppoſition will be confirmed by the ſhort enſuing note.

[FOUR drachms of filings of ſteel being kept two hours on a cupel under a muffler, acquired one drachm ſix grains and a quarter increaſe of weight.]

EXPERIMENT X.

[A Piece of ſilver, refined in our own laboratory, being put upon a cupel under a muffler, and kept there for an hour and half, whilſt other things were refining, was taken out and weighed again, and, whereas before it weighed three drachms, thirty-two grains and a quarter, it now weighed, in the ſame ſcales, three drachms, thirty-four grains and a half, or but little leſs.]

FINDING this memorial among divers others about the weight of bodies, expoſed to the fire, I thought it not amiſs to annex it in this place; though finding it to be but ſingle, I would not have it to be relied on, till further trial have been made to diſcover, whether it was more than a caſual and anomalous experiment; and if the ſilver had not been refined, I ſhould have ſuſpected, that the copper, that was blended with it, as it is uſually blended with common ſilver, might have occaſioned the increaſe of weight.

POSTSCRIPT.

SINCE the foregoing experiment was firſt ſet down, meeting with an opportunity to reiterate the trial once more, we did it with half an ounce of filings of ſilver, well refined with lead in our own laboratory, and kept it about three hours upon the cupel; after the end of which time taking it out, we found it to be of a leſs pleaſant colour than it was of before, and melted (though not ſo perfectly) into a lump, which weighed four drachms and ſix grains; and yet, the ſucceſs being ſo odd, and, if it prove conſtant, of ſuch moment, I could wiſh the trial were further repeated in differing quantities of the metal.

EXPERIMENT XI.

[WE took a drachm of filings of zink or ſpelter, and having put it upon a cupel under a muffler, we kept it there in a cupelling-fire about three hours, (having occaſion to continue the cupellation ſo long for other trials;) then taking it off the cupel, we found it to be caked into a brittle and dark coloured lump, which looked as if the filings had been calcined. This being weighed in the ſame ſcales gained full ſix grains, and ſo a tenth part of its firſt weight.]

EXPERIMENT XII.

AMONG our various trials upon common metals, we thought fit to make one or two upon a metal brought us from the *Eaſt-Indies*, and there called Tutenâg, which name being unknown to our European chemiſts, I have elſewhere endeavoured to give ſome account of the metal it ſelf; whence I ſhall borrow the enſuing note, as directly belonging to our preſent purpoſe.

[Two drachms of filings of tutenâg being put upon a cupel, and kept under the muffler, for about two hours, the filings were not melted into a lump of metal, but looked as if ceruſs and minium being powdered had been mingled together; ſome of the parts appearing diſtinctly white, and others red: the calx being put into the balance appeared to have gained twenty-eight grains and a quarter. Another time the experiment being re-iterated with the like circumſtances, we found, that two drachms of the filed tutenâg gained the like increaſe of weight, abating leſs than one grain.]

So that this Indian metal ſeems to have gained more in the fire, in proportion to its weight, then any we have hitherto made trial of.

EXPERIMENT XIII.

[BEING deſirous to confirm, by a clear experiment, what I elſewhere deliver contrary to the vulgar opinion of thoſe, that believe, that in all cupellations almoſt all the lead, that is employed about them, does, together with the baſer metals, that are to be purged off from the ſilver or gold, fly away in ſmoke, as indeed in ſome ſort of cupellations a good proportion may be blown off that way; we took two ounces of good lead, and one drachm of filings of copper, and having cauſed a cupel to be ignited, and nimbly taken out of the furnace, and weighed, whilſt it was very hot, it was preſently put back, together with the two metals laid on it, into the cupelling-furnace, where having been kept for about two hours, it was taken out again, and it was found, according to what (as I elſewhere* note) uſes to happen in ſuch circumſtances, to have nothing on the ſurface of it worth weighing diſtinctly in the ſcales, in which the cupel, with what was ſunk into it, amounted to four ounces, three drachms and eleven grains, which wanted but nine grains of the whole weight of the cupel and the two metals, when they were all three together committed to the fire.] So that, though we make a liberal allowance for the increment of weight, that may with any probability, be ſuppoſed to have been attained by the cupel, and what was put upon it, yet it will eaſily be granted, that very much the greater part of the metals was not driven off in fumes, but entered into the ſubſtance of the cupel.

TRIALS of the Third Sort.

AFTER having ſhewn, that either flame, or the analogous effluxions of the fire, will be, what chemiſts would call, corporified with metals and minerals expoſed naked to its action; I thought it would be a deſirable thing to diſcover, whether this flame, or igneous fluid, were ſubtile enough to exerciſe any ſuch operation upon the light bodies, ſheltered from its immediate contact, by being included in cloſe veſſels; but it being very difficult to expoſe bodies in glaſſes, to ſuch vehement fires, without breaking or melting the glaſs, and thereby loſing the experiment; I thought fit, firſt, to employ crucibles carefully luted together, that nothing might viſibly get in or out; and of that attempt, I find among my notes, the following account.

EXPERIMENT XIV.

[WE took an ounce of ſteel, freſhly filed from a lump of that metal, that the filings might not be ruſty, and having included them betwixt two crucibles, as formerly, kept them for two hours in a ſtrong fire, and ſuffered them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard caked together, and had acquired a dark colour, ſomewhat between black and blue, and were increaſed five grains in weight.]

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* Eſſay the Sixth of the Uſefulneſs of Natural Philoſophy.

THE foregoing experiment being the firſt I mention of this kind, it will not be amiſs to confirm it, by annexing the following memorial.

[AN ounce of filings of ſteel being put between the crucibles luted together, after they had been kept about an hour and half in the fire, were taken out, and being weighed, were found to have gained ſix grains.]

EXPERIMENT XV.

[TWO ounces of copper-plate were put into a new crucible, over which a leſſer was whelmed, and the commiſſures were cloſed with lute, that nothing might fall in. After the ſame manner, two ounces of tin were included betwixt crucibles, and alſo two ounces of lead; theſe being put into the cupelling-furnance, were kept in a ſtrong fire about an hour and a half, while ſomething elſe was trying there. And then being taken out, the event was, that the copper-plates, though they ſtuck together, were not quite melted, and ſeemed, ſome of them, to have acquired ſcales like copper put into a naked fire, and the two ounces had gained eight grains in weight. The lead had broke through the bottom of the crucible, and thereby hindered the deſigned obſervation. The tin acquired ſix grains in weight, and was, in part, brought to a pure white calx, but much more of it was melted into a lump of a fine yellow colour, almoſt like gold, but deeper.] The proſecution of this trial, as to the copper-plates, you will meet with in Experiment XXI. to which I therefore refer you.

N. B. BECAUSE lead, in cupellation, enters the cupel, we were willing to try, if we could ſo far hinder it from doing ſo, as to make ſome eſtimate, what change of weight the operation of the fire would make in it: and therefore being able already to make a near gueſs, how much a quantity of tin may gain by being calcined on a cupel, and remembering alſo, from ſome of my former trials, the indiſpoſition, which tin gives lead to cupellation, we mixed a drachm of tin with two ounces of lead, and expoſing the mixture (in a cupel) to the fire under a muffler, we firſt brought it to fuſion, and then it ſeemed at the top dry and ſwelled, and diſcoloured; notwithſtanding which, having continued the operation a good while, becauſe of other things, that were to be done with the ſame fire, we were not lucky enough to bring the experiment to an iſſue worth the relating here, in reference to the ſcope above-propoſed, though in relation to another, the ſucceſs was welcome enough.]

EXPERIMENT XVI.

[SUPPOSING, that if copper were beaten into thinner plates than thoſe we lately uſed, and kept longer in the fire, this would have a more conſiderable operation upon them, we took one ounce of very thinly hammered pieces of copper, and putting them betwixt two crucibles (one whelmed over another) as in Experiment XV. with ſome lute at the corners of the juncture, to keep the fire from coming immediately at the metal, we kept them in the cupelling-furnace about three hours, and then disjoining the veſſels, we found the metal covered with a dark and brittle ſubſtance, like that deſcribed in the above-recited experiment. Which ſubſtance, when ſcaled off, diſcloſed a finely coloured metal, which, together with theſe burned ſcales, amounted to one and twenty grains above the weight, that was firſt put in.]

IF, when theſe things were doing, I had been furniſhed with a very good lute, which is no ſuch eaſy thing to procure, as chemiſts, that have not frequently employed vulgar lutes, are wont to think; I would have made a trial of the enſuing experiment, for a good while, in the naked fire, notwithſtanding that divers metalline minerals will ſcarce be brought to fuſion in glaſſes, eſpecially without ſuch a fire, whoſe violence makes them break the veſſels. For I thought, that by making a fit choice of the metals to be employed, I could prevent that inconvenience; but wanting the accommodations I deſired, and yet preſuming, that in a ſand-furnace, I might by degrees adminiſter heat enough to melt ſo fuſible a metal as fine tin, and keep it in fuſion; I reſolved to make ſome trials, firſt upon that, and then upon another metal. For though I was not ſure of being then able to proſecute the experiment far enough; yet I hoped, I might, at leaſt, ſee ſome effects of my firſt trial, which would enable me to gueſs, what I was to expect from a compleat one.

EXPERIMENT XVII.

[WE took then a piece of fine block-tin, and in a pair of good ſcales weighed out carefully half a pound of it: this we put into a choice glaſs-retort, and kept it for two days, or thereabouts, in a ſand-furnace, which gave heat enough to keep the metal in fuſion, without cracking the glaſs. Then taking out the mixture, we carefully weighed it in the ſame ſcales, and found the ſuperficies a little altered (as if it were diſpoſed to calcination) and the weight to be increaſed about two grains, or ſomewhat better.]

EXPERIMENT XVIII.

[THE other experiment, I tried in glaſſes, was with mercury, hoping, that, if I could make a precipitate *per ſe*, in a hermetically ſealed glaſs, I ſhould, by comparing the weight of the precipitate, and the quick-ſilver, that afforded, have a clear experiment to my purpoſe; and I ſhould have no bad one, if I could but make it ſucceed with a glaſs, though not ſealed, yet well ſtopped; inſtead of thoſe infernal glaſſes (as they call them) which are commonly uſed, and wont to be left open (though ſome ſlightly ſtop them with a little paper, or cotton:) but though, partly, that I might a little diverſify the experiment, and make it the more likely to ſucceed in one or other of the glaſſes, I divided the mercury, and diſtributed it amongſt ſeveral of them, and but a little to each, the ſucceſs did not anſwer expectation, the hermetically ſealed glaſſes being unluckily broken; and the precipitation in the others proceeding ſo ſlowly, that I was, by a remove, obliged to leave the trial imperfect: only I was encouraged (in caſe of a future opportunity) to renew it another time, by finding, that moſt of the glaſſes, though tall, and ſtopped with fit corks, afforded ſome very fair precipitate, but not enough to anſwer my deſign.]

TRIALS of the Fourth Sort.

MOST of the experiments hitherto recited having been made, as it were, upon the by with others, whoſe exigencies it was fit theſe ſhould comply with; very few of the expoſed bodies were kept in the cupelling-fire above two hours, or thereabouts. Upon which account, I thought fit to try, how much ſome bodies, that had been already expoſed to the fire, would gain in weight, by being again expoſed to it; eſpecially conſidering, that moſt calcinable bodies, (for I affirm it not of all,) which yield rather calces, than aſhes, by being without additament reduced in the fire to fine powder, ſeemed to be by that operation opened, or (as a chemiſt would ſpeak) unlocked, and therefore, probably, capable of being further wrought upon, and increaſed in weight, by ſuch a menſtruum, as I ſuppoſed flame, and igneous exhalations to be. And about this conjecture, I ſhall ſubjoin the enſuing trials.

EXPERIMENT XIX.

ONE ounce of calx of tin, that had been made *per ſe*, for an experiment in our own laboratory, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the ſcales, where the powder appeared to have gained in weight one drachm, and thirty-five grains, by the operation of the fire, which made it alſo look much whiter than it did before, as appeared by comparing it with ſome of the calx, that had not been expoſed to the ſecond fire: no part of the putty was, as we could perceive, melted by the vehemence of the fire, much leſs reduced into metal.]

EXPERIMENT XX.

[OUT of a parcel of filings of ſteel, that had been before expoſed to the fire, and had its weight thereby increaſed ſome grains, not ſcruples; we took an ounce, and having expoſed it at the ſame time with the calx of tin, and, for the ſame time, kept it in the fire, we took it out at the two hours end; and found the weight to be increaſed two drachms, and two and twenty grains. The filings were very hard baked together, and the lump being broken, looked almoſt like iron.]

EXPERIMENT XXI.

THE following experiment, though it may ſeem in one regard but a continuation of the fifteenth, yet it has in this ſomething peculiar from all the foregoing, that not only it affords an inſtance of the increaſe of weight obtained by a metal at the ſecond time of its being expoſed to the fire, but ſhews alſo, that ſuch an increment may be had, though this ſecond ignition be made in cloſe veſſels.

[SOME of the copper mentioned in Experiment XV. being accidentally loſt, one ounce and four drachms of what remained was included betwixt two crucibles and expoſed to a ſtrong fire for two hours, and ſuffered to continue there till the fire went out: when it was taken out, it appeared to have gained ten grains in weight, and to have upon the ſuperficial parts of the plates (as we obſerved) divers dark-coloured flakes, ſome of which ſtuck to the metal, but more, upon handling it, fell off.]

AND here I ſhall conclude one of the two parts of our deſigned treatiſe: for, though, I remember, that theſe were not all the trials, that were made, and ſet down, upon the ſubject hitherto treated of; yet theſe are the chief, that having eſcaped the miſchances, which befel ſome others, I can meet with among my promiſcuous memorials; whoſe number, when I drew them together, I could ſcarce increaſe, having by all theſe, and other trials of differing kinds, waſted my cupels, and commodious glaſſes, where I could not well repair my loſs. Whether I ſhould have been able by reduction, ſpecifick gravity, or any other of the ways, which I had in my thoughts, to make any diſcovery of the nature of the ſubſtance, that made the increment of weight in our ignited bodies; the want as well of leiſure, as of accommodations requiſite to go through with ſo difficult a taſk, keeps me from pretending to know. But theſe three things, I hope, I may have gained by what has been delivered: the firſt, that we ſhall henceforth ſee cauſe to proceed more warily in the experiments we make with metals in the fire, eſpecially by cupellation. The next, that it will juſtify, and, perhaps, procure an eaſier aſſent to ſome paſſages in my other writings, that have relation to the ſubſtance, whatever it be, that we are ſpeaking of. And the third, (which is the principal,) that it will probably excite you, and your inquiſitive friends, to exerciſe their ſagacious curioſity, in diſcovering what kind of ſubſtance that is, which, though hitherto overſeen by philoſophers themſelves, and, being a fluid far more ſubtil than viſible liquors, and able to pierce into the compact and ſolid bodies of metals, can yet add ſomething to them, that has no deſpicable weight upon the balance, and is able, for a conſiderable time, to continue fixed in the fire.

ADDITIONAL EXPERIMENTS,

About ARRESTING and WEIGHING of

IGNEOUS CORPUSCLES.

EXPERIMENTS to diſcover the increaſe in weight of bodies, though incloſed in glaſſes, being thoſe, that I conſidered as likelieſt to anſwer what I deſigned in the hitherto proſecuted attempt, and finding the ſeventeenth experiment, as well as the next (tried upon mercury) to be very ſlow, and its performance not to be very great, I began to call to mind, what, many years ago, experience had ſhewn me poſſible to be performed, as to the managing glaſs-veſſels, even without coating them, in a naked fire, provided a wary perſon were conſtantly employed to watch them. And ſuppoſing hereupon, that in no longer time, than a laborant might, without being tired, hold out to attend a glaſs, a metal expoſed in it to a naked fire might afford us a much more proſperous trial than that lately referred to, I afterwards reſolved, when I ſhould be able to procure ſome glaſſes conveniently ſhaped, to proſecute my deſign; in purſuance of which, though I had not any furnaces fitted for my purpoſe, I directed a laborant to make the following trials.

EXPERIMENT I.

[WE took eight ounces (*Troy* weight) of block-tin, which being cut into bits, was put into a good round vial with a long neck, and then warily held over quick coals, without touching them, till it was melted; after which, it was kept almoſt continually ſhaken, to promote the calcination, near an hour, the metal being all the while in fuſion, and the glaſs kept at ſome diſtance from the thoroughly kindled coals. The moſt part of this time the orifice of the vial was covered with a cap of paper (which ſometimes fell off by moving the glaſs) to keep the air, and ſteams of the coals, from getting into the neck. And at the end of this time, he, that held the glaſs, being tired, and having his hand almoſt ſcorched, the vial being removed from the fire, was broken, that we might take out the metalline lump, which had a little darkiſh calx here and there upon the upper ſurface, but much more beneath, where it had been contiguous to the bottom of the glaſs; then putting all this, carefully freed from little fragments of broken glaſs, into the ſame balance with the ſelf-ſame counterpoiſe I had uſed before, I found, according to my expectation, an increaſe of weight, which amounted to eighteen grains, that the tin had acquired by this operation.]

EXPERIMENT II.

[THIS done, we ſeparated the calx for fear of loſing it, and having melted the metal in a crucible, that by pouring it out it might be reduced to thin plates, capable of being cut in pieces, and put into ſuch another vial as the laſt; we weighed it again, together with the lately reſerved calx, but found, that notwithſtanding all our care, we had loſt three grains of the eighteen we had gained. This done, we put the metal into another vial. But in regard the neck was ſhorter than that of the former, and could not, like it, be long held in ones hand; and becauſe alſo I was willing to ſee what intereſt the ſhaking of melted tin has in the quickneſs of the calcination, the glaſs, which had a ſtopple of paper put to it to keep out ſmoke and air, was held at ſome diſtance from the coals, only whilſt the tin was melting; and then was warily laid upon them, and kept there for two hours; at the end of which it was again taken off, and the metal weighed with the ſame counterpoiſe and balance, as formerly; and then it appeared to amount to eight ounces, twenty-four grains, and to have much more ſeparable calx than at the firſt time. Nor did I much wonder, that the weight ſhould be increaſed, in this laſt operation, but nine grains in two hours, and in the former, twice ſo many in half the time; ſince, during the two hours, the glaſs was kept in one poſture, whereas, in the firſt operation, it was almoſt perpetually ſhaken all the while it was kept in fuſion. And it it obſerved, that the agitation of melted minerals will much promote the effect of the fire upon them, and conduce to their calcination.]

EXPERIMENT III.

THOUGH theſe trials might well ſatisfy a perſon not very ſcrupulous, yet to convince even thoſe, that are ſo, I undertook, in ſpite of the difficulties of the attempt, to make the experiment in glaſſes hermetically ſealed, to prevent all ſuſpicion of any acceſſion of weight accruing to the metal, from any ſmoke or ſaline particles getting in at the mouth of the veſſel. And in proſecution of this deſign, I thought upon a way of ſo hermetically ſealing a retort, that it might be expoſed to a naked fire, without being either cracked, or burſt; an account of which trial was ſet down.

[Eight ounces of good tin, carefully weighed out, was hermetically ſealed up in a new ſmall retort, with a long neck, by which it was held in one's hand, and warily approached to a kindled charcoal fire, near which the metal was kept in fuſion, being alſo ever now and then ſhaken for almoſt half an hour, in which time it ſeemed to have acquired on the ſurface ſuch a dark colour, as argued a beginning of calcination, and it both emitted fumes, that played up and down, and alſo afforded two or three drops of liquor in the neck of the retort. The laborant being not able to hold the glaſs any longer, it was laid on quick coals, where the metal continued above a quarter of an hour longer in fuſion; but before the time was come, that I intended to ſuffer it to cool, in order to the removing it, it ſuddenly broke in a great multitude of pieces, and with a noiſe like the report of a gun; but (thanks be to God) it did no harm neither to me, nor others, that were very near it. In the neck we found ſome drops of a yellowiſh liquor, which a virtuoſo, that taſted it, affirmed to be of an odious, but peculiar ſapor; and as for the ſmell, I found it to be very ſtinking, and not unlike that of the diſtilled oil of fiſh.]

But though our firſt attempt of this kind had thus miſcarried, we were not thereby diſcouraged, but, in proſecution of the ſame deſign, made the enſuing trial.

EXPERIMENT IV.

[THE tin, which had been before (in the firſt, or ſome ſuch experiment) partly calcined in a glaſs, being melted again in a crucible, that it might be reduced to pieces ſmall enough to be put into another glaſs, was put again into the ſcales, and the ſurpluſage being laid aſide, that there might remain juſt eight ounces; theſe were put into a bolt-head of white glaſs, with a neck of about twenty inches long, which being hermetically ſealed (after the glaſs had been a while kept over the fire, leſt that ſhould break by the rarefaction of the air) the metal was kept in fuſion for an hour and a quarter, as (being hindered by a company of ſtrangers from being there myſelf) the laborant affirmed. Being unwilling to venture the glaſs any longer, it was taken from the fire, and when it was grown cold, the ſealed end was broken off: but before I would have the bottom cut out, I obſerved, that the upper ſurface of the metal was very darkly coloured, and not at all ſmooth, but much, and very oddly aſperated; and the lower part had between the bottom, and the lower part of the lump, a pretty deal of looſe, dark-coloured calx, though the neighbouring ſurface, and ſome places of the lump itſelf, looked by candle-light (it being then night) of a golden colour. The lump and calx together were weighed in the ſame ſcales carefully, and we found the weight to have increaſed twenty three grains, and better, though all the calx, we could eaſily ſeparate, being weighed by itſelf, amounted not to four ſcruples, or eighty grains.

For confirmation of this experiment, I ſhall ſubjoin another, wherein but a quarter of ſo much metal was employed, with ſuch ſucceſs, as the annexed memorial declares.

EXPERIMENT V.

[TWO ounces of filings of tin were carefully weighed, and put into a little retort, whoſe neck was afterwards drawn ſlenderly out into a very ſmall apex: then the glaſs was placed on kindled coals, which drove out fumes at the ſmall orifice of the neck for a pretty while. Afterwards the glaſs, being ſealed up at the apex, was kept in the fire above two hours; and then being taken off, was broken at the ſame apex; whereupon I heard the outward fire ruſh in, becauſe, when the retort was ſealed, the air within it was highly rarified. Then the body of the glaſs being broken, the tin was taken out, conſiſting of a lump, about which there appeared ſome grey calx, and ſome very ſmall globules, which ſeemed to have been filings melted into that form. The whole weighed two ounces, twelve grains, the latter part of which weight appeared to have been gained by the operation of the fire on the metal. In the neck of the retort, where it was joined to the body, there appeared a yellowiſh and clammy ſubſtance thinly ſpread, which ſmelt almoſt like the fœtid oil of tartar.]

EXPERIMENT VI.

TO vary the foregoing experiments by making trials on a mineral, that is held to be of a very metalline nature, but is not a true metal, nor will be brought to fuſion by ſo moderate a heat, as will ſuffice to melt tin, and yet has parts leſs fixed than tin, as being far more eaſily ſublimable, we thought fit to make the following experiment.

[We took an ounce of filings of zink, carefully weighed; and having as carefully put them into a round bolt-glaſs, we cauſed the neck to be drawn out very ſlender, and then ordered the laborant to keep it upon quick coals for the appointed time. Afterwards returning home, I called for the glaſs, which, he ſaid, he had kept four hours upon the coals; anſwering me alſo, that there did, for a great part of the time ſmoke appear to aſcend from the zink, and get out at the unſtopped apex. And in effect I obſerved, that the upper part of the glaſs was lined with flores or ſublimate of a darkiſh grey. The glaſs being dexterouſly cut aſunder, we took out, not only the filings of zink, ſome of which were melted into little globules, but the flores too; and yet weighing all theſe in the ſame ſcales we had uſed before, we found five grains and ſomewhat better wanting of an ounce. Which we the leſs wondered at, becauſe of the continuance of the lately mentioned exhalations emitted by the filed mineral.]

EXPERIMENT VII.

FOR more ample confirmation of the truth diſcovered by what I have been reciting about tin, I thought fit to try the like experiment upon another metal, which, though of ſomewhat more difficult fuſion than tin, I had reaſon to think might, if employed in a moderate quantity, and warily managed, be kept melted in glaſs without breaking it. And accordingly, having carefully weighed out four ounces of good lead, cut before-hand into pieces little enough for the orifice of the glaſs, I cauſed them to be put into a ſmall retort with a long neck, wherein was afterwards left but an orifice not much bigger than a pin's head: then leaving directions with the laborant what to do, becauſe I was my ſelf called abroad, at my return he brought me, together with the glaſs, this account; that he had kept it over and upon the coals two hours, or better, and then ſuppoſing the danger of breaking the glaſs was over, he had ſealed it up at the little orifice newly mentioned, and kept it on the coals two hours longer. Before the glaſs (which I found to be well ſealed) was broken, I perceived the pieces of lead to have been melted into a lump, whoſe ſurface was dark and rugged, and part of the metal to have been turned into a dark-coloured powder or calx: all this being taken out of the retort, was weighed in the ſame balance, whereon the lead appeared to have gained by the operation ſomewhat above thirteen grains.

EXPERIMENT VIII.

TO ſhew, that metals are not the only bodies, that are capable of receiving an increaſe of weight from the fire, I thought fit to make upon coral a trial, whereof my memorial gives me this account.

[LITTLE bits of good red coral, being hermetically ſealed up in a thin bubble of glaſs, after two drachms of them had been weighed out in a pair of nice ſcales, were warily kept at ſeveral times over and upon kindled coals, and at length being taken out for good and all, were found of a very dark colour, and to have gained in weight three grains and about a half.]

EXPERIMENT IX.

ONE experiment there is, which, though it might have come in more properly at another place, is not to be omitted in this, becauſe it may invite us to conſider, whether, in the foregoing experiments, excepting thoſe made on lead and tin in ſealed veſſels, there may not be more of the fire adherent to, or incorporated with the body expoſed to it, than one would conclude barely from the recited increments of their weight. For having taken very ſtrong freſh quick-lime, provided on purpoſe for choice experiments, and expoſed it, before the air had time to ſlake it, upon the cupel, to a ſtrong fire, where it was kept for two hours; I found, that it had increaſed in weight even ſomewhat beyond my expectation. For being ſeaſonably put into the balance, the lumps, that weighed, when expoſed, but two drachms, amounted to two drachms and twenty-nine grains; which makes this experiment a pregnant one to our purpoſe. For by this it appears, that notwithſtanding a body may for many hours, or even for ſome days, be expoſed to a very violent fire, yet it may be ſtill capable of admitting and retaining freſh corpuſcles; ſo that, though well made lime be uſually obſerved to be much lighter than the ſtones whereof it is made, yet this lightneſs does not neceſſarily prove, that, becauſe a burnt lime-ſtone has loſt much of its matter by the fire, it has therefore acquired no matter from the fire; but only infers, that it has loſt far more than it has got. And this may give ground to ſuſpect, that in moſt of the foregoing trials, the acceſſion of the fiery particles was greater (though in ſome more, in others leſs ſo,) than the balance diſcovered; ſince, for aught we know, divers of the leſs fixed particles of the expoſed body might be driven away by the vehemence of the heat; and conſequently the igneous corpuſcles, that faſtened themſelves to the remaining matter, might be numerous enough, not only to bring the acceſſion of weight, that was found by the ſcales, but to make amends for all the fugitive particles, that had been expelled by the violence of the fire. And ſince ſo fixed a body, as quick-lime, is capable of being wrought upon by the igneous effluvia, ſo as that they come to be, as it were, incorporated with it, it may, perchance, be worth conſidering, whether in other calcined, or incinerated bodies, the remaining calces, or aſhes, may not retain more than the bare impreſſion (unleſs that be ſtretched to mean ſome participation of a ſubſtance,) of the fire. Whether theſe particles, that adhere to, or are mingled with the ſtony ones of the lime, may have any thing to do in the heat and tumult, that it produced upon the ſlaking of lime, this is not a fit place to examine. And though by this experiment and thoſe made in ſealed retorts, which ſhew, that what is afforded by fire may in a corporeal way invade, adhere, and add weight to even fixed and ponderous bodies, there is a large field opened for the ſpeculative to apply this diſcovery to divers phænomena of nature and chemiſtry; yet I ſhall leave this ſubject unmeddled with in this place.

A DISCOVERY OF THE PERVIOUSNESS OF GLASS TO PONDERABLE PARTS of FLAME.

With ſome Reflections on it by way of COROLLARY.

THAT I might obviate ſome needleſs ſcruples, that may be entertained by ſuſpicious wits upon this circumſtance of our additional experiments, "That the glaſſes employed about them were "not expoſed to the action of mere flame, "but were held upon charcoals," (which to ſome may ſeem to contain but a groſſer kind of fire:) and that alſo I might, by diverſifying the way of trial, render ſuch experiments both more fit to afford corollaries, and more ſerviceable to my other purpoſes, I attempted to make it ſucceed with a body ſo thin and diſengaged from groſs matter, as mere flame is allowed to be, knowing, that by going cautiouſly with it to work, one might handle a retort without breaking it, in ſpite of a violent agitation of kindled matter.

EXPERIMENT I.

SUPPOSING then, that good common ſulphur, by reaſon of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fuel for my purpoſe, I provided a ſmall double veſſel ſo contrived, that the one ſhould contain as many coals, as was neceſſary to keep the ſulphur melted, and that the other, which was much ſmaller, and ſhaped like a pan, ſhould contain the brimſtone requiſite for our trial; and, laſtly, that theſe two ſhould be with a convenient lute ſo joined to one another, that all being cloſed at the top, ſave the orifice of the little pan, (the fire and ſmoke of the coals having their vent another way,) no fire ſhould come at the retort to be employed, but the flame of the burning brimſtone. Then two ounces of filings of tin being heedfully weighed out, and put into a glaſs-retort provided for ſuch trials, and made fit to be eaſily ſealed up at the neck, when the time ſhould be convenient, the ſulphur (which ought to be of the purer ſort) was kindled, and the glaſs by degrees expoſed to it; where it continued, as the laborant informed me, (the ſmell of brimſtone, peculiarly offenſive to me, forbidding me to be preſent,) near two hours before the metal melted; after which, he kept the retort near an hour and half more, with the metal melted in it. Then bringing it me to look upon, I perceived a pretty deal of darkiſh calx at the bottom, and partly too upon the ſurface of the far greater part of the metal, which now lay in one lump. The part of the retort, that had been ſealed, being broken off, we firſt took out the calx, and then the lump, and putting them into the ſcales, they had been formerly weighed in, found them to have made a very manifeſt acquiſt of weight, which, if both the laborant and I be not miſtaken, (for the paper, which ſhould inform us, is now miſſing) amounted to four grains and a half, gained by the recited operation. Afterwards, we being grown more expert in making ſuch trials, the experiment was repeated with the ſame quantity of filings of the ſame metal: at the end of the operation, (which in all laſted ſomewhat above three hours) having broken off the ſealed neck of the retort, we found, that a good proportion of dark coloured calx had been produced. This being weighed with the uncalcined part of the metal, the two ounces, we firſt put in, appeared to have acquired no leſs than eleven grains and a half (and ſomewhat better.)

SUCH ſuperſtructures, both for number and weight, may poſſibly in time be built on this and the like experiments, that I ſhall venture to obviate, even ſuch a ſcruple, as is like to be judged too ſceptical. But I remember, that, conſidering upon occaſion of ſome of the experiments formerly recited, that though it were very improbable, yet it did not appear impoſſible, that the increment of weight, acquired by bodies expoſed in glaſs-veſſels to the fire, might proceed, not from the corpuſcles of fire, but from the particles of the glaſs it ſelf, looſened by the power of ſo intenſe a heat, and forcibly driven into the incloſed body; I was content to take a couple of glaſſes, whereof one was ſhaped into a little retort, and having weighed them, and then having kept them for a conſiderable time upon kindled coals, and then weighed them again, I could gather little of certainty from the experiment, (the retort at one time ſeeming to have acquired above half a grain in the fire,) ſave that there was no likelihood at all, that ſo conſiderable an encreaſe of weight, as we divers times obtained in cloſe veſſels, ſhould proceed from the glaſs it ſelf, and not from the fire.

EXPERIMENT II.

BECAUSE it ſeems evident enough, that, whatever chemiſts tell us of the hypoſtatical ſulphur, common brimſtone is a body heterogeneous enough, having in it ſome parts of an oily or inflammable nature, and others acid, and very near of kin to the ſpirits of vitriol; I thought fit to vary our experiment, by making it with a liquor, that is generally reputed to be as homogeneous as chemiſts themſelves are wont to render any, I mean with a ſpirit of wine, or ſome ſuch liquor as will totally flame away without affording ſoot, or leaving any drop of phlegm behind it. In proſecution of this deſign, we carefully weighed out an ounce of filings of block-tin, and put them into a glaſs retort, fit for the purpoſe, whoſe neck was afterwards drawn out to a great ſlenderneſs; and we alſo provided a conveniently ſhaped metalline lamp, ſuch as that the flame of this ardent ſpirit might commodiouſly burn in it, and yet not melt or crack it; which lamp, though furniſhed with a cotton wick, afforded no ſoot, becauſe, as long as it was ſupplied with liquor enough, it remained unburnt. Theſe things being in readineſs, the retort was warily approached to the flame, and the metal was thereby in a ſhort time melted. After which, the glaſs being kept expoſed to the ſame flame for near two hours in all, the ſealed apex of the retort was broken off, and there appeared to have been produced a not inconſiderable quantity of calx, that lay looſe about the remaining part of the tin, which, upon its growing cold, was hardened into a lump. This, and the calx, being taken out of the retort with care, that no little fragment of glaſs ſhould at all impoſe upon us, was weighed in the ſame ſcales as formerly, and found to have gained four grains and a half, beſides the duſt, that ſtuck in the inſide of the retort, of which we reckoned enough to make about half a grain more; ſo that of ſo fine and pure a flame, as of this totally ardent ſpirit, enough to amount to five grains was arreſted, and in good meaſure fixed by its operation on the tin it had wrought upon.

EXPERIMENT III.

FOR confirmation of the former trial, wherein we had employed the *ſpiritus ardens* of ſugar, we made the like experiment with highly rectified ſpirit of wine, only ſubſtituting an ounce of lead inſtead of one of tin. The event, in ſhort, was this; that after the metal had been for two hours or better, kept in the flame, the ſealed neck of the retort being broken off, the external air ruſhed in with a noiſe, (which ſhewed the veſſel to have been very tight,) and we found pretty ſtore of the lead; for it was above ſeven ſcruples, turned into a greyiſh calx, which together with the reſt of the metal being weighed again, there was very near, if not full ſix grains of increaſe of weight acquired by the operation.

1. N. B. The lump of lead, that remained after the newly recited operation, being ſeparated from the calx, was weighed and cut in pieces, that it might be put into a freſh retort, wherein it was again expoſed to the flame of ſpirit of wine, that I might ſatisfy my ſelf, whether probably the whole body of the lead might not, by repeated operations, or (perhaps by one continued long enough) be reduced to calx. And though, after the retort (whoſe neck had been drawn out) had been kept in the flame for about two hours, it was, by the negligence of a foot-boy, unluckily broken, and ſome of the calx loſt; yet we made a ſhift to ſave about five grains of it, (whoſe colour was yellowiſh;) which was enough to make it likely, that, if we had had conveniency to purſue the operation to the utmoſt, the whole metal might have been calcined by the action of the flaming ſpirit.

2. N.B. And leſt you ſhould be induced by ſome chemical conceits to imagine, that the particles, that once belonged to flame, did make more than a coalition with thoſe of the lead, and by a perfect union were really tranſmuted into the metal whoſe weight they encreaſed; I ſhall add, that (according to a method elſewhere delivered) I examined the ſeven ſcruples of calx, mentioned to have been made in the third experiment, by weighing them in air and water, and thereby found, as I expected, that though the abſolute gravity of the metal had been encreaſed by the particles of flame, that ſtuck faſt to it, yet this aggregate of lead and extinguiſhed flame had loſt much of its ſpecifick gravity. For whereas lead is wont to be, to water of the ſame bulk, as about eleven and an half to one; the ſubtil calx of lead was to water of the ſame bulk, little, if at all, more than as nine to one.

These are not the only experiments I made of the operation of mere flame upon bodies incloſed in glaſſes; but theſe, I ſuppoſe, are ſufficient to allow me to comply with my preſent haſte, and yet make good the title prefixed to this paper. For whence can this encreaſe of abſolute weight (for I ſpeak not of ſpecifick gravity,) obſerved by us in the metals expoſed to the mere flame, be deduced, but from ſome ponderable parts of that flame? And how could thoſe parts invade thoſe of the metal incloſed in a glaſs, otherwiſe than by paſſing through the pores of that glaſs? But becauſe I judge it unphiloſophical, either to be more careful, that what one writes ſhould appear ſtrange, than be true; or to be forward to advance the repute of ſtrangeneſs, to the prejudice of the intereſt of truth, though it be perhaps but a remote one, or a collateral one; I ſhall deal ſo impartially, as to ſubjoin on this occaſion two or three ſhort intimations, that may prove both ſeaſonable for caution, in reference to the porouſneſs of glaſs, and give a hint or two in relation to other things.

I do not then, by the foregoing experiments, pretend to make out the poroſity of glaſs any farther, than is expreſſed in the title of this paper; namely, in reference to ſome of the ponderable parts of flame. For otherwiſe I am not at all of their mind, that think glaſs is eaſily penetrable, either, as many do, by chemical liquors; or, as ſome, by quick-ſilver; or, as others, at leaſt by our air; thoſe opinions not agreeing with the experiments I made purpoſely to examine them, as you may find in another paper.

AGAIN, if we compare the increaſe we obſerve to be made in the weight of the bodies, that we expoſe to the naked fire, and thoſe of the ſame or the like kinds, that we included in glaſſes, or ſo much as in crucibles; it may be worth conſidering, whether this difference in acquired weight may not give cauſe to ſuſpect, that the corpuſcles, whereof fire and flame conſiſts, are not all of the ſame ſize, and equally agitated, but that the interpoſed veſſel keeps out the groſſer particles like a kind of ſtrainer, though it gives paſſage to the minuteſt and moſt active.

I offer it alſo to conſideration, whether this perviouſneſs of glaſs, even to the minute particles, that pervade it, and their adheſion to the metal they work on, does neceſſarily imply pores conſtantly great enough to tranſmit ſuch corpuſcles: or, whether it may not be ſaid, that glaſs is generally of a cloſer texture, than when in our experiments the pores are opened by the vehement heat of the flame, that beats upon it, and in that ſtate, may let paſs corpuſcles too big to permeate glaſs in its ordinary ſtate; and, that this penetration is much aſſiſted by the vehement agitation of the igneous parts, which, by the rapidneſs of their motion, both force themſelves a paſſage through the narrow pores of the glaſs, and pierce deep enough into thoſe of the included body, to ſtick faſt there; (as hail-ſhot thrown with one's hand againſt a board will paſs off from it, but being ſhot out of a gun will pierce it, and lodge themſelves in it:) and I know a menſtruum, that does not work upon a certain metal, whilſt the liquor is cold, or but faintly heated; and yet by intending the heat would be made to turn it into a powder or calx, (for it does not not properly diſſolve it.)

PERHAPS it may not be amiſs to add on this occaſion, that though glaſs be generally acknowledged to have far ſmaller pores, than any other matter wont to be implied to make veſſels, that are to be expoſed to the fire; yet, till I be farther ſatisfied, I ſhall forbear both to determine, whether the rectitude, that ſome philoſophers ſuppoſe in the pores of glaſs, as it is a tranſparent body, or rather in their ranks or rows, may facilitate the previouſneſs we above obſerved in glaſs, and to conclude from the foregoing experiments, that ponderable parts of flame will be able, as well to paſs through the pores of metalline veſſels, as thoſe of glaſs. For though, with a ſilver veſſel, made merely of plate, without ſoder, I made two or three trials, (of which you may command an account) in order to the reſolving of theſe doubts; yet by an accident, which, though it were not a ſurprizing one, was unlucky enough to defeat my endeavours, I was kept, for want of fit accommodations, from bringing my intended trials to an iſſue.

AND now having endeavoured by the foreing advertiſements, to prevent the having unſafe conſequences drawn from our experiments; it remains, that I briefly point at three or four corollaries, that may more warily be deduced from them. To which, if I get time, I may ſubjoin a hint or two about further enquiries.

COROLLARY I.

Confirming this paradox, that flame may act as a menſtruum, and make coalitions with the bodies it works on.

THE experiments we have made and recited, of the permeating of flame (as to ſome of its parts) through glaſs-veſſels, and of its working on included metals, may much confirm the paradox I have elſewhere propoſed, that flame may be a menſtruum, and work on ſome bodies at the rate of being ſo; I mean, not only by making a notable comminution and diſſipation of the parts, but by a coalition of its own particles, with thoſe of the fretted body, and thereby permanently adding ſubſtance and weight to them. Nor is it repugnant to flame's being a menſtruum, that in our experiment, the lead and tin, expoſed to it, were but reduced to powder, and not diſſolved in the form of a liquor, and kept in that ſtate. For, beſides that the interpoſed glaſs hindered the igneous particles from getting through in plenty enough; I conſider, that it is not neceſſary, that all menſtruums ſhould be ſuch ſolvents, as the objection ſuppoſes. For whether it be (as I have ſometimes ſuſpected,) that menſtruums, that we think ſimple, may be compounded of very differing parts, whereof one may precipitate what is diſſolved by the other; or for ſome other cauſe, I have not now time to diſcuſs. Certain it is, that ſome menſtruums corrode metals and other bodies, without keeping diſſolved all, or perhaps, any conſiderable part; as may be ſeen, if you put tin in a certain quantity of aqua fortis, which will in a very ſhort time reduce it almoſt totally to a very white ſubſtance, which, when dry, is a kind of calx. And ſo by a due proportion of oil of vitriol, abſtracted from quickſilver by a ſtrong fire, we have divers times reduced the main body of the mercury into a white powder, whereof but an inconſiderable part would be diſſoluble in water. And ſuch a white calx I have had by the action of another fretting liquor on a body not metalline.

AND having thus cleared our paradox of the oppoſed difficulty, my haſt would immediately carry me on to the next corollary, were it not, that there is one phænomenon, belonging to this place, that deſerves to be taken notice of. For, whether it be, as ſeems probable, from the vehement agitation of the permeating particles of flame, that violently tear aſunder the metalline corpuſcles, or from the nature of the igneous menſtruum, (which being, as it were, percolated through glaſs itſelf, muſt be ſtrangely minute,) it is worth obſerving, how ſmall a proportion, in point of weight, of the additional adhering body, may ſerve to corrode a metal, in compariſon of the quantity of vulgar menſtruums, that is requiſite for that purpoſe. For, whereas we are obliged to employ, to the making the ſolution of crude lead, ſeveral times its weight of ſpirit of vinegar, and (though not ſo many times) even of aqua fortis, it was obſerved in our experiment, that, though the lead was encreaſed but ſix grains in weight, yet above ſix ſcore of it were fretted into powder, ſo that the corroſive body appeared to be about the twentieth part of the corroded.

COROLLARY II.

Propoſing a paradox about calcination, and calces.

ANOTHER conſequence, deducible from our diſcovery of the pervious̄neſs of glaſs to flame, may be this; that there is cauſe to queſtion the truth of what is generally taken for granted about calcination, and particularly of the notion, that not only others, but chemiſts themſelves, have entertained about the calces of metals, and minerals. For, whereas it is commonly ſuppoſed, that in calcination the greater part of the body is driven away, and only the earth, to which chemiſts add the fixed ſalt, remains behind; and whereas even mechanical philoſophers, (for two or three of them have taken notice of calcination,) are of opinion, that much is driven away by the violence of the fire, and the remaining parts, by being deprived of their more radical and fixed moiſture, are turned into dry and brittle particles: Whereas theſe notions, I ſay, are entertained about calcination, it ſeems, that they are not well framed, and do not univerſally hold; ſince, at leaſt, they are not applicable to the metals, our experiments were made on. For, it does not appear by our trials, that any proportion, worth regarding, of moiſt and fugitive parts, was expelled in the calcination; but it does appear very plainly, that by this operation the metals gained more weight than they loſt; ſo that the main body of the metal remained entire, and was far from being, either as a peripatetick would think, elementary earth, or a compound of earth and fixed ſalt, as chemiſts commonly ſuppoſe the calx of lead to be. From which very erroneous hypotheſis they are wont to infer the ſweet vitriol of lead, which they call *ſaccharum Saturni*, to be but the ſweet ſalt of it extracted only by the ſpirit of vinegar, which does indeed plentifully enough concur to compoſe it. Whence I conclude, that the calx of a metal even made as they ſpeak, *per ſe*, that is, by fire without additament, may be, at leaſt in ſome caſes, not the *caput mortuum*, or *terra damnata*, but a magiſtery of it. For, in the ſenſe of the moſt intelligible of the chemical writers, that is properly a magiſtery, wherein the principles are not ſeparated, but the bulk of the body being preſerved, it acquires a new and convenient form by the addition of the menſtruum, or ſolvent, employed about the preparation. And, not here to borrow any argument from my notes about particular qualities, you may gueſs, how true it is, that the greateſt part of the body, or all the radical moiſture, is expelled in calcination, which therefore turns the metal into an arid, unfuſible powder; by this, that I have ſeveral times, from calx of lead, reduced corporal lead. And I remember, that having taken, what I gueſſed to be but about a third, or fourth part, of the calx of lead, produced by the third experiment, I found by a trial purpoſely deviſed, that without any flux-powder, or any additament, but meerly by the application of the flame of highly rectified ſpirit of wine, there could, in a ſhort time be obtained a conſiderable proportion of malleable lead; whereof the part I had the curioſity to examine, was true malleable lead; ſo little was the arid powder, whence this was reduced, deprived by the foregoing calcination of the ſuppoſed radical moiſture requiſite to a metal. The conſideration of what may be drawn from this reduction, in reference to the doctrine of qualities, belongs not to this place.

COROLLARY III.

ONE uſe, among the reſt, we may make, by way of corollary, of the foregoing diſcovery, which is in reference to a controverſy warmly agitated among the corpuſcular philoſophers themſelves. For ſome of them, that follow the Epicurean or atomical hypotheſis, think, that when bodies are expoſed in cloſe veſſels to the fire, though the igneous corpuſcles do not ſtay with the bodies they invade, yet they really get through the pores of the interpoſed veſſels, and permeate the included bodies in their paſſage upwards; whereas others, eſpecially favourers of the carteſian doctrine, will not allow the atomiſts igneous corpuſcles, which they take to be but vehemently agitated particles of terreſtrial matter, to penetrate ſuch minute pores as thoſe of glaſs; but do ſuppoſe the operation of the fire to be performed by the vehement agitation made of the ſmall parts of the glaſs, and by them propagated to the included bodies, whoſe particles, by this violent commotion, are notably altered, and receive new textures, or other modifications.

But our experiments inform us, that, though neither of the two opinions ſeems fit to be deſpiſed, yet neither ſeems to have hit the very mark; though the Epicurean hypotheſis compriſe ſomewhat more of the truth, than the other. For, though it be not improbable, that the briſk agitation, communicated by the ſmall parts of the glaſs to thoſe of the body contained in it, may contribute much to the effect of the fire; and though, by the ſmall increment of weight, we found in our expoſed metal, it is very likely, that far the greater part of the flame was excluded by the cloſe texture of the glaſs; yet, on the other ſide, it is plain, that igneous particles were trajected through the glaſs, which agrees with the Epicureans; and they, on the other ſide, miſtook, in thinking, that they did but paſs through, and divide, and agitate the included bodies; to which, nevertheleſs, our experiments ſhew, that enough of them, to be manifeſtly ponderable, did permanently adhere.

Whether theſe igneous corpuſcles do ſtick, after the like manner, to the parts of meat, dreſſed by the help of the fire, and eſpecially roaſt-meat, which is more immediately expoſed to the action of the fire, may be a queſtion, which I ſhall now leave undiſcuſſed, becauſe I think it difficult to be determined, though, otherwiſe, it ſeems worthy to be conſidered, in regard it may concern men's health to know, whether the coction of meat be made by the fire, only as it is a very hot body, or whether it permanently communicates any thing of its ſubſtance to the meat expoſed to it: in which laſt caſe, it may be ſuſpected, that not only the degree, and manner of application of a fire, but the nature of its fuel, may be fit to be conſidered.

COROLLARY IV.

The experiments above recited give us this further information, that bodies very ſpirituous, fugitive, and minute, may, by being aſſociated with congruous particles, though of quite another nature, ſo change their former qualities, as to be arreſted, by a ſolid and ponderous body, to that degree, as not to be driven away from it by a fire intenſe enough to melt and calcine metals.

For the foregoing trials (taking in what I lately delivered of the leſſened ſpecifick gravity of calcined lead) ſeems plainly enough to diſcover, that even the agitated parts of flame, minute enough to paſs through the pores of glaſs itſelf, were, as it were, entangled among the metalline particles of tin and lead, and thereby brought to be fixed enough to endure the heat, that kept thoſe metals in fuſion, and little by little reduced them into calces: which is a phænomenon, that one would not eaſily look for, eſpecially conſidering how ſimple a texture, that of lead or tin may be ſuppoſed to be, in compariſon of the more elaborate ſtructures of very many other bodies. And this phænomenon, which ſhews us, what light and fugitive particles of matter may permanently concur to the compoſition of bodies ponderous and fixed enough, may perchance afford uſeful hints to the ſpeculative; eſpecially if this ſtrict combination of ſpirituous and fugitive ſubſtance with ſuch, as being groſs or unwieldy, are leſs fit, than organized matter, to entangle or detain them, be applied, (as it may be with advantage) to thoſe aggregates of ſpirituous corpuſcles, and organical parts, that make up the bodies of plants and animals. And this hint may ſuggeſt a main inference to be drawn from the operations of the ſunbeams on appropriated ſubjects, ſuppoſing it to prove like that of flame on tin and lead.

Exp. III. N. B. 2.

And now having diſpatched our corollaries, we might here enquire, whether all the particles of fire and flame, that are ſubtil, and agitated enough to penetrate glaſs, and faſten themſelves to included bodies, be reduced by ignition to the ſame nature, or elſe retain ſomewhat of their proper qualities? which enquiry I have ſome cauſe not to think ſo undeterminable, as at firſt bluſh it may appear. For one of the ways, that may be propoſed for this examen, is already intimated at the cloſe of the third experiment; which ſhews, that we may compare the ſpecifick gravity of the calces of the ſame metal, made in glaſſes by the operation of flames, whoſe fuels are of very differing natures. And I ſaid, one of the ways, becauſe it is not the only way I could name, and have partly tried. But though I might ſay more concerning expedients of this kind, and could perhaps propound other enquiries, that may reaſonably enough be grounded upon the hitherto recited phænomena, (and thoſe of ſome other like trials) yet I muſt not unſeaſonably forget, that the purſuit of ſuch diſquiſitions would lead me much farther, than I have now the leiſure to follow it.

A
LETTER
CONCERNING
AMBERGREASE,
And its being
A VEGETABLE PRODUCTION.

Firſt publiſhed in the Philosophical Transactions, Nº. XCVII. p. 6113, for *September* 13, 1673.

SIR,

SOME occaſions calling me this afternoon up to *London*, I met there with a very intelligent gentleman, who was ready to go out of it; but before he did ſo, he willingly ſpared me ſome time to diſcourſe with him about ſome of the affairs of our Eaſt-Indian Company, of which he was very lately deputy-governour; and, his year being expired, is ſtill one of the chief of the court of committees, which a foreigner would call directors, that manage all the affairs of that conſiderable ſociety. And, among other things, talking with him about ſome contents of a journal lately taken in a Dutch Eaſt-Indian prize, I learned from him, that he, who underſtands that language very well, is now peruſing that manuſcript, and, among many other things recorded there, that concern the oeconomical, and political affairs of the ſaid Dutch Company, he met with one phyſical obſervation, which he thought ſo rare, that remembring the curioſity I had expreſſed for ſuch things, he put it into Engliſh, and tranſcribed it for me, and immediately drawing it out of his pocket, he preſented me the ſhort paper, whereof I now ſhew you the copy: upon peruſal of which, you will very eaſily believe, that not only his civility obliged me, but the information it brought me, ſurprized me too. For the ſeveral trials, and obſervations of my own, about ambergreaſe, have long kept me from acquieſcing either in the vulgar opinions, or thoſe of ſome learned men concerning it; yet I confeſs, my experiments did much leſs diſcover what it is, than this paper has done, in caſe we may ſafely and entirely give credit to its information, and that it reach to all kinds of ambergreaſe. And, probably, you will be invited to look on this account, though not as complete, yet, as very ſincere, and, on that ſcore, credible, if you conſider, that this was not written by a philoſopher, to broach a paradox, or ſerve an hypotheſis, but by a merchant, or factor, for his ſuperiors, to give them an account of a matter of fact; and that this paſſage is extant in an authentick journal, wherein the affairs of the company were, by publick order, from time to time regiſtered, at their chief colony, *Batavia*. And it appears by the paper itſelf, that the relation was not looked upon as a doubtful thing, but as a thing, from which a practical way may be deduced to make this diſcovery eaſily lucriferous to the Dutch Company. And I could heartily wiſh, that in thoſe countries, that are addicted to long navigations, more notice, than is uſual, were taken and given of the natural rarities, that occur to merchants and ſeamen. On which occaſion, I remember, when I had, in compliance with my curioſity, put myſelf into our Eaſt-Indian Company, and had, by their civility to me, been choſen of their committee, as long as my health allowed me to continue ſo, I had the opportunity, in ſome regiſter-books of merchants, Engliſh and Dutch, to obſerve ſome things, which would eaſily juſtify this wiſh of mine, if my haſte, and their intereſt, would permit me to acquaint others with them. But to return to our account of ambergreaſe, I think you will eaſily believe, that, if I had received it not by a paper, but immediately from the writer, I ſhould, by propoſing divers queſtions, have been enabled to give you a much more ſatisfactory account, than this ſhort one contains. But the obliging perſon, that gave it me, being juſt going out of town, I could not civilly ſtay him to receive my queries about it; which though (God permitting) I may propoſe, ere long, if I can light on him again, yet I fear he has given me, in theſe few lines, all that he found about this matter. However, this relation, as ſhort as it is, being about the nature of a drug ſo precious, and ſo little known, will not, I hope, be unwelcome to the curious; to whom none is ſo like to convey it ſo ſoon, and ſo well, as Mr. *O.*; whoſe forwardneſs to oblige others by his various communications, challenges returns of the like nature from others, and particularly from his affectionate humble ſervant.

Follows the Extract itſelf out of a Dutch journal, belonging to the Dutch Eaſt-Indian Company.

“ AMBERGREASE is not the ſcum, or excrement of the whale, &c. but iſſues out of the root of a tree, which tree, how far ſoever it ſtands on the land, always ſhoots forth its roots towards the ſea, ſeeking the warmth of it, thereby to deliver the fatteſt gum, that comes out of it: which tree, otherwiſe, by its copious fatneſs, might be burned and deſtroyed. Wherever that fat gum is ſhot into the ſea, it is ſo tough, that it is not eaſily broken from the root, unleſs its own weight, and the working of the warm ſea doth it, and ſo it floats on the ſea.

“ THERE was found, by a ſoldier, ⅞ of a pound, and by the chief, two pieces weighing five pounds. If you plant the trees, where the ſtream ſets to the ſhore, then the ſtream will caſt it up to great advantage.

“ *March* 1, 1672, in *Batavia*: journal advice from———.”

TRACTS,

CONSISTING OF

OBSERVATIONS about the SALTNESS of the SEA.

An Account of a STATICAL HYGROSCOPE, and its USES.

TOGETHER WITH

An APPENDIX about the FORCE of the AIR'S MOISTURE.

A FRAGMENT about the NATURAL and PRETERNATURAL STATE of BODIES.

To all which is premiſed,

A SCEPTICAL DIALOGUE about the POSITIVE or PRIVATIVE NATURE of COLD.

WITH

Some EXPERIMENTS of Mr. *BOYLE*'s referred to in that Diſcourſe.

OF THE POSITIVE OR PRIVATIVE NATURE OF COLD.

A SCEPTICAL DIALOGUE between CARNEADES, THEMISTIUS, ELEUTHERIUS, PHILOPONUS.

SECTION I.

Eleuth. MAY one be allowed to aſk *Carneades*, what book it is he is reading with ſo much attention?

Carn. The queſtion, *Eleutherius*, is very allowable, and as eaſily anſwered, by ſaying, that what I was reading, is our friend Mr. *Boyle*'s newly publiſhed Hiſtory of Cold.

Them. YOUR readineſs, *Carneades*, to anſwer, encourages me alſo to aſk you a queſtion; which ſhall not be, as probably you expect it ſhould, how you like this new piece? for I know you would be too kind to the author, not to tell me; that he has detected ſome old errors, and made diſcovery of ſome new truths: but my queſtion ſhall be about what is my wonder, as well as that of divers others, who think it ſtrange, that a writer, that has de-

delivered ſo many effects and other phænomena of cold, ſhould omit to tell us ſo much, as whether he aſſerts it to be a poſitive quality, or a bare privation of heat; as, ſince *Cardan* (in his treatiſe *De Subtilitate*) ſome other learned men, and eſpecially *Carteſius*, have maintained.

Carn. You will not wonder, if a perſon, that you look upon, and I confeſs not injuriouſly, as a friend to Mr. *Boyle*, tell you, that this author, by the many hiſtories he has preſented us, and by his not ſeeming to dare to determine the controverſy you have mentioned, ſhews, that he was more ſolicitous to leſſen his ignorance, than to pretend to knowledge: and upon the obſervation I have made of his humour in general, I preſume one principal reaſon of his ſilence may be, that he has not yet compleated the trials he had deſigned about cold; and thinks, that in abſtruſe ſubjects, ſuch as this is, it is not ſo convenient to deliver a poſitive opinion of the nature of it at the beginning, as to reſerve it for the latter end, after the hiſtory of the phænomena; when the nature of the thing enquired into may, as it were, ſpontaneouſly reſult from the conſiderations ſuggeſted by the precedent matters of fact ſurveyed together.

Eleuth. If ſuch a warineſs were indeed the motive of your friend's ſilence, I ſhall eaſily excuſe it; and perhaps think too, that the like would not miſ-become naturaliſts on many other occaſions. And yet I do not diſlike *Themiſtius*'s queſtion; for it is one thing to venture upon declaring the adæquate nature of cold, and another to determine, whether it be a poſitive, or a privative quality? the latter attempt importing a much leſs venture than the former.

Carn. I will not pretend to know the very reaſons, that induced the author ſilently to paſs by this controverſy; but having been once preſent, when he had occaſion to diſcourſe of it, I then conjectured, that among his experiments of cold, that are not yet publiſhed, there may be ſome uncommon ones, that may have ſuggeſted to him ſcruples, which obliged him to forbear declaring himſelf, till he had cleared them, which thoſe, that are unacquainted with ſuch trials, may probably have never thought of.

Them. If what you call a controverſy, were indeed worthy of that name, I ſhould not unwillingly allow of your friend's ſilence; but the opinion broached by *Cardan*, and adopted by Mr. *Des Cartes* and others, ſeems to me ſo devoid, not only of reaſon, but of all appearance of it, that methinks one, that has delivered ſuch conſiderable effects of cold, as Mr. *Boyle* has done, may well aſcribe to their cauſe, at leaſt, a poſitive nature; and, without at all being guilty of boldneſs, reject an opinion, that is not only barely an error, but an extravagance, and perhaps a plain abſurdity.

Carn. Poſſibly the gentleman we are ſpeaking of, may be wary and ſceptical enough to reckon among difficult things, not only the declaring the adæquate nature of cold, and the manner of its operations; but the demonſtrating, whether it be a poſitive quality or not. And though I will not take upon me to know his thoughts about that ſubject, which, perhaps, are grounded upon ſome of his peculiar experiments and notions; yet, for diſcourſe ſake, I am content to debate with *Themiſtius*, Whether or no the opinion, he ſo ſeverely cenſures, be not only erroneous, as, for aught appears, Mr. *Boyle* himſelf may be found to have thought it; but alſo, as *Themiſtius* would have it, abſurd.

Them. I readily accept of your offer; for it cannot be an unpleaſant entertainment to obſerve the arts, whereby one, that I know will not ſpeak impertinently, will endeavour to make reaſon elude the cleareſt teſtimonies of ſenſe. And though I might preſs you with the concurrent authority of *Ariſtotle*, and all the philoſophers, that have lived between his time and thoſe of that extravagant fellow *Cardan*; yet I ſhall rather employ, to convince you, the authority and reaſons of a grand leader among your new philoſophers, who being a great broacher of paradoxes, and having upon that ſcore written books expreſly againſt *Ariſtotle*, was not like to have ſided with him, unleſs the evidence of truth had, as it were, neceſſitated him to do ſo.

Carn. I preſume, you mean the learned and ſubtle *Gaſſendus*, whom I am glad you have pitched upon for your cauſe's champion, not only becauſe, in defending the common opinion, he waves the common practice of troubling his readers with a multitude of authorities, which to me, in ſuch a caſe as this, would ſignify very little, and betakes himſelf to arguments; but becauſe, being ſo modern and judicious a writer, we may well ſuppoſe him to have ſummed up and improved what can be ſaid in behalf of the cauſe he maintains. Upon which account, I ſhall be excuſed from anſwering impertinent objections againſt the opinion I defend, and from the trouble of ranging about, among other authors, for more weighty arguments than thoſe, which the diſproving of his will ſhew to be unſatisfactory.

Them. I am glad you named the author I meant, *Carneades*, for I apprehended you had not met with what he ſays upon this ſubject; becauſe I could ſcarce imagine, that an intelligent perſon, after having read his arguments, will doubt of a truth he hath ſo clearly evinced by them. But ſince I perceive you have ſeen what he has written, I ſhall, without farther preamble, propoſe his reaſons to you, though not in the very ſame order, wherein he has couched them.

Eleuth. But before you begin them, give me leave to aſk *Carneades* a ſhort queſtion, whoſe anſwer will, I ſuppoſe, conduce, if not be neceſſary, to the clearing of the ſtate of the controverſy betwixt you. For it is one thing to deny belief to the received opinion, that cold is a poſitive quality, and another thing to aſſert, that it is but a privation of heat; ſince, if *Carneades* does undertake the latter of theſe two, he muſt bring poſitive arguments to prove cold to be but a negative thing. Whereas, if he content himſelf to play a doubting part, it may ſuffice him, being in effect but a defendant,

dant, to ſhew, that the proofs brought to conclude cold to be a poſitive quality are not cogent.

Carn. I acknowledge your queſtion, *Eleutherius*, to be pertinent, and not unſeaſonable. And, I preſume, you will not be ſurprized, that a perſon accuſed of ſcepticiſm anſwers it by declaring, that he undertakes not to demonſtrate, that cold muſt be a privative or negative quality, and thinks it ſufficient for his turn, to ſhew, that the arguments brought to evince it to be a poſitive one, are not concluding. And, ſince you have already diverted *Themiſtius* from beginning ſo ſoon as he intended, it will not be amiſs, that I continue that ſuſpenſion a little longer, to prevent, what I know we both hate, verbal controverſies; which yet may very eaſily ſpring from undetermined acceptions of words, as ambiguous as I have obſerved heat (of which I now make cold but a privation) to be.

We may therefore conſider, that the word heat being made uſe of to ſignify, as well the operations of that quality upon other bodies (as when the heat of the fire makes water boil, or that of the ſun melts wax, and hardens clay) as its operations upon the ſenſe of man, (as when a moderate degree of heat is ſaid to cauſe pleaſure, and an exceſſive one to produce pain;) this term, I ſay, as Mr. *Boyle* alſo has ſomewhere noted, may be employed ſometimes in a more abſolute and indefinite ſenſe, and ſometimes in a more confined and reſpective ſenſe: in the latter of which, it is eſtimated by its relation to the organs of feeling of thoſe men, that judge of it. Upon which account, men are wont to eſteem no body hot, but ſuch an one, the agitation of whoſe ſmall parts is briſk enough to encreaſe, or ſurpaſs, that of the particles of the organ, that touches it: for, if that motion be more languid in the object, than in the ſentient, the body is reputed cold; as may appear by this, that if the ſame perſon put one of his hands, when it is hot, and the other when it is cold, into luke-warm water, that liquor will feel cold to the warm hand, and warm to the cold.

Eleuth. So, that according to this doctrine, methinks, one may, for brevity ſake, conveniently enough apply to your two-fold notion of heat, thoſe expreſſions, which ſome ſchoolman employ about certain qualities, of any of which they ſay, that it may be either materially or formally conſidered. And by analogy to their doctrine, ſince heat is a tactile quality, and, as ſuch, imports primarily a relation to the organ of touching, that relation, with what depends upon it, may paſs for that, which is the *formale*, in the quality, called heat; and its effects and operations upon other bodies, may ſupply us with a notion of heat, materially taken.

Carn. I do not always quarrel, *Elutherius*, with terms borrowed from the ſchools, if they be as much more ſhort and expreſſive than others, as they are more unuſual, or even barbarous. But there is another diſtinction of heat, partly grounded upon that already propoſed, which, becauſe it may be of uſe in our future diſcourſe, will not be unfit to be here intimated. For we may conſider, that though, for the moſt part, a hot body is taken in the vulgar ſenſe, for that, wherein the degree of heat is ſenſible to our organs of feeling; yet, in a looſer ſenſe, and which, for diſtinction ſake, we may call philoſophical, becauſe concluded by reaſon, though not perceived by ſenſe, a body may be conceived not to be deſtitute of heat, even when the degree of that quality is not great enough to be felt by the touch; provided it can produce, in ſome degree, thoſe other operations, which, when more intenſe, are acknowledged to proceed from manifeſt heat. For elucidation of which, we may alledge, that in very froſty, and yet clear weather, the ſun may be judged to warm the air, when it melts ſnow, and thaws ice; though, perhaps, many men, eſpecially of tender conſtitutions, feel in their fingers and toes much ſtiffneſs, and more pain, upon the account of cold. To this I may add the common obſervation, if you grant the truth of it, that ſnow melts much ſooner upon land newly turned up by the plow, than, *cæteris paribus*, in the neighbouring ground; which argues a warmth in that newly expoſed earth; though, according to the touch, it would queſtionleſs appear cold. But we may be furniſhed with a clearer, and more pregnant inſtance, by but recalling to mind, what was juſt now mentioned of the warmth of tepid water, which was not to be felt by a hot hand, but produced there, a contrary ſenſation of cold. Which inſtance I therefore ſcruple not to repeat, becauſe it affords an experiment in favour of that premiſed diſtinction, which, I think, may alſo have this ground in reaſon, that a conſiderable heat is often requiſite to be ſenſible to our hands, &c. which are continually irrigated with the circulating blood, that comes very warm out of the heart, and enlivened by animal ſpirits, plentifully ſupplied from the brain.

If *Eleutherius* thinks fit to accommodate this diſtinction in the vulgar, and in the philoſophical ſenſe to his heat, formally and materially taken, I leave him to his liberty. And I ſhall alſo leave it to you both, Gentlemen, to accommodate to cold, *mutatis mutandis*, as they ſpeak, what has been ſaid about the diſtinctions of heat; becauſe, I fear, *Themiſtius* thinks himſelf to have been too long detained already from propoſing his arguments, which he may now begin to do as ſoon as he pleaſes.

SECTION II.

Them. I WILL then, with your permiſſion, begin with that argument of *Gaſſendus*, which I am able to give you in his own words; becauſe, upon the occaſion of Mr. *Boyle*'s book, I made a tranſcript of what he ſays, to evince the poſitive nature of cold; and having the tranſcript yet about me, it is eaſy for me to tell you, that it is this: [*] *Ii ſunt frigoris*

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[*] Vid. Gaſſend. Phyſicam. Sect. I. Lib. VI. cap. 6.

frigoris effectus, quales habere privatio, quæ actionis eſt incapax, non poteſt.

THIS argument, though he begins not with it, I chuſe to make the firſt, becauſe I think it of ſuch weight, that, though it were the only one he could alledge, it would ſerve, his turn and mine, ſince it is drawn from the effects of cold, which, though he mentions them but in few and general words, experience ſhews to be both ſo manifold and ſo conſiderable, that if *Carneades* employ an hundred times as much time to anſwer the argument they afford, as I have done to recite it, he will, I think, do no more than would be neceſſary, and perhaps not enough to be ſufficient. For cold affects the organs of feeling, and ſometimes cauſes great pain in them, condenſes air and water, and breaks bottles, that are too well ſtopped, congregates both homogeneous and heterogeneous things, increaſes hunger, checks fermentation in liquors, produces heat by antiperiſtaſis, in deep cellars, mines, &c. and yet freezes men and beaſts to death, diſmantles whole woods and foreſts of their leaves, and does (I know not how many) other feats; among which it is not the leaſt admirable, though one of the moſt common, that it turns the fluid and yielding waters of rivers and lakes, and ſometimes of part of the ſea itſelf, not too far from the ſhore, into firm and ſolid ice, which is often in northern climates ſtrong enough, not only to be travelled upon by merchants with their carriages, but to be fought upon by whole armies with their trains of artillery. From which, and other inſtances, it is manifeſt, that effects ſo numerous and great cannot proceed from a mere privation, or any negative thing, but require a conſiderable, and therefore ſure a poſitive quality to produce them.

Carn. THIS objection, *Themiſtius*, is, I confeſs, a conſiderable one, and of more weight than any of the reſt, if not than all of them put together: but, as I think it very worthy to be anſwered, ſo I think it very poſſible to be well anſwered; and to give you my reaſons for my ſo thinking, I ſhall diſtinctly conſider in the argument the two particulars, which it ſeems to conſiſt of.

AND firſt we are told, that if cold be but a privation, it cannot be the object of ſenſe. To clear this difficulty, which, I know, you will think it very hard, if at all poſſible to do, I muſt beg your leave to obſerve ſomething about ſenſation in general; not as deſigning an intire and ſolemn diſcourſe of that ſubject, but becauſe the particular remark I am about to make, is neceſſary to the ſolution of our preſent difficulty. I obſerve then, that that, which, at leaſt in ſuch caſes as we are ſpeaking of, produces in the mind thoſe perceptions, which we call ſenſations of outward objects, is the local motion, cauſed by means of their action upon the outward organs in ſome internal part of the brain, to which the nerves belonging to thoſe organs correſpond; and the diverſity of ſenſations may be referred to the differing modifications of thoſe internal motions of the brain, either according to their greater or leſſer celerity, or other circumſtances, as our friend Mr. *Boyle* has ſomewhere exemplified in the variety of ſounds; whereof ſome are grave, ſome ſharp, ſome harmonious and pleaſant, ſome jarring and offenſive; and yet all this ſtrange variety proceeds from the variations of thoſe ſtrokes or impulſes, which the air, put into motion by ſonorous bodies, gives to the ear.

TO this it will be conſonant, that as the air, or rather the mind by the intervention of the air, is differingly affected by a very grave ſound, and a very acute one; though the former proceed from the want of that celerity of motion in the undulating air, which is to be found in the latter; which ſlowneſs, or imminution of motion, does, as ſuch, participate of, or approach to, the nature of reſt: ſo in the ſenſory of feeling there may, upon the contact of a cold body, be produced a very differing perception from that, which is cauſed by the contact of a hot body; and this, though the thing perceived, and by us called coldneſs, conſiſts but in a leſſer agitation of the parts of the cold body, than of thoſe of the hot body, in reſpect of our hands or other organs of feeling.

AND this leads me, for the farther clearing of this matter, to repreſent to you, that ſince it is manifeſt, that bodies in motion are wont to communicate of their motion to thoſe more ſlow bodies they happen to act upon, and to loſe of their own motion by this communicating of it: ſince this, I ſay, is ſo, if, for inſtance, a man take a piece of ice in his hand, the agitation of the particles of the ſenſory will, in good part, be communicated to the corpuſcles of the ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the hand loſing of the motion they thus part with to the ice, there needs nothing elſe to leſſen the agitation they had before. And there needs no more than this ſlackening, or decrement of agitation, to occaſion in the mind ſuch a new and differing perception, as men have tacitly agreed to refer to coldneſs.

Eleuth. IT ſeems by this diſcourſe, *Carneades*, that you think, that ſenſation is properly and ultimately made in, or by the mind, or diſcerning faculty; which, from the differing motions of the internal parts of the brain, is excited and determined to differing perceptions; to ſome of which men have given the names of heat, cold, or other qualities. So that, according to you, if a conſiderable change or variation be made in the moſt ordinary, or in the former motion or modification of motion of the parts of a ſenſory, and conſequently of the parts, that anſwer them in the brain, new ſenſations will be produced, whatever the cauſe of this alteration be, whether privative or poſitive.

Carn. YOU do not miſapprehend my thoughts, *Eleutherius*, and what you ſay gives me a riſe to illuſtrate this matter yet a little farther, by obſerving, that the ſenſories may be ſo accuſtomed to be affected after a certain man-by thoſe external objects, whoſe operation on them is very familiar, or perhaps almoſt conſtant, that the privation, or the bare imminution of the wonted operation leaves the parts of the ſenſory, for want of it, in a different diſpoſition from what they formerly were in; which change in the ſenſory, if it be not too ſmall, will be attended by a perception of it in the mind. To declare and confirm this by an example, we may conſider, that though darkneſs be confeſſedly a privation of light, and the degrees of it gradual imminutions of light; yet the eye, that is, the perceptive faculty, by the intervention of the eye, may well enough be ſaid to perceive both light and darkneſs, that is, both a poſitive thing, and the privation of it. And it is obvious, that the motion of a ſhadow, which is a gradual privation of light, is plainly, and without difficulty, diſcoverable by the eye; of which the reaſon may be eaſily deduced from what I have been lately ſaying. And to ſhew you, that there is on theſe occaſions ſuch a change made in the organs of ſeeing, as is viſible even to by-ſtanders, I ſhall need but to appeal to the experiment of making in the day time a boy or girl look towards an enlightened window, and then towards an obſcure part of the room; for when the latter comes to be done, you will plainly perceive, that for want of ſuch a degree of light, as was wont to come in at the pupil, and ſtraiten a little that perforation of the uvea; that round circular hole, or, as you know they call it, apple of the eye, will grow very manifeſtly larger than it was before, and than it will appear again, if the eye be expoſed to a leſs ſhaded light.

This obſervation may be ſeconded by what happens to a man, when coming out of the ſun-ſhine, where the ſun-beams much contract his pupil to ſhut out an exceſſive light, that would be offenſive to the organ, he comes preſently into a dark room, where he muſt continue ſome time before he can ſee others, as well as he is ſeen by them, whoſe pupils have had time to be ſo enlarged, as in that darker place to let in light enough to make objects viſible to their eyes, which are not ſo to his, whoſe pupils are yet contracted by the light they were but juſt before expoſed to. To this I might add divers other phænomena, explicable upon the ſame grounds; but I ſhall rather chuſe to relate to you an uncommon accident, which happening to eyes ſomewhat unuſually diſpoſed, does more remarkably diſcover, what alteration darkneſs, or a privation of light, may have upon thoſe organs. I know a very learned man, who is no leſs ſtudious of mathematicks, and other real parts of knowledge, than ſkilled in thoſe, which are taught of the ſchools: this virtuoſo, who ſeemed to me to have ſomething peculiar in his eyes, confeſſed, and complained to me, that if he come, though but out of a moderate light of the open air, into a room, that is any thing dark, he does not only feel ſuch an alteration, as other men are wont to do on the like occaſion; but is ſo powerfully affected by it, that he thinks, he ſees flaſhes of fire before his eyes, and feels a troubleſome diſcompoſure in thoſe parts, that ſometimes laſts an hour or two together, if he ſo long continue there.

Eleuth. I know not, *Carneades*, whether after this you will think it any great confirmation of your opinion, that *Ariſtotle* has ſomewhere this ſaying, that, *Oculus cognoſcit lucem & tenebras.*

Carn. I thank you, *Eleutherius*, for ſo pertinent an allegation, though not for my own ſake, yet for theirs, that will more eaſily receive a truth upon the teſtimony of *Ariſtotle*, than that of nature. And now, I hope, that *Themiſtius* will conſent, that, diſmiſſing the argument hitherto examined, we proceed to the next.

SECTION III.

Them. SINCE you will have it ſo, I ſhall comply at preſent, and the rather, becauſe, not only I foreſee there will be occaſion to ſpeak of it again, but becauſe you experimental philoſophers, that are wont ſo much to cry up the informations you think you receive from ſenſe, ſometimes, in ſpite of contrary dictates of reaſon, will, I hope, be prevailed with by the argument I am about to propoſe, which is ſo manifeſtly grounded upon ſenſe, that without denying, that we do feel what we feel, we cannot deny cold to be a poſitive quality. For thus *Gaſſendus* moſt convincingly argues; *Cùm per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea ſentitur, non poteſt dici mera privatio, aliúdque prorſus eſſe apparet ſentiri aquam frigidam, & ſentiri non calidam. Et fac eandem aquam gelari, ſentietur haud dubiè frigidior: an dices hoc eſſe nihil aliud quàm minùs calidam ſentiri? Atqui calida jam antea non erat: quomodo ergo potuit minùs calida effici?*

Carn. I will not ſay, *Themiſtius*, his argument is not ſpecious, but you, perhaps, or at leaſt *Eleutherius*, will not affirm it to be more than ſpecious, if you pleaſe to conſider, with me, two or three things, that I have to ſuggeſt about it.

And firſt, to ſhew *Themiſtius*, that, whatever he was juſt now intimating, experimental philoſophers do not prefer the immediate impreſſions made on the ſenſes to the dictates of reaſon, though they think the teſtimony of the ſenſes, however ſometimes fallacious, much more informing than the dictates of *Ariſtotle*, which are oftentimes, and that groundleſly, repugnant to them; I will repreſent to you, that the organs of ſenſe, conſidered preciſely as ſuch, do only receive impreſſions from outward objects, but not perceive, what is the cauſe and manner of theſe impreſſions, the perception, properly ſo called, of cauſes belonging to a ſuperior faculty, whoſe property it is to judge, whence the alterations made in the ſenſories do proceed, as may eaſily be proved, if I had time and need to do ſo, by many inſtances, wherein the ſenſes do, to ſpeak in the uſual phraſe, miſ-inform, and, as far as in them lies, delude us, and therefore muſt be rectified by reaſon. As when the eye repreſents a ſtrait ſtick, that has part of it under water, as if it were crooked; and two fingers, laid croſs over one another, repreſent us a ſingle bullet, or a button, rolled between them, as if there were a couple: ſo that it is very poſſible (for I forbear ſaying it is true, having not yet proved it) that though the ſenſory be very manifeſtly, and vehemently, affected upon the contact of cold water, or other cold bodies, yet the cauſe of that impreſſion, or affection, is, and may be judged, and determined by reaſon to be, other than that, which the ſenſe may to an inconſiderate perſon ſuggeſt. As when a child, or one, that never heard of the thing before, firſt ſees a ſtick, whereof one part is in the air, and the other under water, he will preſently, but erroneouſly, conclude that phænomenon to be cauſed by the ſticks being crooked, or broken.

NEXT we may conſider, that ſenſations may, in divers caſes, be made, as well from alterations, that may happen in the internal parts of the body, as from thoſe, that are manifeſtly produced in the external organ, by external objects and agents; as may appear by hunger, thirſt, the titillation of ſome parts of the body, barely upon venereal thoughts, and (which belongs directly to our preſent argument) the great coldneſs, that we have known hyſterical women complain of in their heads and backs, and the great, and troubleſome degree of cold, which we every day obſerve, upon the firſt invaſion of the fits of agues, eſpecially quartans; which troubleſome ſymptoms, that ſometimes laſt for ſeveral hours, are therefore commonly called the cold fits.

AND now it would be ſeaſonable for me to call upon you to remember (and add to what I have now ſaid) that, which, at the beginning of our conference, I took notice to you of, about ſenſation in general; if I did not preſume, that thoſe things are yet freſh enough in your memory, to allow me to proceed directly to anſwer the objection, which I ſhall do, though not like a ſchool-man, yet like a naturaliſt, by giving an account of the propoſed phænomenon, without having recourſe to that hypotheſis, which it is urged to evince.

I obſerve then, that though, in the reſpective ſenſe above-mentioned, water, wherein the objection ſuppoſes the hand to be plunged, be cold, in regard its parts are leſs agitated, than the ſpirits and blood harboured in the hand; yet, in a philoſophical ſenſe, it is not quite deſtitute of heat, ſince it is yet water, not ice, and would not be a liquor, but by reaſon of that various agitation of its minute parts, wherein fluidity, a quality eſſential to liquors, conſiſts. Upon the ſcore of this reſpective coldneſs of the water, the hand is refrigerated; for the ſpirits and juices of that organ meeting in the water, with particles much leſs agitated than they are, communicate to them ſome part of their own agitation, and thereby loſe it themſelves, upon which decrement of wonted agitation, ſuch a change is made in the ſenſory, and, though not ſo manifeſtly in ſome other parts of the body, as is perceived by the animadverſive faculty under the notion of coldneſs; ſenſation (whatever obſcure definitions are wont to be given of it) being indeed an internal perception of the changes, that happen in the ſenſories.

AND if now, as the objection ſuppoſes, the water, wherein the hand is plunged, comes to be more refrigerated than before, the ſpirits, blood, and other parts of the hand, finding the aqueous corpuſcles more ſlowly moved than formerly, muſt, according to the laws of motion, (according to which a body, that meets another much more ſlowly moved than itſelf, communicates to it more of its motion, than if it were leſs ſlowly moved) transfer to them a greater meaſure of their own motion, and conſequently themſelves come to be deprived of it: and upon this increaſe of the ſlowneſs of motion in the parts of the hand, there follows a new and proportionable perception of the mind, and ſo a more vehement ſenſation of cold. But though it be not to be admired, that the bare ſlowneſs of motion in the object ſhould be diſcernable by ſenſe, albeit it ſeems to participate of reſt, which, with you, paſſes for a privation, ſince the ear perceives, when a voice grows faint, and when a ſharp ſound degenerates into a flat one; and we can perceive by the hand (abſtracting from heat and cold) the celerity or ſlowneſs of bodies, that in their paſſage ſtrike upon it, as for inſtance, of winds, or ſtreams; yet this is not the only thing I think fit to be taken notice of on this occaſion. For I conſider farther, that beſides the moſt conſiſtent and ſtable parts of the hand, there are, from the heart and the brain, freſh blood and ſpirits continually tranſmitted to the hand; and the former of theſe, the blood, is, according to the laws of its circulation, and after it has received a great change in the much refrigerated hand, carried back through other parts to the heart; whence it is, in the ſame circulation, diſtributed to the whole body. To which may be added, that when the great refrigeration of the hand happens, external agents may contribute to the effects of it, as I ſhall by and by have occaſion to ſhew.

IF then you pleaſe to remember, that upon the turning one's eye to the dark part of a room leſs enlighted than the window, though darkneſs be but a privation, and though the obſcurity of that part be not abſolute, but conſiſt only in a leſs degree of light; yet the action of the ſpirits, and other parts of the body, is ſo changed, upon occaſion of the light's acting more faintly than was uſual upon the organ, that the pupil is immediately and manifeſtly dilated, and in ſome caſes, as in that, which I mentioned to you of a learned man, much conſiderabler effects enſue; you will not wonder, that, where not only the ſpirits, but the blood, (whence thoſe ſpirits are generated) that circulates through the whole body, and upon whoſe diſpoſition all the other parts ſo much depend, is very much diſaffected, there ſhould be felt a great alteration in the hand, which is the moſt immediately expoſed to the action of the cold water. And for the reaſons newly given, it ought to be as little ſtrange, that in other parts of the body, the diſordered, and not circulating blood, ſhould have its wonted action on them conſiderably altered; ſince the more ſtable parts, and eſpecially thoſe external ones, that are moſt expoſed to the cold, have their pores ſtraitened, and conſequently their texture ſomewhat altered; on the ſame occaſion, on which the wonted agitation of the ſpirits, with the particles, that compoſe the blood, is notably leſſened. And that ſuch cauſes may produce great effects in a human body, you will be more prone to admit, if you conſider the diſorders, that happen in the cold fit of an ague, and oftentimes, upon the ſhutting up of thoſe excrementitious ſteams, that are wont to be diſcharged by inſenſible tranſpiration; to whoſe being ſtopped in the body, by the conſtriction of the pores, which chiefly happens through cold, ſome learned phyſicians, eſpecially the famous *Sennertus*, impute the cauſe of moſt fevers, as indeed experience itſelf does but too frequently ſhew it to be guilty of many.

Phil. I confeſs, *Carneades*, you have ſaid ſome things, that I thought not on before; but yet *Gaſſendus*'s argument ſeems to be ſuch, that I fear it will be hard to hinder many from ſaying, That if cold be but a privation of heat, it is a privation of a ſtrange nature: for it may be introduced into bodies, that were not hot before, nay, in ſome caſes, into ſuch as are naturally cold, and alſo by conſequence muſt have been put into a preternatural ſtate to be at any time hot.

Carn. This objection, *Philoponus*, being in effect ſo much the ſame with that of *Gaſſendus*, that it differs from it but in the dreſs you give it, it will ſcarce require a peculiar and diſtinct anſwer; and therefore, as ſoon as I have reminded you of the diſtinction, that we have formerly made of the vulgar and philoſophical ſenſe of the word cold, I ſhall need to alter but a little what I ſaid before, by telling you, that ſince fluidity conſiſts in the various agitation of the inſenſible corpuſcles of a liquor, and that heat conſiſts in a tumultuary, but a more vehement agitation of the inſenſible parts of a body, and ſo, that hot water ſcarce differs otherwiſe than gradually from that, which is cold to ſenſe; if cold be taken in the larger and philoſophical ſenſe, it may well be ſaid, that as long as water retains the form of water, and ſo continues to be a fluid body, though it may be very cold to the touch, yet it is not abſolutely or perfectly cold, and therefore is capable of a farther degree of coldneſs, which it receives when brought to congelation: for till then it was not deſtitute of thoſe agile corpuſcles, that were requiſite to keep it fluid; and till then, *Gaſſendus* himſelf muſt acknowledge, that it was not abſolutely, or perfectly cold; becauſe he, as you may remember, did in his former (but lately-mentioned) argument aſcribe the glaciation of water to the invaſion of thoſe, that he calls corpuſcles of cold.

Eleuth. Give me leave to add, *Carneades*, that it is not every glaciation it ſelf, that brings liquors to be perfectly cold in the philoſophical ſenſe of that expreſſion, and quite expells, or ſubdues all the agile particles, that were in the water before it was turned into ice. For I think, that to effect this change, it is ſufficient, that ſo many of theſe reſtleſs particles be deſtroyed or diſabled, that there remains not enough of them to keep the water in a ſtate of fluidity, ſo that the ſurpluſage may yet continue in the frozen liquor, and whilſt they are there, perform ſeveral things, as the making it evaporable in the air, and even odorous, and by their receſs or deſtruction the ice may grow yet more cold. And as this notion ſuits very well with the differing degrees of hardneſs, that we find in differing portions of ice, ſometimes upon the account of the matter, (as frozen water is harder than frozen oil,) and ſometimes upon that of the different degrees of cold in the ſame water, or other matter, (as our friend ſomewhere obſerves;) ſo it may be highly confirmed by an experiment I ſaw him make, but that is not yet publiſhed.

The ſum of the experiment was this; that he firſt put an hermetically ſealed thermoſcope into a glaſs, broader at the top than at the bottom, and greaſed the inſide with tallow, that ice might not ſtrongly ſtick to it. In this glaſs was put water, more than enough to cover the ball of the inſtrument; and that water being warily frozen, notice was taken, whereabouts the tincted ſpirit of wine reſted in the ſtem; after which, the inſtrument and the ice being removed into the open air, upon an exceeding froſty morning, the ice was taken off from the ball, and preſently after, the tincted liquor, as the maker of the trial expected, ſubſided a pretty way (the length of the inſtrument conſidered) below the former mark; which argued that he rightly gueſſed, that ſuch a degree of cold, as is ſufficient to turn water into ice, may not produce a body perfectly cold; this ice it ſelf keeping the encloſed ball, in a ſenſe, warm, by fencing off the air, which, at that time, (even in our temperate clime) by the effect, appeared to be colder than the very ice. And, methinks, it may ſtrengthen *Carneades*'s diſcourſe, to repreſent, that there is no ſufficient cauſe, why many things, that are reckoned among privations, or negations, by the Peripateticks themſelves, as well as cold is by *Carneades*, may not admit of degrees; as may be exemplified by deafneſs, ignorance, and divers other things. And to bring a caſe, not very unlike that under conſideration, we may take notice of a total eclipſe of the moon, which you know always happens when ſhe is at the full. For darkneſs in the air being acknowledged to be a privation or negation of light, when the earth, interpoſed betweeen the moon and the ſun, has eclipſed her, for inſtance, nine digits, (as aſtronomers ſpeak,) men generally complain of darkneſs in the air, though there remain a conſiderable part of the diſcus, or the hemiſphere of the moon, obverted to us, yet enlightened by the ſun; but when the interpoſed earth proceeds to cover the remaining three digits, and ſo makes the eclipſe total, the darkneſs alſo is ſaid and eſteemed to be much encreaſed: nor would men otherwiſe be perſuaded, though *Themiſtius* ſhould tell them, that the air cannot have grown darker, though it were dark before; and indeed though the air was more and more darkened in proportion to the increaſe of the eclipſe, yet it was never completely darkened till it became total. But I fear I dwell too long upon one argument.

SECTION IV.

Eleuth. LET me therefore, *Carneades*, ſum up what I take to be your doctrine, and tell theſe gentlemen, that I think you do not look upon the ſenſation of cold as a thing effected by an intire privation, properly ſo called, and conſidered as ſuch; but that, according to you, that ſlowneſs of motion in the particles of cold water, which the hand finds, when it is thruſt into that liquor, does occaſion the ſpirits, and the corpuſcles of the blood, to part with to thoſe of the water a conſiderable ſhare of their own ſurpluſage of agitation, whereby they loſe it themſelves; upon which is conſequent a perception of this change made in the hand, which, if it be very great, is alſo frequently accompanied with ſome ſenſible change in other parts of the body, occaſioned chiefly by the frequent returns of the circulating and highly refrigerated blood to the heart, whence it is diſperſed to the whole body. According to which doctrine, the ſenſation of cold is but a perception of the leſſened agitation of the parts of the hand, either ſtable or fluid, eſpecially of the blood; which alterations are in great part produced, not by the coldneſs of the water, as cold is a privation, but from the new modification of the action of the blood and ſpirits upon the nervous and membranous parts, the conſtriction of whoſe pores concurs to that modification. And if I do not miſunderſtand your opinion, *Carneades*, methinks it may be confirmed by this, which I have known obſerved by experienced ſurgeons, that by too ſtrict ligatures unſkilfully made, an arm, for inſtance, may be gangrenated; in which caſe, all the proper and immediate effect of the ligature is but the conſtriction of the part, though that conſtriction being unuſual and exceſſive, it proves the occaſion of the mortifying of the hand and arm, by hindering the free and uſual acceſs of the blood and ſpirits to that limb; upon which, by the depraved action of the parts of the body one upon another, and the concurrence of external agents, there enſues a mortification, or gangrene of the part, which, if due remedies be not timely employed, is communicated to other parts, and kills the man.

Carn. WHATEVER become of your inſtance, *Eleutherius*, I thank you for your readineſs to propoſe it in favour of my hypotheſis, which you will eaſily judge not to be much concerned in the cloſe of the excellent *Gaſſendus* his argument, for the poſitive nature of cold. For though theſe words of his ——

Them. You may ſave your ſelf the trouble of naming of them now, ſince, whatever they may ſeem to you, I profeſs I look upon them, as containing a diſtinct argument, which I ſhall therefore propoſe in its due place hereafter; but in the mean time, and before we leave the argument you would have us diſmiſs, give me leave to remind you, *Carneades*, of ſome part of your former diſcourſe, and to take thence a riſe to tell you, that you, who told us, that we ought not to conſider the operations, that qualities have upon our own ſenſories only, but alſo what they do to other bodies, will, I hope, allow me to demand, how a privation, or, if you will, how an imminution of motion can produce the hundredth part of thoſe effects, which we daily ſee produced by cold in the bodies, that are about us.

Carn. I thought, *Themiſtius*, I had intimated to you already, what might have prevented your queſtion; but ſince I ſee it is otherwiſe, you ſhall not find me backward to explain my ſelf a little more fully. I do not pretend, that either an abſolute privation of motion in a body, or a ſlowneſs of motion in the parts of it, is, as ſuch, the proper efficient cauſe of the effects, vulgarly but unduly aſcribed to cold alone; for, in my opinion, cold is rather the occaſion, that the true efficient cauſe of ſuch effects, which, I think, are properly to be aſcribed to thoſe phyſical agents, whoſe actions, or operations, happen to be otherwiſe modified, than elſe they would have been upon the occaſion of that imminution or ſlackneſs of agitation, which they meet with in cold bodies, by occaſion of which, they are both deprived themſelves of the agitation they communicate to ſuch ſlow bodies, and thereby act no longer, as, were it not for that loſs, they would, and by a natural conſequence of this change, which is made in themſelves, they do alſo, though leſs notably, modify the action of other bodies upon them: From which unuſual alterations happening in a world ſo framed as this of ours is, and governed by ſuch laws, reſpecting motion and reſt, as are obſerved among bodies, there muſt, in all probability, reſult many new, and ſome of them conſiderable phænomena. For though quieſcent bodies ſeem not to have any action, which among corporeal ſubſtances ſeems to be performed only by local motion; yet bodies quieſcent themſelves may concur to great effects, both by determining the motions of other bodies, this or that way, or by receiving their motion totally, or in part, and ſo depriving the formerly moving bodies of it. Thus the arches of a bridge, though immoveable themſelves, by guiding the water of the river, that beats againſt them, may occaſion a rapid and boiſterous ſtream, capable to drive the greateſt mills, and perform more conſiderable effects, though the river, before it met with them, ran calmly enough, as is evident at *London* bridge, eſpecially when the water is near a low ebb. And now I have mentioned water, I will add, that though water it ſelf be not a quieſcent body, but, being a liquor, has its parts in perpetual motion among themſelves; yet ſince that agitation is exceeding ſlow, in compariſon of the ſwiftneſs of a cannon-bullet, in reſpect whereof the calm ſurface of the water participates of the nature of a quieſcent body, bullets themſelves ſhot from out of guns elevated but little above the level of the water, (upon which ſcore they make but a very ſharp angle with it;) theſe bullets, I ſay, do not unfrequently rebound from the ſurface of the water, and conſequently, even theſe ſo wonderfully ſwift bodies receive a new determination from it.

Eleuth. One may add, *Carneades*, to your inſtances, that in a tennis-court the wall, againſt which balls are ſtrongly impelled by a racket, contributes much to the miſchief, that thoſe balls do often to by-ſtanders in the gallery, as the wall, though itſelf unmoved, gives a new determination to the moving ball, and by its reſiſtance makes it rebound or reflect at an angle equal to that of the ball's incidence. And this concurrence of the wall to ſuch effects is the more evident, becauſe of this other circumſtance, which alſo befriends your opinion, that, if the impelled ball, inſtead of hitting againſt the wall, hits againſt the net, this, by yielding, deprives the ball of its impetus, and hinders the reflection, that would elſe enſue.

Carn. You have, I confeſs, ſomewhat prevented me, *Eleutherius*; but yet not altogether: for though I was going to propoſe the example of a ball, yet it was in ſomewhat a differing way; for I was about to propoſe to *Themiſtius* the example of a ball, which, if it be forcibly and perpendicularly thrown againſt the hard ground, has its determination ſo altered, that whereas it moved before towards the centre of the earth, it immediately, with almoſt the like ſwiftneſs of motion, tends directly upwards. And if on the other ſide you throw the ball, not againſt a hard, but againſt a muddy piece of ground, it will not rebound, loſing its own motion, by communicating it to the parts of the yielding mudd; as may be in ſome meaſure illuſtrated by the great commotion made in a ſmall pond of water, when a ball (or a round ſtone) being but gently let fall upon the ſurface of it, has its motion thereby deaded, and transferred to the parts of the liquor, which, perhaps, will be viſibly agitated at the remoteſt brink of the pond.

Eleuth. These examples may conduce much to explicate your doctrine, *Carneades*; but ſince *Themiſtius* himſelf was ſo equitable a while ago, as to allow you much time to defend ſuch a paradox as yours againſt *Gaſſendus*'s argument, I ſhall with your leave, of which I doubt not, to the examples already mentioned, add this one more. Suppoſe upon a ſtream, that runs through ſome town, which is not very rare, there were built a number of differing mills, ſome for the grinding of corn, others for the fulling of cloth, others for the moving of bellows to melt oars and metals, others for forging of ſword-blades, others for making of paper, and others for other uſes: and ſuppoſe, that an enemy coming to beſiege this town, ſhould ſucceſsfully imitate *Cyrus*'s ſtratagem, when by ſuddenly diverting the courſe of *Euphrates* he took *Babylon*; would it not be conſequent to this dirivation of the water into ſome lower place, and this ceaſing of the ſtream to run in its former channel, that the action of all theſe mills, by which ſo many differing operations were performed, muſt of neceſſity ceaſe too? though the beſiegers do not produce this change by any poſitive and direct violence, that they offer to the mills, but only by hindering them from receiving the wonted impulſes, which were requiſite to keep them in motion.

Carn. I diſlike not your inſtance, *Eleutherius*, which yet will not altogether render uſeleſs what I was going to ſay about a wind-mill, which will illuſtrate one part of my doctrine, for which your water-mill does not ſeem to have been intended. And, that this example may the better do ſo, I will ſuppoſe a wind-mill to be built in ſome low place near the bank of your ſtream, which ſtream we will ſuppoſe to be liable, as ſome others are, upon the falling of great and ſudden rains upon the neighbouring hills, to overflow its banks, in caſe the increaſe of the water be not then hindered by the wind-mill's lifting up conſtantly ſome parts of it, and conveying it away by pipes or otherwiſe: and then let us ſuppoſe, what really ſometimes happens, that the wind ſhould ſo ceaſe, that there ſhould not blow any wind ſtrong enough to move the ſails for a great while together; will it not hence manifeſtly follow, that by reaſon of this abſence of the wind, which abſence has the nature of a privation, or negation of a ſtream-like motion in the air, not only there will be a ceaſing of thoſe effects and operations, whatever they were, that were wont to be performed within the mill itſelf, but alſo there will be a durable intermiſſion of that main work of the mill, whereby it carried off ſuch a quantity of water; which work ceaſing with the wind, whilſt the flowing in of the water does not ceaſe too, but continues as formerly, the ſtill-increaſing water muſt bear down or overflow its wonted banks, or other boundaries, and by its unruly effuſions drown the neighbouring parts, and produce the diſorders, that is, the new phænomena naturally conſequent to an inundation made by ſuch a quantity of water. And if the water conveyed away, by means of the mill, through pipes or channels, were employed to water grounds, or other particular uſes, the growth or fertility, at leaſt of the vegetables, that water was requiſite to nouriſh, or the other uſes, to which it was neceſſary, muſt conſequently be much, if not totally, hindered.

Phil. I know not, whether we may not refer to the ſubject of your diſcourſe, what may be obſerved in paralytick affections, where a little viſcous or narcotick humour, obſtructing, or otherwiſe diſaffecting one part of a nerve, though its proper and immediate action be only to hinder, or weaken the ſpirits, that were wont, in competent plenty, to paſs freely along the nerve to the muſcles whereto it leads; yet the action of the other parts of the body, and the relaxation of the fibres do oftentimes produce a tremulous motion in the limbs, and particularly the hands; and ſometimes alſo the mouth, neck and other parts, are drawn awry in an odd and frightful manner.

Carn. THOUGH I approve of *Philoponus*'s fancy, yet I think a more quick and notable inſtance to the ſame purpoſe may be taken, from what happens to birds, and rats, and cats, and ſuch kind of warm animals, in Mr. *Boyle*'s engine. For, as the air by the agitation of its parts, or that of ſome ethereal ſubſtance, that pervades it, entertains the fluidity of water, and other aqueous liquors; and when that agitation is hindered, or too much leſſened, water ceaſes to be fluid, and upon that divers violent effects enſue, wont to be aſcribed to glaciation: ſo the bodies of warmer animals, having been borne in the air, and perpetually expoſed to the action of it (though that be ſeldom heeded) when being placed in the receiver of the air-pump, and by the operation of that inſtrument, which withdraws the former air, and keeps out the new, the air, that was wont continually to act upon them, is kept from doing ſo any longer, though this abſence, or not touching of the air, be but a privative or negative thing, yet by reaſon of the ſtructure of the animal, his ſpirits and humours, aſſiſted by the concourſe of more general cauſes, are brought to act ſo differingly from what they were wont to do, that the blood and juices ſwell, the ſtomach vomits, the animal grows faint and ſtaggers, the limbs, and at length the whole body are convulſed, the circulation is ſtopped, and at laſt the whole animal killed; and all this done in a very few minutes of an hour, without the viſible intervention of any poſitive agent.

Eleuth. WHAT you ſay, *Carneades*, concerning the quick and violent death of warm animals in Mr. *Boyle*'s engine, puts me in mind of an experiment I ſaw made in that inſtrument upon cold animals, which, methinks, may well illuſtrate the compariſon we lately employed of a wind-mill. For as thoſe great artificial engines loſe their motion, and the operations depending on it, if that ſtream of air, we call the wind, be held from keeping them going; ſo inſects, and ſome other cold animals, have their differing motions ſo dependent upon the contact of the air, that, as ſoon as ever they are deprived of it (by the engine we are ſpeaking of) divers ſorts of them will lie moveleſs, as if they were dead; and I have known ſeveral of them, that were put in together, continue in that ſtate for many hours, as long as it pleaſed our friend to with-hold the air; but when once he thought fit to let a ſtream of air enter the receiver, theſe ſeemingly dead animals, as worms, bees, flies, &c. like ſo many little wind-mills of nature's (or rather, her great author's,) making, were ſet a moving in various manners (as creeping, flying, &c.) ſuitable to their differing ſpecies.

Carn. So that, to ſum up, in a few words, the reſult of theſe inſtances, and the reſt of the paſt diſcourſe on the ſame ſubject, it appears by what has been ſaid, that the effects undeſervedly aſcribed to cold need not, in our hypotheſis, be referred to a privation, but to thoſe poſitive agents, or active cauſes, which, by their own nature, are determined to act otherwiſe, or ſuffer otherwiſe from one another, in caſes, where there is a great hinderance; or ceaſing of wonted agitation, than where there is not.

SECTION V.

Them. IT may, perhaps, now be time to put *Carneades* in mind, that, in what he has been diſcourſing all this while, he has propoſed anſwers but to a couple of *Gaſſendus*'s arguments, and left the reſt untouched.

Carn. I ſhould readily grant, *Themiſtius*, that I have dwelt too long upon ſo few arguments, if I did not hope, that by fully anſwering them, and giving the company a particular account of my notions concerning cold, I might very much ſhorten and facilitate the remaining part of my taſk, which engages me to return anſwers to the other arguments you ſpeak of, the grounds of ſolving which, I think, I have already laid in the paſt diſcourſe. And therefore you may go on to propoſe the next argument of *Gaſſendus*, as ſoon as you pleaſe.

Them. And I ſhall do it, *Carneades*, in that learned man's own words, which I well remember to be theſe: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quamobrem manus intra iſtam, non intra illam refrigeratur? An quia calor manûs intra frigidam retrabitur, manûſque proinde relinquitur calida minùs? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? at ſi frigus eſt tantùm privatio, quidnam calor ab illâ metuit? privatio ſanè nihil eſt, atque adeò nihil agere, unde ejus motus incutiatur, poteſt.*

Gaſſend. Lib. VI, cap. 6.

Carn. This objection, *Themiſtius*, may indeed puzzle many ſchool-philoſophers, but will eaſily admit an anſwer in my hypotheſis. For that does not oblige, or ſo much as tempt me to aſcribe (as a Peripatetick would do,) to a meer quality, (for ſuch is heat) both a knowledge of its danger, and a care, and ſkill, to preſerve itſelf from its enemy, the cold, by a retreat inwards. For, agreeably to what I lately delivered, it is obvious for me to explicate the phænomenon thus: when a man puts his hand into warm water, the agitation of the corpuſcles of that liquor ſurpaſſing that of the ſpirits, blood, and other parts of his hand, cannot but excite in him a ſenſe of heat; but when he puts the ſame hand into cold water, the caſe ought to be much altered, not by any imaginary retreat of the ſpirits, but the communication of motion, by other parts, to the ſurrounding water, by which means, there muſt be in the hand a great leſſening of the former agitation of its parts, the perception or ſenſe of which decrement of motion is that, which we call the feeling of cold.

Eleuth. I think indeed, *Carneades*, that though this argument may be conſiderable againſt thoſe, that the learned framer of it might have in his eye, it is but invalid againſt you. But can you as well decline the force of that other objection, which *Gaſſendus* more inſiſts on, and which ſeems as directly to oppoſe you, as any other adverſaries of his hypotheſis?

Them. I preſume, *Eleutherius*, you mean that cogent argument, with *Gaſſendus* propoſes, and proſecutes more fully, than the reſt, deducing it from the way of artificially freezing water by a mixture of ſnow and ſalt, placed about the outſide of the glaſs, that contains the liquor. For, from this practice, he rationally concludes, that ſince this frigorifick mixture is, through the glaſs, able to freeze the water into ice, it may as juſtly be affirmed to act by corpuſcles of cold, as fire can be to act by calorifick corpuſcles, when kindled coals, placed on the outſide of the glaſs, make the contained water boil. And this cogent argument will, I hope, prove the more ſatisfactory to *Carneades*, ſince it is not drawn from what he would call a diſputable peripatetick notion, but from the ſame quiver, whence he affects to take his ſhafts, experience itſelf.

Carn. I freely acknowledge, gentlemen, this argument to be very plauſible; but that it is clear and cogent, I muſt not grant, till I be better ſatisfied, that it is ſo.

And I ſhall ſcarce think it as evident, that ice, and ſalt, act by a poſitive quality, as that burning coals do ſo, though cold ſeems as well to be produced by the former, as heat by the latter. For innumerable experiments ſhew, that heat, in the fire eſpecially, is a poſitive quality, conſiſting in a tumultuary and vehement agitation of the minute parts of the body, that is ſaid to be hot, and producing alſo in the bodies, that it is communicated to, a local motion, which is manifeſtly a poſitive thing. This is ſo evident, in the heating of bodies by mere attrition, the ſmoking and melting of divers bodies in the ſun-beams (eſpecially at fit times of the day, and year,) the ſudden boiling and diſſipation of water, oil, &c. dropped, on a red-hot iron, and many other obvious inſtances, that it were a needleſs work to go about to prove it, eſpecially, ſince both *Themiſtius*'s Peripateticks, and *Gaſſendus* himſelf, who ſo often diſagree about other things, agree in confeſſing, that heat is a poſitive quality.

Them. But remember, *Carneades*, that the grounds, on which they do ſo, are the ſame, on which *Gaſſendus* juſtly builds the propoſition, that cold alſo is a poſitive quality.

Carn. I did not forget that, *Themiſtius*; for I was about to ſubjoin to what I laſt ſaid, that it is evident, not only by the confeſſion of my adverſaries, but by that (which to me is much more conſiderable) of nature herſelf, proclaiming it in the inſtances I juſt now mentioned, that heat is a poſitive quality; whereas, that cold likewiſe is ſo, does not appear to me by the experiment of artificial congelations. For, in this, all that is clear in matter of fact, is, that ſnow, or beaten ice, and ſalt, are put about a veſſel full of water, or other aqueous liquor, and that, within a while after, this water begins to be turned into ice; but that this glaciation is performed by ſwarms of atoms of cold, that permeating the glaſs, invade and harden the liquor, is not perceived by ſenſe but concluded by a ratiocination, the cogency of which I am allowed to examine, without affronting the certainty of ſenſe, that not being concerned in the caſe. If then an intelligible way can be propoſed of fairly explicating the phænomenon, beſides that inſiſted on by *Gaſſendus*, the objection drawn from this experiment againſt my hypotheſis will be invalid. And ſuch an explication, monſieur *Des Cartes* ingeniouſly gives in his meteors: *Quia materia ſubtilis* (ſays he) *partibus hujus aquæ circumfuſa craſſior aut minus ſubtilis, & conſequenter plus virium habens, quàm illa quæ circa nivis partes hærebat, locum illius occupat, dum partes nivis liqueſcendo partibus ſalis circumvolvuntur. Facilius enim per ſalſæ aquæ quàm per dulcis poros movetur, & perpetuò ex corpore uno in aliud tranſire nititur, ut ad ea loca perveniat, in quibus motui ſuo minùs reſiſtitur: quo ipſo materia ſubtilior ex nive in aquam penetrat, ut egredienti ſuccedat, & quum non ſatis valida ſit ad continuandam agitationem hujus aquæ, illam concreſcere ſinit.*

Lib. Meteor. Cap. III.

Phil. I leave *Themiſtius* to conſider, whether this explication be without exception; but I confeſs it is not without analogy, and that even amongſt the four firſt qualities themſelves. For when we chemiſts have a mind to dry (for inſtance) the calces, or precipitates, or other powders, from which we have filtrated the liquors we employ to waſh or dulcify them, it is uſual either to put the filters, wherein theſe powders remain almoſt in the form of mudd, or to ſpread the ſtuff itſelf upon brown paper, or pieces of brick, or chalk, which much haſten the exſiccation of the things laid upon them, not by any drying particles which they emit into the ſoft ſubſtances, but by imbibing the ſuperfluous parts of the liquor, and thereby freeing from them the ſubſtances to be dryed. And I remember, I have ſeen our friend Mr. *Boyle*, by immerſing a piece of ſoft crumb of bread into an actually cold liquor, that would haſtily imbibe its aqueous corpuſcles, and dry it in a minute, or two, of an hour, ſo as to make it feel hard.

Eleuth. Theſe inſtances bring into my mind another chemical experiment, that I have ſeen made by the ſame gentleman, which was; that by putting into weak ſpirit of wine a ſufficient quantity of ſalt of tartar, he quickly defleghmed the ſpirit without diſtillation, or ſo much as heat. And this will the better illuſtrate the *Carteſian* explication, becauſe it is manifeſt, by the change, that will be made of the moſt part of the ſalt of tartar into a liquor, that will not mix with the now defleamed ſpirit of wine, that the reaſon of the operation is, that the aqueous particles of the phlegmatick ſpirit, finding, it ſeems, more convenience, or facility, to continue their motion among the fixed corpuſcles of the ſalt, than the vinous ones of the ſpirit, paſs into the alkaly, and diſſolve it; and thereby deſert the liquor, through which they were diffuſed before. And I know another ſaline body, that ſo unites with water, as not to be, by the eye, diſtinguiſhable from it, and yet is of ſuch a texture, that water is ſo much leſs diſpoſed to mingle with it, than with ſpirit of wine itſelf, that it will forſake the body it kept in agitation, to paſs into this ſpirit; and ſo leave that, which it kept in the form of a liquor before, to appear in the form of a conſiſtent body; which inſtance comes ſomewhat nearer, than the former, to the experiment of glaciation.

Carn. THOUGH what you have recited, gentlemen, be not unwelcome to me, yet, I think, I can propoſe you an experiment fitter to delucidate the Carteſian explication. For, I remember, that our common friend, having a mind to ſhew, that a ſmall proportion of agile matter, inviſibly diffuſed through a body, that would be otherwiſe conſiſtent, may bring it to, and keep it in the ſtate of fluidity; deviſed and ſhewed me the following experiment. He took camphire broken into ſmall bits, and caſting a convenient quantity of it upon aqua fortis, ſuffered it to float there, till, without heat, the camphire was diſſolved into a liquor, and it looked and felt like an oil, which, though ſhaken with the aqua fortis, would emerge to the top again. If this oil were kept well ſtopped, that the ſpirits of the menſtruum might not evaporate, it would (as he affirmed trial had taught him) continue long fluid, he having ſometimes kept it a year, or two, or more. And that it is the agile ſpirits of the aqua fortis, that keep the camphire fluid, he has made probable by divers things, that I muſt not now ſtay to recite. And that the quantity of theſe agile particles is but ſmall, I am induced to think by this, among other things, that when I have made a ſmall parcel of but moderate aqua fortis turn a pretty proportion of camphire into oil, and ſeparated that oil from it, I could, by caſting freſh camphire on the ſame menſtruum, reduce that alſo into the form of oil. Now, that theſe fluidifick ſpirits (if I may ſo call them) are not ſenſibly warm (no more than the Carteſian *materia cœleſtis)* in water, is manifeſt to the touch: and whereas I at firſt ſuſpected, that the reaſon, why the pouring of this oil into water doth preſently reduce it into camphire again, might by the coldneſs of the water; I after thought, upon a farther information, that the reaſon rather was, that the nitrous ſpirits being diſpoſed to paſs out of the oil into the water, this liquor readily imbibed and diluted them, and conſequently, diſabled ſo many of them, that thoſe, that remained, could not do their former work any longer: ſince he had tried purpoſely, that the reduction of the oil into camphire would preſently be made, though that liquor were not poured into cold water, but hot; ſo that the agitation, that it received from the particles of the menſtruum, though not to our touch ſenſibly warm, was much more efficacious, than that, which it received from the heat of the water.

Eleuth. I know not, whether beſides the inſtances, that have been now propoſed, one may not alledge ſuch an argument alſo in favour of the Carteſian opinion about cold, as would not be inſignificant, though it ſhould be made appear, that cold may ſometimes be produced by, or upon the emiſſion of corpuſcles, that in ſome ſenſe may be called frigorifick. For there may be corpuſcles of ſuch a nature, as to ſize, ſhape, and other attributes, as to be fit to enter the pores, and pierce even into the inward parts of water, and ſome other bodies, ſo as to expel the calorifick corpuſcles they chance to meet with, or to clog, or hinder their activity, or on ſome other account, conſiderably to leſſen that agitation of the minute parts, by which the fluidity of liquors, and the warmth of other bodies, is maintained. But even in ſuch caſes, though the agent, and the actions, that produce coldneſs, be poſitive things; yet the nature of coldneſs itſelf may conſiſt in a privation. As when a man is killed by a bullet, his death is effected by a poſitive, and even impetuous action, and yet death itſelf is but a privation of life. If alſo, in a dark room, a man caſt cold water upon a burning coal, though the water act by its poſitive quality of moiſture, and, by virtue of that, extinguiſh the fire, and, by that means, deſtroy the light, yet the darkneſs, that is conſequent upon this action, is not a poſitive thing, but a privation.

SECTION VI.

Phil. THE pauſe you here made, Gentlemen, makes me think it ſeaſonable to put the company in mind, that it begins to grow late, and therefore to call upon *Themiſtius* to produce what he has yet to alledge out of *Gaſſendus.*

Them. The philoſopher, you have named, has indeed another weapon to deſtroy the error about cold, which he confutes. And this argument, like a two-edged ſword, that cuts on both ſides, does not only confirm what he maintains, but deſtroy the chief objection, that can be made by his adverſaries. The argument I ſpeak of, he propoſes in theſe terms: *Tametſi multa videantur ex ſola caloris abſentia frigeſcere, nihilominus niſi frigus extrinſecus introducatur, non tam profectò frigeſcere quàm decaleſcere ſunt cenſenda. Eſto enim lapis, lignum, aut aliquid aliud, quod nec calidum, nec frigidum ſit, id ubi fuerit admotum igni caleſiet ſanè; at cùm deinceps calor excedet, neque frigidum ullum circumſtabit, non erit cur dicas ipſum frigeſieri potius quàm minùs calidum fieri, redirève in ſuum ſtatum.*

Carn. WHETHER this contain not a diſpute *de modo loquendi*, I ſhall leave the company to judge, by what I ſhall return in anſwer to it. I ſay then, that it ſeems to me, that there is in the diſcourſe an obſcurity, if not an ambiguity, though, I am confident, not affected by the candid *Gaſſendus.* But to anſwer as directly as I can; if we ſpeak only of a coldneſs, as to ſenſe, I ſee not, why water, or wood, or any ſuch body, that is heated by the fire, may not, upon its removal thence, be ſaid to grow cold, and not barely to *decaleſcere*, in our philoſopher's ſenſe of that word. For the heat and coldneſs of water, in reference to ſenſe, conſiſting, as I lately ſhewed in this, that the particles of it are more or leſs agitated, than the hand that is immerſed in it, they need nothing elſe to make the liquor grow cold, than ſuch an imminution of the briſk motion of its corpuſcles, that they ceaſe to be as much agitated, as thoſe of our organs of feeling: and if this already impaired agitation be ſtill more and more leſſened, the liquor will ſtill grow colder and colder, without the help of any poſitive cauſe, until at length the agile parts, that kept it fluid, being quite expelled, or diſabled, the form of the liquor comes to be exchanged for that of ice.

Phil. But what ſay you to that part of *Gaſſendus*'s argument, where he propoſes an adiaphorous body, which, when affected with an adventitious heat, would not grow cold by the bare removal, or ceſſation of that heat, unleſs it were refrigerated by an agent, that were poſitively and actively cold?

Eleuth. I ſay, *Philoponus*, this ſuppoſition ſhould not be made, and that I know of no ſuch adiaphorous body. For ſince, as I have been obliged to inculcate, thoſe bodies muſt be cold, as to ſenſe, whoſe parts are leſs agitated than thoſe of our hands, and conſequently metals, ſtone, wood, and other ſolid bodies, and alſo water, wine, and all other unmingled liquors, we know, being heated by the fire, will grow cold again of themſelves, becauſe the adventitious motion ceaſing by degrees, either upon the receſs of the igneous corpuſcles, or the imparting of the extraneous agitation to the air, or other contiguous bodies, the ſtone, or water, &c. will again have ſo much fainter an agitation, than that of a man's ſenſory, as to be by him judged cold: and becauſe almoſt all the ſpecies of permanent bodies here below, that are known, have, in what is called their natural ſtate, a leſs degree of agitation in their inſenſible parts, than men's organs of feeling are wont to have, thoſe bodies may be ſaid to be naturally cold, and therefore ought not to be ſuppoſed to be indifferent to cold or heat.

Phil. But whether or no nature do really afford us an adiaphorous body; yet ſurely the mind is able to conceive one, and therefore *Gaſſendus* may be allowed to ſuppoſe ſuch bodies, and *Carneades* may be obliged to anſwer what he argues upon that ſuppoſition.

Carn. It is one thing to propoſe an adiaphorous body, as barely an intelligible, or a poſſible thing; and another, to give inſtances, of it, as *Gaſſendus* has done in particular bodies, in which that indifference is not to be found. And it is this laſt kind of ſuppoſition, that I diſallowed in *Gaſſendus*'s argument. But if a body ſhould be propoſed, as adiaphorous in reference to heat or cold, I might ſay, without prejudice to my cauſe, that if ſuch a body ſhould be carried into a hot place, it might there grow warm; and if it ſhould be removed back again, and kept, till it loſt that new adventitious heat, it might rather *decaleſcere*, than grow cold as to ſenſe. But the reaſon is, becauſe, it is not every degree of imminution of heat, that is able to denominate a body cold, but ſuch a degree as reduces the parts of it to a fainter motion, than is at that time, in thoſe of our organs of feeling; and till this be done, or at leaſt very near done, the propoſed body is ſtill (if I may ſo ſpeak) in the ſtate of heat, as to ſenſe: which laſt words I add, becauſe, that in reference to other bodies, it may then be notably refrigerated. As lead, that has but heat enough to keep it in fuſion, may, by the pouring on of ſuch water, as to a man's hand, would feel hot, be brought to grow hard, which loſs of fluidity is alſo the natural effect of cold, though perhaps, both the metal, and the liquor, be yet as to ſenſe conſiderably hot.

Eleuth. So that, according to you, none of the kinds of bodies, that are actually known in nature, are adiaphorous as to ſenſe. On which occaſion let me note by the by, that the frequent variations of ſenſe muſt render it but an uncertain ſtandard of heat and cold: and upon ſuppoſition, that there were an adiaphorous body in reference to our ſenſe; yet it would not be ſo in reference to all other bodies, or, in the phraſe of our *Verulam*, ſpeaking of heat, *in ordine ad univerſum*. And for what remains, the controverſy grounded on *Gaſſendus*'s argument ſeems to be rather verbal, than real, and may be determined, or compoſed, by ſettling the diſtinct acceptions of the words cold and heat.

SECTION VII.

Phil. WHEREFORE I wiſh, that we may not waſte the little time, that is left us, upon niceties of no greater concernment; and I think this ſhort time would be better employed, if *Carneades* would be pleaſed to tell us a little more particularly, what he ſuppoſes to be the thing, that withheld Mr. *Boyle* from delivering an opinion about the nature of cold.

Eleuth. Yet, methinks, it is but fair, that *Carneades*, who has all this while been confined to the anſwering another's arguments, ſhould now take his turn to propoſe his own.

Carn. I find, in each of your motions, Gentlemen, ſomething ſo equitable, and ſo expedient, that I ſhal in part comply with both. And that I may haſten to do what *Philoponus* deſires, I ſhall do no more than briefly point at two things, that may be alledged in favour of the hypotheſis I defend. For if you reflect upon what we have already diſcourſed, we may take notice of things there, that will ſcarce be well accounted for by being aſcribed to poſitive cold, but may be far better explained agreeably to our hypotheſis. And muſt add, in the next place, that I, who ſuſtained the perſon of a reſpondent, may pretend to have ſufficiently diſcharged my office, if I have ſhewn the invalidity of all the opponents arguments; and it is his part, who aſſerts a poſitive thing in nature, to make it good, whereas he, that denies it, needs not alledge any other reaſon why he does ſo, than the authority of that juſtly received axiom in philoſophizing, *Entia non ſunt multiplicanda abſque neceſſitate*. And, I hope, there will need no other engine to demoliſh an ill-formed and proofleſs opinion about cold, than an axiom ſo ſolid and efficacious, that in the opinion of almoſt all the modern naturaliſts it has been able to aboliſh ſuch potent and immenſe bodies as the *primum mobile* itſelf, and a ſuperior orb or two, the leaſt of which contained that firmament, in compariſon whereof the whole earth is but a point. And not only ſo, but the ſame axiom has baniſhed the angels and intelligences from the cœleſtial orbs, that *Ariſtotle* and his followers, had aſſigned them to turn about; or rather hath releaſed thoſe noble and happy ſpirits from the drudgery, to which the philoſophers of ſo many ages, had needleſly doomed them.

Eleuth. I the leſs diſtruſt the validity of the axiom you alledge, becauſe I obſerve it to be the ground, on which is built a great part of the reformation of philoſophy, that is introduced by the moderns. For one of the main things, that firſt moved conſidering men to ſeek for more ſatisfactory opinions, than thoſe of the peripatetick ſchools, was, that theſe obtruded a great many tenets in philoſophy, that were not only unproved, but unneceſſary to the explication of the phænomena of nature; as it were not difficult to ſhew.

But I ſee *Philoponus* preparing to renew the motion he lately made, in which the ſhortneſs of time makes me now think it ſeaſonable to join with him, I being no leſs deſirous than he to know, what may be the motives of your friend's declining to declare himſelf fully about the nature and cauſe of cold.

Carn. I have already intimated to you, at the beginning of our conference, that he is himſelf the fitteſt perſon to be addreſſed to for ſatisfying this enquiry. But not to be altogether ſilent on this occaſion, I ſhall tell you, that, as far as I can gueſs, he waits till farther trials and ſpeculations have reſolved him in ſome points, wherein he is not yet ſatisfied: for, being of a temper backward enough to acquieſce without ſufficient evidence, when the enquiry is difficult, and the ſubject important; he ſeems to me to be kept in ſuſpence, both by ſome ſpeculative doubts, and the phænomena of divers experiments, ſome of which are not delivered in his book. It would be now improper to mention the ſcruples and heſitancies they have occaſioned in him; though of thoſe, I have heard him ſpeak of, I ſhall name ſome inſtances, that occur the moſt readily. As I remember I heard him make enquiry, as to thoſe, that would have cold produced by corpuſcles of cold; whether, and on what account, thoſe little fragments of matter are cold? whether thoſe frigorifick particles, that muſt in multitudes crowd into water to turn it into ice, have gravity or levity, or are indifferent to both? And how any of the three anſwers, that may be made to this enquiry, will agree to ſome phænomena, that may be produced? what ſtructure the corpuſcles of cold can be of, that ſhould make them frigorifick to that innumerable variety of bodies they are ſaid to pervade? And, whether the frigorifick faculty of theſe corpuſcles be looſable, or not? As alſo, whether or no they be primitive bodies; and if it be ſaid, they are not, whether there was not cold in the world before they were produced, and whence that cold could proceed? And if it were ſaid, they are primitive bodies, he demanded, how it came to paſs, that by putting a certain factitious body actually warm, into water, that was alſo warm, (both which appeared by a good ſealed weather-glaſs) there ſhould preſently be produced an actual coldneſs (diſcernible by the ſame thermoſcope?) Theſe, and I know not what other ſcruples and difficulties, ſuggeſted to him by his thoughts, or his experiments, were the things, that, I ſuppoſe, prevailed with a man of his temper to forbear for a while the declaring of his ſentiments about cold, leſt the event of ſome farther trial ſhould ſhew him cauſe to retract them.

Phil. What you have freſhly intimated, *Carneades*, of Mr. *Boyle*'s having other heſitations, than thoſe you have named and ſuggeſted by experiments, not publiſhed in his hiſtory, does, I confeſs, the more excite my curioſity to have, at leaſt, a taſte of thoſe preplexing phænomena.

Carn. You may eaſily gueſs, *Philoponus*, by what I have told you already, that you are not to exſpect a full ſatisfaction from me on this occaſion. But yet, that your curioſity may not be fruſtrated, I ſhall venture to acquaint you with two phænomena, which were, I ſuppoſe, none of the leaſt motives of his backwardneſs to declare himſelf. But though ſome body perhaps thinks, that the grounds of ſolving theſe phænomena, and moſt of the newly recited ſcruples, may be picked out of ſome things, that may already have paſſed among us in this conference; yet, becauſe we have not now time to enter upon a diſcuſſion of this matter, I am willing you ſhould ſuſpend the debate, till we have occaſion to meet another time; and therefore I ſhall now only acquaint you with a couple of experiments, that he ſet down for a virtuſo, who was to ſolve the two main problems ſuggeſted by them. The firſt whereof was, whence water ſhould, upon congelation, acquire ſo vaſt a force, as he found it had, to lift up great weights, and burſt containing bodies; though it ſeemed by ſeveral circumſtances, that the motion of the water is very much diminiſhed, when it is changed into ice. And the ſecond problem is thus conceived; if, as a briſk agitation of a body's inſenſible parts produces heat, ſo the privation of that motion is, as *Cardan*, and the Carteſians would have it, the cauſe of cold; whence is it, that, if certain bodies be put together, there will be a manifeſt and furious agitation of the ſmall parts, and yet, upon this conflict, the mixture will not grow hot, but ſenſibly and even conſiderably cold? The narratives themſelves, of the experiments, are too long to be now read over to you. And therefore, I ſhall leave the paper, that contains them, among you, to be peruſed at your leiſure, between this and our next meeting, till when I muſt bid you farewell; only deſiring you in the mean while, to remember, that, as I have but acted a part impoſed upon me in our paſt conference, ſo notwithſtanding any thing, that I have ſaid in my aſſumed capacity, I reſerve to myſelf the right of appearing as little pre-engaged, as any of you at our next meeting.

TWO PROBLEMS
ABOUT
COLD,
Grounded on NEW EXPERIMENTS,
And propoſed in a LETTER to a FRIEND.

To my very Learned Friend Mr. *J. B.*

SIR,

I PRESUME, that you will not be ſurprized to be told, that I ſend you the incloſed papers, not only, that I might gratify your curioſity, but, that you may by them be enabled to help me to ſatisfy my own; and therefore I ſhall accompany the hiſtorical tranſcripts I made of the following experiments, as I found them regiſtered for my own remembrance, with ſome of the doubts ſuggeſted to me by ſome of the phænomena, that occurred. But yet I ſhall not trouble you with all the difficulties, that at firſt troubled me, but reduce the exerciſe, I deſire to give your ſagacity, to the ſolution of two problems. And I will begin with propounding that firſt, which is grounded upon the laſt of the two following papers, becauſe, though the hiſtorical part of that be much the longeſt, yet the grounds of my quære concerning it, will be much more briefly propoſed, the experiment itſelf naturally ſuggeſting this problem; "How, "upon the mixture of two or three bodies, "ſuch as thoſe mentioned in the paper, there "ſhould manifeſtly enſue a great and tumultuary agitation of ſmall parts, and yet, even "during this conflict, not any ſenſible heat, "but a conſiderable degree of cold, be pro"duced," and that even in the internal parts of the mixture? Prob. II.

The inducements to make this problem need not be far fetched, it being obvious enough, that, according to the corpuſcularian philoſophy, which you and I agree in, a briſk, and various agitation of the minute parts of a body is that, which makes it hot, both in reference to our ſenſories, and to its operations on other bodies. But I doubt, the riſe of the problem is much more eaſy to be underſtood, than the cauſe of the phænomenon, about which I will not aſk you, whether one may not aſſert, that local motion is, in its own nature, a generical thing, which may be ſo diverſified by circumſtances, that one kind of modification of it, as it is made in corpuſcles of ſeveral ſizes, and ſhapes, may be the cauſe of heat, and another, that of cold? or elſe, whether we may ſuppoſe, that cold is a poſitive thing, and operates by real corpuſcles of cold, which happening to abound, and yet to be locked up in the bodies, whoſe mixture I employed, they are, by the great conflict, that diſſolves the texture of the claſhing ſalts, ſeparately put into motion, and that in ſuch numbers, that though really there would be a heat produced by the briſk and confuſed agitation of ſome of the parts, yet that heat is not only concealed, and checked, but maſtered by the over-powering operation of the frigorifick corpuſcles. But to aſk you about this, or any other particular way of ſolving our phænomenon, were to forget, that my aim is to learn not your opinion of this, or that particular conjecture, or fancy, about our problem, but in general, how it may be beſt reſolved, and what you think to be the true cauſe of ſo odd an effect.

HAVING thus diſpatched the little I had to ſay about the paper, that ſuggeſted the ſecond problem, I will now ſuppoſe, that you have read the phænomena, that contain the riſe of the firſt, to which I ſhall proceed, without farther preamble, ſince the queſtion, or problem, that theſe naturally call for, is, "Whence this "vaſt force of freezing water proceeds?" Prob. I.

FOR the breaking of reſiſting bodies being to be made by a violent local motion, and cold, according to the judgment even of the moderns, either conſiſting in, or, at leaſt, being accompanied with a privation, or a great imminution of motion, it ſeems very difficult to conceive, how cold ſhould make water to exert ſo wonderful a force. I know the learned *Gaſſendus*, and divers other philoſophers, teach us, that glaciation is performed by the entering of ſwarms of corpuſcles of cold, as they call them, into the liquor. But I much doubt, whether, from this hypotheſis, a good ſolution of our phænomenon will be derived, ſince theſe atoms of cold ſeem not barely, as ſuch, to make that expanſion of the water, which is required in the experiment by me recited. For I ſee, that though water will be more and more refrigerated, according as the air grows colder and colder, yet, till it be brought to an actual glaciation, all the ſwarms of the frigorifick atoms in it are ſo far from expanding it, that they more and more condenſe it. And even that degree of cold, which deſtroys fluidity, though it expands water, does not do it merely by the multitudes of the frigorifick corpuſcles, that invade the pores of the lately fluid body, ſince pure ſpirit of wine, and almoſt all chemical oiles, though expoſed to the ſame degree of cold, that turns water into ice, or, as I have tried, unto a far greater than is neceſſary to do ſo, will be but the more condenſed by thoſe ſwarms of particles. But, which is more conſiderable, I have carefully obſerved, that, beſides common or expreſſed oils, chemical oil of aniſeeds itſelf, being frozen, or concreted by an intenſe degree of cold, will not be expanded, but notably condenſed, and accordingly grow ſpecifically heavier than before. And this was one thing, that kept me from expecting the removal of our difficulty from the ingenious explication given of freezing by the Carteſians, when they teach, that the eel-like particles, whereof they ſuppoſe water to conſiſt, are very remiſsly agitated, and their want of pliantneſs makes their contexture leſs cloſe; which ſeems not to agree with the lately mentioned trials. And though theſe eel-like particles ſhould loſe all their flexibleneſs, though, in that caſe, it may probably be ſaid, that they would take up leſs room than before, if nothing oppoſe their expanſion, yet it does not thence appear, how they ſhould acquire ſo vaſt a power to expand themſelves in ſpite of oppoſition, as we have ſhewn water, by freezing, does acquire.

I did not hope to reſolve our problem by the help of a vulgar ſuppoſition, that well ſtopped veſſels are broken in froſty weather *ob fugam vacui*, ſince I found that ſuppoſition to be erroneous by divers experiments, ſome of which are mentioned in the hiſtory of cold.

It ſeemed leſs improbable, that ſome aſſiſtance to the ſolving of our difficulty might be given by two other things. Whereof the firſt is, that, for aught I have yet obſerved, no liquor but water, or that which participates of water, by having aqueous particles ſeparable from it, will be made to ſwell by cold; nor will water itſelf do ſo upon every degree of cold, but only upon ſo great an one as actually turns it into ice. And the ſecond is, that upon the glaciation of water, and aqueous liquors, we may obſerve in the ice many bubbles, greater or ſmaller, intercepted between the ſolid parts, and ſuppoſed to be full of air, (I ſay, ſuppoſed, becauſe, upon trial, I found them to have yielded but a ſmall proportion of common air;) which ſuppoſition, if true, would perhaps invite one to ſuſpect, that the air contained in theſe bubbles might have an intereſt in our phænomenon; ſince I have found, by trials purpoſely made, that air congregated into viſible, though not great portions, may exerciſe a conſiderable elaſticity, which appeared not whilſt it was inviſibly diſperſed through the water.

And if I did not ſuppoſe, both that you had taken notice, that there are wont to be numerous particles of ſpringy air diſperſed through the pores of water; and that you had conſidered, whether the want of pliantneſs, occaſioned by cold in the aqueous corpuſcles, whilſt they are yet agitated and brandiſhed by ſome permeating matter; and whether, upon the change of the pores, that we may conceive to be made in freezing water, either by the receſs of one ſort of ſubtil corpuſcles, or the admiſſion of another, or the cloſer conſtipation of the groſſer parts, there may not be produced in corpuſcles, that compoſe water, to ſay nothing of the intermixed air, or the concretions, or the coalitions, occaſioned by the cold) a ſpringineſs capable to make many little bodies, endowed with it, exert a great force againſt the ſides of the veſſel, that oppoſe their joint endeavour to expand themſelves: if, I ſay, I did not believe, that theſe, and the like ſuſpicions, had occurred to you, as well as to me, together with the difficulties, wherewith each of them ſeems to be incumbered, I would acquaint you with what thoughts, and trials, occurred to me about theſe, and the like conceits. But I not daring to think this could prove other than a needleſs work, I muſt remember, that my buſineſs, in this paper, is to propoſe difficulties, not the ways of ſolving them; it being, from your kindneſs and ſagacity, that theſe are as well expected, as deſired, by,

SIR,

Your, &c.

AN ATTEMPT

To Manifeſt and Meaſure the

GREAT EXPANSIVE FORCE

OF

FREEZING WATER.

CONSIDERING, when I writ the hiſtory of cold, that though divers phænomena might induce an attentive obſerver to think, that freezing water had an expanſive force, yet I had not met with any, that endeavoured, or even propoſed, to meaſure it, whether, becauſe they reflected not on it at all, or judged not the force conſiderable; I, who looked with other eyes upon it, thought fit to repair that omiſſion, but was then ſo ill furniſhed with requiſites for doing it fully, that, I remember, I complained of it in my hiſtory of cold. And though, even afterwards, when the time of the year was favourable, I could not procure ſuch accommodations, as my deſign exacted; yet, thinking an imperfect way of meaſuring to be better than none, I preferred, to the making no attempt at all, the endeavouring to do what the leaſt defective inſtruments, I could procure, would permit me, towards the making an eſtimate by known meaſures, of the expanſive power of freezing water. For though I did not expect I ſhould be able accurately to define it, yet I hoped I ſhould make ſuch an eſtimate, as to know, that force not to be, as one would think it, faint and contemptible, but very great and conſiderable.

I remember on this occaſion, that to manifeſt the force of freezing water, I cauſed the barrel of a ſhort gun to have a ſcrew fitted to the noſe of it, by which we might exactly ſtop it, as we did the touch-hole another way; then filling the barrel with common water, and cloſing it accurately by the help of the ſcrew, we laid it in a conveniently-ſhaped veſſel, wherein we encompaſſed it with a frigorifick mixture (of ſnow, or ice, and ſalt,) and, in a ſhort time, we found, as we expected, the barrel to be burſt, part of the ice appearing along the gaping ſlit, that had been made in the body of the iron by the freezing water, which, by this effect, ſeemed to emulate the juſtly-admired force of kindled gun-powder. But the deſign of this ſhort paper tending not ſo much to prove, as (in ſome ſort) to meaſure the expanſive force of water, I ſhall ſubjoin the tranſcripts of two or three experiments, made chiefly for that purpoſe.

EXPERIMENT I.

[THERE was taken a ſtrong cylinder of braſs, whoſe cavity was two inches in diameter; into this was put a bladder of a convenient ſize, with a quantity of water in it, that the neck of the bladder (which I had taken care to have oiled) being ſtrongly tied, the water might not get out into the cavity of the cylinder, nor be capable of expanding itſelf ſome other way, than upwards. Then into this cylinder was fitted a plug of wood, turned on purpoſe, which was ſomewhat leſs in diameter than the cylindrical cavity, that it might riſe and fall eaſily in it. Upon the upper part of this plug was laid a conveniently ſhaped flat body, upon which were placed divers weights to depreſs the plug, and hinder its being lifted up by the expanſion wont to be made in water, that is made to freeze: then a frigorifick mixture being afterwards applied to the cylinder, it appeared, within half an hour, or ſomewhat more, by a circle, that had been purpoſely traced on the ſide of the plug, where it was almoſt contiguous to the orifice of the cylinder, that the water in the bladder began to expand itſelf, and about two hours after, having occaſion to ſhew the experiment to ſome inquiſitive perſons, the circle appeared to have been heaved up, in my eſtimate, about ⅜, if not half, of an inch, notwithſtanding all the weights, that endeavoured to hinder the aſcenſion, though theſe weights amounted to 115 pound, which were all the determinate weights we could then procure, beſides a brick, and ſome other things, that were eſtimated at five pound more; nor did I doubt, that a far greater load would not have hindred its expanſion.]

EXPERIMENT II.

[WE took a braſs cylinder, whoſe dimenſions were three inches eight tenths in diameter, and in depth four inches. Into this we put a fine bladder of a convenient ſize, almoſt filled with water, and ſtrongly tied about the neck; upon this bladder we put the wooden plug to ſtop up the orifice, as much as was convenient, and upon the plug we put a piece of a flat board for the weights to ſtand upon. Theſe things being prepared, we conveyed the cylinder, with all, that belonged to it, ſave the board, into a large wooden bowl, where we applied to the cylinder a good quantity of the frigorifick mixture, made with beaten ice and bay ſalt; and having firſt marked with a circular line the edge or contact, where the orifice, or lip of the cylinder, touched the plug, we laid on the weights upon the board; and when by their weight they had depreſſed the plug till the cover of it leaned upon the cylinder, we diſpoſed ourſelves to attend the iſſue of the trial. The event whereof was this, that when the action of the frigorifick mixture had produced ſome ice in the water included in the bladder, that liquor appeared to have dilated itſelf ſtrongly enough to begin to raiſe the plug with the ſuper-incumbent weights, and by degrees they were, by the growing ice, raiſed, till the mark, diligently made on the plug, where the edge of the cylinder touched it, was about a tenth part of an inch above the ſtation it had before the plug had been depreſſed. Then we took out the bladder, and found the cylinder of water within the bladder not to be wholly turned into ice, but to contain ſome quantity of unfrozen water in the parts about the centre, which liquor, if we had not ſo ſoon deſiſted from the experiment, (as for certain reaſons we did) might probably have raiſed the weights ſomewhat higher. But as it was, the ice in length was but three inches and about ⅛, and yet ſo ſmall a quantity of ice ſufficed to raiſe, beſides the board they leaned on, as many weights of lead, as amounted to an hundred pound averdupois.]

EXPERIMENT III.

[THE day after the above-mentioned experiment was made, to try yet farther the expanſive force of freezing water, the ſame was reiterated after the manner above delivered, but with this difference, that having procured more weight, when the plug was lifted up ⅜, or ſomewhat better, (which plug began ſenſibly to riſe within half, or three quarters of an hour, after the frigorifick mixture was applied) it was loaded with a weight of two hundred pounds, and a fifteen pound piece of lead, and other bodies, as boards, &c. to lay the weights upon, which being alſo weighed by themſelves, came to fifteen pound more, ſo that the whole amounted to two hundred and thirty pound; and if the hundred pounds were both of them, as their bulk and ſhape invited us to gueſs, of that ſort of weights, which are called the great hundred, containing an hundred and twelve pound a-piece, twenty-four pound muſt be added to the ſum, which would thereby be made up two hundred and fifty-four pound.]

A
NEW EXPERIMENT
ABOUT THE
PRODUCTION of COLD
By the CONFLICT of BODIES, appearing to make an EBULLITION.

AND now, that we are ſearching after the nature of cold, I am put in mind, that I have ſometimes wondered at a certain experiment, that is ſo anomalous, and ſeems ſo little of kin to the uſual phænomena of cold, that though I do not particularly teach the way of making it, becauſe I could not do it without diſcovering ſomething in chemiſtry, that cogent conſiderations forbid me at preſent to publiſh; yet I cannot forbear to relate, on this occaſion, the matter of fact, both becauſe it may afford conſiderable hints to ſagacious enquirers, and becauſe it ſeems ſo little congruous to moſt theories of the cauſes of cold, that it may make the framers of theories more wary, and help alſo to excuſe my backwardneſs to propoſe hypotheſes about cold in a reſolute and confident way.

THE experiment is this: We took three ſaline bodies, each of them purified by the fire; and whereas there are divers bodies, that being mingled together acquired a heat, which neither of them had apart; and whereas it is ſaid by ſome, that there are a few, which being blended together, make a mixture ſomewhat colder than either of themſelves, theſe ſalts of ours being put together in due proportion, do upon their mixture produce that, which the eye judges to be a great effervеſcence; but though the hiſſing noiſe be loud, and though the numerous bubbles ſuddenly generated will make the matter apt to overflow the glaſs, if the one be not capacious, and the other be not put in by little and little; yet even whilſt this ſeeming ebullition laſts, the glaſs, which one would expect to find very hot, (as uſually happens upon the mixture of the ſalt of tartar, and ſpirit of nitre, and upon the confuſion of the like ſaline bodies diſpoſed to produce together ſuch efflореſcencies) inſtead of growing hot, does, if it be held in one's hand, feel much cooler than before, and that in a wonderful degree; inſomuch, that even in winter the outſide of the glaſs would quickly be covered with great drops of dew, which after a while would unite, and trickle down by their own weight. And this we could make to laſt for a great while, by caſting in by degrees more and more of one of the ingredients on the other. And beſides that, this copious dew on the outſide of the glaſs, reached as high as the mixture within, which argued whence it proceeded; beſides that, purpoſely looking on the bottom of the glaſs, whoſe outſide was concave, we found no ſuch drops of dew there, becauſe the vapours of the external air could not, in any quantity, have acceſs to it; which ſhewed the dew, conſpicuous elſewhere, not to come from the tranſudation of the finer parts of the mixture through the pores of the glaſs: beſides theſe things, I ſay, I remember, that having ſometimes purpoſely wiped off the dew here and there with my handkerchief, the dry parts of the glaſs would in no long time regain freſh drops of dew. And this odd experiment we did for the main repeat, not only in the preſence of an induſtrious chemiſt, (whoſe trials unexpectly gave us the riſe of the experiment,) but alſo alone, and at differing ſeaſons of the year.

I ſhall add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the experiment, becauſe I could then make uſe of a ſealed weather-glaſs, which I had not at hand when I made the former trials; I took two deep glaſſes, into the one of which I put a good quantity of fair water, and in the other I made ſuch a mixture, as I was lately mentioning; and having by a ſtring, (to prevent the altering of the temper of the included air by the warmth of my fingers) let down the weather-glaſs into the water, that the liquor ſhut up in the inſtrument might be cooled by the ambient water; after it had ſtaid there a reaſonable time, I took it out, by the ſtring, that was faſtened to the upper part of it, and letting it down into the mixture, that was then hiſſing, and filling the veſſel, that contained it with multitudes of ſucceſſively emerging and haſtily vaniſhing bubbles; I perceived nevertheleſs, that the coldneſs of the ſeemingly effervеſcent mixture made the impriſoned tincted liquor to ſubſide ſo low, that from four inches and three quarters (or thereabout) at which height it ſtood in the carefully divided ſtem, when the weather-glaſs was taken out of the water, it fell in a ſhort time lower than to one inch and an half. And becauſe I foreſaw, that this might ſeem ſcarce credible, eſpecially if I ſhould relate, how ſwiftly the impriſoned liquor ſubſided at the beginning; I ſhall annex, that, for farther ſatisfaction of others, I removed the thermometer out of the mixture into the common water again, where it ſoon reached to ſomewhat above four inches and a half; and not content with that, I put it a ſecond time into ſome of the frigefactive mixture before it had done foaming, in which it fell, as before, ſomewhat below an inch and a half, and, preſently after, almoſt as low as to an inch. And having once more put it back into the glaſs, that contained the water, the included liquor re-aſcended to above four inches and a half, and this in an excellent ſealed weather-glaſs, whoſe ſtem was not in all above ten inches long, with a ball proportionably big. And for farther confirmation, I took notice, that, whilſt the mixture, by its hiſſing noiſe, and its ſtrangely numerous bubbles, ſeemed to be in a ſtate of ebullition, the outſides of the glaſs, that contained it, were, as far as the mixture reached, ſo plentifully bedewed with the condenſed vapours of the ambient air, that their weight carried them down in little ſtreams, which left round about the bottom of the veſſel a pretty quantity of liquor, that appeared by its taſte not to have been made by the tranſudation of any of the ſharp and ſaline liquors, that were agitated within the glaſs. There remained only one ſcruple, which was ſuggeſted to me by the remembering of a circumſtance, which however, at the making of the fore-mentioned trials, I had not minded, and which poſſibly moſt obſervers would have neglected; but calling to mind, that the water, I had made uſe of to immerſe the weather-glaſs in, was brought out of a room, wherein a fire was wont (though not conſtantly) to be kept, whereas the ingredients of the mixture were kept, and put together in a chamber, which, though contiguous to the former, had no chimney in it; I thought fit, for greater circumſpection ſake, to let the water ſtand all night in this laſt-mentioned chamber, that the ambient air might have the ſame operation upon it, as upon thoſe bodies, that were to be ingredients of the mixture: and then repeating the formerly recited experiment, though I thought it needleſs to ſpend time to watch, as before I had done, the greateſt difference in cold betwixt the water and the bubbling mixture; yet by making removes of the weather-glaſs to and fro, from one liquor to another, it ſufficiently appeared, that the greater coldneſs, remarkable in the mixture, did not before proceed in any conſiderable degree (if in any degree at all) from the water's not having been kept in the ſame room with it.

So that by theſe different trials it ſeems manifeſt, that the coldneſs of the mixture was not a deception of the ſenſory, ſince it would be diſcovered by the operation it had, not only upon the vapours of the air on the outſide of the glaſs, but upon the thermometer itſelf, placed in the midſt of the mixture, which this laſt named circumſtance argues to have been cold throughout, and even in its innermoſt parts.

And to ſhew, how much this ſtrange coldneſs depended upon the peculiar texture of the mixture, or the ſtructure of its component corpuſcles, and the peculiar kind of motion, that was excited in the tumultuating particles; I ſhall here ſubjoin a relation, which probably will not appear deſpicable; namely, that in the firſt place I took ſome of the acid liquor, the reſt of which I had made uſe of to make the mixture, whereof I have been ſpeaking; and put a convenient quantity of fair water, which had been kept a night or two, in the ſame room (wherein was no chimney) with it, that there might be no cauſe of ſuſpicion, that the one had been expoſed to a more or leſs cold air than the other; and yet theſe two liquors did ſcarce ſenſibly differ in coldneſs; though to diſcover whether they did or no, I removed from one to another of them a good ſealed weather-glaſs, with a very ſlender ſtem.

And in the next place, I took a convenient quantity of the pure ſalt, I had ſo often employed, and caſt it into a glaſs full of water, which I had kept many hours in the ſame room with it, and wherein I had a little before placed a ſealed weather-glaſs, that the included liquor might be brought to the temper of the ambient liquor; but upon this injection, the tincted liquor of the thermoſcope ſubſided ſo little, as not to make me look upon this ſalt as being itſelf extraordinarily cold, ſince other obvious ſalts (that I have at other times caſt into water to cool it a little) and even ſea-ſalt would (according to my eſtimate) have refrigerated it as much, if not more. Nor did I obſerve the glaſs, wherein I was wont to keep ſtore of our ſalt, (though I had often occaſion to handle it) diſcloſe to the touch any remarkable degree of coldneſs; ſo that the coldneſs of our hiſſing mixture could not be attributed to that of either of the ingredients apart, but was a quality emerging upon their being blended. Now, when I thus made theſe preparatory trials, having afterwards placed in the ſame window (of the chamber laſt mentioned) a couple of glaſſes, with common water in one, and in the other ſome of that mixture, of whoſe frigefactive power I had very recently made trial; I left them to ſtand there together all night, and left alſo ſtanding by them ſuch a ſealed weather-glaſs, as I have been mentioning; and the next morning, when all the viſible commotion or agitation of the minute parts of the contrary ſalts of the mixture was quieted, I put the weather-glaſs, firſt, into one of thoſe two liquors, and then into the other, and after removed it back into the former again, without perceiving any difference worth minding, betwixt the coldneſs of the mixture, and that of common water: and with much the like ſucceſs I repeated the trial, after the water, and the other liquor had ſtood in the ſame room (unfurniſhed with a chimney) for near two days and nights.

And for farther confirmation, I ſhall add, that having inſtead of the ſalt, which I hitherto made uſe of, taken ſome of the ſpirit, that was wont to come over together with that ſalt, and did ſo abound with it, that a good deal of it lay undiſſolved at the bottom of the liquor; having, I ſay, employed this ſaline ſpirit, inſtead of the ſalt itſelf, and having for trial's ſake mixed with it another ſpirit, drawn in my own laboratory for the purpoſe, which to me ſeemed as like, as could be made, to that, which I had all this while made uſe of; I found, that the mixture of theſe two liquors, though it produced far fewer bubbles than I was wont to have, inſtead of growing cold, grew lukewarm, and quickly impelled the liquor in the weather-glaſs, from a little above three inches, to as much above eight; and yet, beſides that this laſt ſpirit was, as far as I could perceive, and that after the ſame manner, drawn from the ſame materials with that I had uſed all this while; the ſmell and taſte, (which are both of them peculiar and odd enough) concurred to manifeſt the two ſpirits to be of the ſame kind.

And, for farther proof, I ſhall add, that to ſatisfy myſelf the more fully, I took a parcel of the ſame liquor, I had lately employed with ſucceſs in making the frigorifick mixture; and yet even this liquor, which with the dry ſalt would queſtionleſs have produced a frigefactive mixture, as well as the reſt had done, which I had a little before taken out of the ſame viol; this liquor, I ſay, put to a new portion of the ſaline ſpirit above-mentioned, though they did not produce minute bubbles numerous enough to make a foam; yet the mixture, inſtead of growing very cold, grew manifeſtly lukewarm, not only in the judgment of the touch, but by its operation on a good ſealed weather-glaſs, carefully, and for a competent while employed to examine the temper of it. Whereas on the contrary, having purpoſely kept ſome of the frigorifick ſpirit by the fire-ſide, till its temper was ſo altered, that it nimbly enough rarified and impelled up the ſpirit of wine contained in a ſealed weather-glaſs, immerſed in it, and having into this liquor caſt ſome of the frigorifick ſalt, even whilſt the ſpirit of wine was riſing, and would probably have riſen a pretty while longer; this injected ſalt, when it began to be diſſolved, did not only give a check to the riſing liquor, and quickly put a ſtop to its aſcent; but, as I expected, ſoon made it ſubſide again, till it fell about three inches or more (which was very much in a ſhort weather-glaſs) beneath the ſtation where the ſpirit of wine had reſted, before the liquor was ſet by the fire ſide; nay, afterwards, I tried, that a frigorifick ſalt, being well warmed by the fire ſide, did, with an appropriated liquor, that was alſo warmed, produce a coldneſs manifeſtly perceivable by the weather-glaſs. So that in theſe caſes a body but moderately cold, nay, actually warm, haſtily reduced one, actually warm, or at leaſt tepid, to a far greater degree of actual coldneſs than itſelf had.

THESE are ſome of the experiments I tried with the liquors and ſalts, of which, upon allowable conſiderations, I muſt now forbear to ſet down the way of preparing: but, that even at preſent I may not be altogether wanting to the curious, I deviſed a way of making a ſuccedâneum to this experiment, which I ſhall here willingly annex, as that, which though it be much inferior to what I may one day be at liberty to acquaint the reader with; yet it will ſhew the main thing intended, by manifeſting, that cold may, by the mingling of bodies, be produced, or increaſed to a degree exceeding that of either of the bodies, that compoſed the mixture; and this, though at the ſame time a ſeeming effervеſcence be made by the bodies, that thus refrigerate each other.

I took then very good ſalt of tartar, and putting to it a convenient quantity of ſpirit of vinegar, I did, whilſt the mixture was hiſſing, (but ſeemed to the touch to have refrigerated the glaſs, that contained it,) immerſe into it the ball of a good ſealed thermoſcope, furniſhed with ſpirit of wine. And, though the weather-glaſs were not much above a foot long, yet the coldneſs of this mixture made the tincted liquor deſcend, haſtily enough, two inches and almoſt a half. And to ſhew farther, that this mixture was actually colder than cold water, removing the weather-glaſs out of the mixture into that liquor, the tincted ſpirit began to reaſcend, and that ſo nimbly, that in about three minutes (that the ball of the thermoſcope ſtayed under water) the ſpirit of wine had reaſcended about an inch and a half, if not more. And to try whether this coldneſs of the mixture did proceed from, or depend upon, ſome texture of the parts, that was not very permanent, and yet did not quite degenerate, immediately after the ingredients had ceaſed to work upon one another; I remember, that near an hour after the ebullition of the ſpirit and ſalt of tartar was over, the thermoſcope being removed out of the common water, where it had ſtood immerſed, into the mixture, deſcended about half an inch or more. For want of ſalt of tartar I could not begin the experiment anew, and ſo am not ſure it will always ſucceed uniformly*. But yet to give my ſelf what further ſatisfaction I could, by trying the ſame experiment in ſuch a way, as might diſcover, whether or no the phænomenon did not depend upon, or require ſome peculiar texture in the fixed ſalt, that had been employed; I took ſome alcali (made by diſſolving pot-aſhes in fair water, and reducing them by coagulation to a white ſalt,) and pouring ſpirit of vinegar to it, I found, that this mixture did not, whilſt it hiſſed, grow at all colder, but rather ſomewhat warmer. And, for farther ſatisfaction, immerſing into it the ball of the newly mentioned weather-glaſs, I found, that it aſcended in a ſhort time about an inch, and, being removed into the water, deſcended about half an inch; and by making removes of it from one of theſe liquors into the other, two or three times more, I found, that the ſpirit of wine did riſe and fall, according to what has been newly obſerved, but its motions upwards and downwards were both leſs than before, and more ſlow.

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* The Author's warineſs was not here amiſs, he having afterwards found, that this experiment did not always ſucceed.

OBSERVATIONS AND EXPERIMENTS ABOUT THE SALTNESS OF THE SEA.

The FIRST SECTION.

CHAP. I.

THE cauſe of the ſaltneſs of the ſea appears, by *Ariſtotle*'s writings, to have buſied the curioſity of naturaliſts before his time; ſince which, his authority, perhaps, much more than his reaſons, did, for divers ages, make the ſchools, and the generality of naturaliſts, of his opinion, till towards the end of the laſt century, and the beginning of ours, ſome learned men took the boldneſs to queſtion the common opinion; ſince when the controverſy has been kept on foot, and, for aught I know, will be ſo, as long as it is argued on both ſides but by dialectical arguments, which may be probable on both ſides, but are not convincing on either. Wherefore, I ſhall here briefly deliver ſome particulars about the ſaltneſs of the ſea, obtained by my own trials, where I was able; and where I was not, by the beſt relations I could procure, eſpecially from navigators.

FIRST then, whereas the Peripateticks do, after their maſter *Ariſtotle*, derive the ſaltneſs of the ſea from the aduſtion of the water by the ſun-beams, it has not been found, that I know of, that where no ſalt, or ſaline body, has been diſſolved in, or extracted by water expoſed to the ſun or other heat, there has been any ſuch ſaltneſs produced in it, as to juſtify the *Ariſtotelian* opinion. This may be gathered, as to the operation of the ſun, from the many lakes and ponds of freſh water to be met with, even in hot countries, where they lie expoſed to the action of the ſun. And as for other heats, having out of curioſity diſtilled off common water in large glaſs bodies and heads, till all the liquor was abſtracted, without finding, at the bottom, the two or three thouſandth part, by my gueſs, of ſalt, among a little white earthy ſubſtance, that uſually remained. And though I had found a leſs inconſiderable quantity of ſalt, which, I doubt not, may be met with in ſome waters, I ſhould not have been apt to conclude it to have been generated out of the water, by the action of the fire, becauſe, I have, by ſeveral trials purpoſely made, and elſewhere mentioned, found, that in many places, (and I doubt not, but if I had farther tried, I ſhould have found the ſame in more) common water, before ever it be expoſed to the heat of the ſun or other fire, has in it an eaſily diſcoverable ſaltneſs of the nature of common ſalt, or ſea-ſalt; which two I am not here ſollicitous to diſtinguiſh, becauſe of the affinity of their natures, and that in moſt places the ſalt, eaten at table, is but ſea-ſalt freed from its earthy and other heterogeneities, the abſence of which makes it more white than ſea-ſalt is wont to be with us. Theſe laſt words I add, becauſe credible navigators have informed me, that in ſome countries, ſea-ſalt, without any preparation, coagulates very white; of which ſalt I have had, (from divers parts) and uſed ſome parcels.

BUT ſome of the champions of *Ariſtotle*'s opinion are ſo bold, as to alledge experience for it, vouching the teſtimony of *Scaliger* to prove, that the ſea taſtes ſalter at the top, than at the bottom, where the water is affirmed to be freſh. But as for the authority of *Scaliger*, though I take him to be an acute writer, yet I confeſs, that, for reaſons elſewhere given, I do not allow it that veneration, which I find given it by very learned men; nor am I over prone, even as to matters of fact, to acquieſce in what he tells us, when he neither ſignifies, that he delivers things upon his own experience, or declares from what credible information from others he received them.

IT is true, that having often obſerved, that ſea-ſalt diſſolved in water is, upon the receſs of the ſuperfluous liquor, wont to begin its concretion, not as moſt other ſalts do, at either the lateral or lower parts of the veſſel, but at the top of the water, I will not think it impoſſible, that ſometimes in very hot climates, or weather, the ſea may taſte more ſalt at the top, than at ſome diſtance beneath it. But conſidering, how great a proportion of the ſalt common water is wont to be impregnated with, before it ſuffers ſaline concretions to begin, and how far ſhort of that proportion the ſalt contained in the ſea-water is wont to be, in ſo much, that about *Holland*, a Dutch geographer or two have not found it to amount to the proportion of one to forty; and I in *England* found it to be no more than I ſhall hereafter ſpecify; it ſeems not unlikely, that *Scaliger*'s[*] obſervation was well made, and it muſt be very unlikely, that it ſhould generally hold, if the ſaltneſs of the ſuperficial parts of the ſea be compared with that of the lower parts of it.

And yet I do not build my opinion wholly upon this argument of ſome modern philoſophers, that ſalt being a heavier body than water, muſt neceſſarily communicate moſt ſaltneſs to the loweſt parts.

For though this argument be a probable one, yet water being a fluid body, the reſtleſs agitation of whoſe corpuſcles makes them, and the corpuſcles they carry with them, perpetually ſhift places, whereby the ſame parts come to be ſometimes at the top, and ſometimes at the bottom; this conſideration, together with what was lately noted of the peculiar diſpoſition of diſſolved ſea-ſalt, to begin its coagulation upon the ſurface of the water, may make the argument, we are conſidering, ſuſpected not to be ſo cogent, as at firſt ſight, one may think it. Which ſuſpicion I might ſomewhat countenance by ſubjoining, that in divers metals, and other tincted ſolutions, I have not uſually obſerved the upper part of the liquor, to be manifeſtly deeper coloured than the lower; though, between metalline bodies, and their menſtruums, the diſproportion of ſpecifick gravity, does uſually much exceed that, which I have met with, between ſea-ſalt and common water.

CHAP. II.

It is urged out of *Linſchotten* by a learned modern writer, that wanting freſh water near *Goa* (the Metropolis of the Portugals in the *Eaſt-Indies*) they make their ſlaves fetch it, by diving, from the bottom of the ſea; which ſeems a clear evincement of the peripatetick opinion. But in this obſervation, I cannot acquieſce, for two reaſons: the one, becauſe, that though what is alledged, as matter of fact, were ſtrictly true, yet ſo general a concluſion could not be ſafely drawn from that particular inſtance, ſince in other parts of the ſea, the contrary has been found by experience, as I ſhall ſhew ere long. And other reaſons than thoſe given by the Peripateticks may be rendered of what happens at *Goa*, which reaſons may extend to the like caſes, if elſewhere they ſhall happen to be met with. For it may very well be, that ſprings of freſh water may ariſe in ſome parts of the ſurface of the earth, that are covered with the ſea, as they do in innumerable vallies and other places of the terreſtrial ſurface, that is not ſo covered, Not to mention thoſe ſprings, that appear in divers places upon a low ebb, covered with the ſea during the flood. The curious Hungarian[†] governor, that gives us an account of the wonderful waters, that ennoble his country, relates, that in the river *Vagus*, that runs by the fortreſs *Galgotium*, the veins of hot water ſpring up in the bottom of the river itſelf. Pag. 65. *Neque in ripa tantum*, ſays he, *eruuntur calidæ, ſed etiam intra amnem, ſi fundum ejus pedibus ſuffodias; calent autem immodicè*, &c. Nay, I have been aſſured by more than our learned eye-witneſs, that there is a place upon the Neapolitan coaſt, where they (and, I think, a writer or two of thoſe parts) obſerved the water to ſpring up hot beneath the ſurface of the ſea, in ſo much, that one of my relators thruſting in his hand and arm ſomewhat deeper than was convenient, found there an offenſive degree of heat.

Besides, (which is my ſecond conjecture) as to the particular caſe of *Goa*, I had the curioſity to enquire of a great traveller, and a man of letters, that lived in that city and the neighbouring places, and gave me a pertinent account of them, eſpecially of that place, whence the freſh water is fetched by the divers, which his curioſity led him to viſit, and take ſpecial notice of; but I found by him, that the divers do not now think it needful to fetch their freſh water ſo low as from the bottom of the ſea, and that, by the little depth, whence his and other men's curioſity cauſed it to be taken up, he judged, it did not ſo much come from any freſh water ſprings, riſing at the bottom of the ſea, as from a ſmall river (whoſe name I do not remember) that nor far from thence runs into the ſea, with ſuch a juncture of circumſtances, that at the mentioned places, the freſh water does yet keep itſelf tolerably diſtinct, and is not yet ſo far made brackiſh, as not to continue potable, though not very good. Which conjecture of his I could make probable, by what I have had from eminent and obſerving men among our own navigators, touching the ſliding of waters one over another, in ſome parts of the ſea, eſpecially near the mouths of rivers. But the diſcuſſion of this matter, and the particulars of the account given me of the ſituation of the place, where water is dived for near *Goa*, would require more words, than they would in this place deſerve, unleſs the point under debate were more important to our preſent purpoſe.

I might here pretend to a clear demonſtration, by experience, of the contrary of what *Scaliger* delivers, by vouching the teſtimony of the learned *Patricius*, who affirms, that being upon the ſea, which takes its denomination from the iſland of *Crete* (now *Candia*,) he did, in the company of a Venetian magiſtrate, *Mocinigo*, let down a veſſel (furniſhed with a weight to ſink it) to the bottom of the ſea, where, by the help of a contrivance, it was unſtopped, and filled with water there, which being drawn up, was found to be not freſh, but ſalt. This experiment, I ſay, I could oppoſe as a demonſtration againſt *Scaliger*; but though it be a very probable argument, and more conſiderable than any I have ſeen brought by the Peripateticks for their opinion, yet I confeſs, it would be more ſatisfactory to me, if it would not permit me to ſuſpect, that in the drawing up of the veſſel through the ſalt water, though there had been freſh water taken in at the bottom, the taſte may have been altered by the ſubingreſſion of ſalt water, which being bulk for bulk heavier than freſh, would by its ponderouſneſs endeavour to ſink into the aſcending veſſel, and thereby more eaſily expel part of the freſh water, and mingle with the reſt. Wherefore, I ſhall confirm the ſaltneſs of the ſea at the bottom by ſome obſervations, that are not liable to the ſame objections as that of *Patricius*.

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[*] See the third Section, towards the latter end.

[†] De admirandis Hungariæ Aquis.

The firſt is that of the perſon, whom I elſewhere mention, to be able, by help of an engine, to ſtay a conſiderable time at the bottom of the ſea; for of him I learned, among other things, that I deſired to be informed of touching that place, that he found the water to have as ſalt a taſte there, as at the top.

The next obſervation I obtained by means of a great traveller into the *Eaſt* and *Weſt-Indies*, who having had the curioſity to viſit the famous pearl-fiſhing at *Manar*, near the great *Cape* of *Comori*, anſwered me, that he had the ſame curioſity, that I expreſſed to learn of the divers, whether they found the water ſalt at the bottom of the ſea, whence they fetch their pearl-fiſhes? and that he was aſſured by them, that it was ſo: and the ſame perſon being aſked by me about the ſaltneſs of the ſea in a certain place under the Torrid Zone, which the relation of a traveller inclined me to think to abound extraordinarily with ſalt, affirmed to me, that not only the divers aſſured him, that the ſea was there exceeding ſalt at the bottom, but brought up ſeveral hard lumps of ſalt from thence, whereof the fiſhermen, and others, were wont to make uſe to ſeaſon their meat, as he himſelf alſo did; which yet I may aſcribe not only to the plenty of ſalt already diſſolved in the water, but to the greater indiſpoſition, that ſome ſorts of ſalts, whereof this may be one, have to be diſſolved in that liquor.

To theſe I ſhall add this third obſervation: meeting with an inquiſitive engineer, that had frequented the ſea, and had ſeveral opportunities to make obſervations of other kinds in deep waters, I deſired him, that he would take along with him a certain copper veſſel of mine, furniſhed with two valves opening upwards, and let it down for me the next time he went to ſea; on which occaſion he told me, that, if I pleaſed, I might ſave myſelf the trouble of the intended trial, for, with a tin veſſel, very little differing from that I deſcribed unto him, he had had the curioſity, near the ſtraight of *Gibraltar*'s mouth, (where he had occaſion to ſtay a good while) to fetch up ſea-water from the depth of about forty fathom, and found it to be as ſalt in taſte as the water near the ſurface.

These obſervations may ſuffice to ſhew, that the ſea is ſalt at the bottom in thoſe places where they were made; but yet I thought it was not fit for me to acquieſce in them, but rather endeavour to ſatisfy myſelf, by the beſt trial I could procure to be made with my copper veſſel, (as more ſtrong and fit than a tin one,) what ſaltneſs is to be found in the water at the bottom of our ſeas, not only, becauſe it may more concern us to know that, but chiefly, becauſe, though I deny not, that in the fore-going obſervations the taſte may ſufficiently prove, that the ſea is ſalt at the bottom as well as the top, yet I thought the taſte, by reaſon of the prediſpoſitions, and other unheeded affections, it is liable unto, no certain way to judge, whether the top and the bottom be as ſalt one as the other. Wherefore, I thought it would be more ſatisfactory to examine the ſea-water by weight, than by taſte; and in order thereunto, having delivered the above mentioned inſtrument to the engineer I lately ſpake of, when he was going to ſea, he ſent me, together with it, a couple of bottles of ſea-water, taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour, and ſmell of theſe two waters were ſomewhat differing; but when I examined them hydroſtatically, by weighing a roll of brimſtone firſt in one, and then in the other, I ſcarce found any ſenſible difference at all in their ſpecifick gravities. So that if the degree of the ſaltneſs of ſea-water may be ſafely determined by its greater or leſſer weight, then ſo far forth as this ſingle experiment informed me, the ſaltneſs is equal at the top and bottom of the ſea: I ſaid, if the degree, &c. becauſe of what I ſhall hereafter take notice of about ſalts of leſs ſpecifick gravity than ſea-ſalt.

CHAP. III.

It follows now, that I make out, what I formerly intimated, that though it were granted, that near *Goa*, and perhaps in ſome other places, the divers may have found the water freſh at the bottom of the ſea, it would not therefore neceſſarily follow, that the ſea-water, generally ſpeaking, is freſh at the bottom; for the obſervations lately mentioned ſufficiently manifeſt the contrary: and as to thoſe very few places (if really there have been any) where the ſea-water has been found freſh at the very bottom, I think one may aſcribe the taſte of the water to the bubbling up of ſprings of freſh water, at, or near enough to thoſe very places. I know this may appear a paradox, ſince it may ſeem altogether unlikely, that ſo ſmall a ſtream of water, as can be afforded by a ſpring, ſhould be able to force its way up in ſpite of the reſiſtance of ſo vaſt a weight, as that of the ſuper-incumbent ſea-water, eſpecially ſince this liquor, by reaſon of its ſaltneſs, is heavier in ſpecie than freſh water.

But this objection needs not oblige me to forſake my conjecture; for whatever moſt men believe, and even learned men have taught, to the contrary, it matters not how great the quantity of liquor be, which is laterally higher than the lower orifice of the pipe, or channel, that gives paſſage to the liquor, that is to be impelled up into it; provided the upper ſurface of the liquor in the channel, or pipe, have a ſufficient perpendicular height in reference to that of the ſtagnant water; for no more of all this fluid will hinder its aſcent, than the weight of ſuch a pillar of the ſaid fluid, as is directly ſuper-incumbent on it. [*] *Stevinus*, and I, have, by differing ways, particularly proved, that, according to the laws of the true hydroſtaticks, the prevalency of the two liquors, that preſs againſt each other, is not to be determined according to the quantity of them, but to be adjudged to that, which exceeds the other in perpendicular height; ſo that, conſidering the channel, wherein a ſpring runs into the ſea, as a long and inverted ſyphon, if that part of the either neighbouring, or more diſtant ſhore, whence the ſpring, or river, takes its courſe, be a neighbouring hill, or rock, or any other place conſiderably higher, than that part of the bottom of the ſea, or of the ſhore covered with the ſurface of the ſea, at which the channel, which conveys freſh water, terminates, that liquor will iſſue out in ſpite of the reſiſtance of the ocean.

To illuſtrate at once, and prove this paradox, I thought upon the following experiment. I took a veſſel of a convenient depth, and a ſyphon of a proportionable length, both of them of glaſs, that their tranſparency might permit us to ſee all that paſſed within them. Into the larger veſſel we put a quantity of ſea-water, and into the longer leg of the ſyphon, which had been for that purpoſe inverted, we poured a convenient quantity of freſh water, which we kept from running out at the ſhorter leg, by ſtopping the orifice of the longer with the thumb or finger: then this ſyphon being ſo placed in the greater veſſel, that the orifice of the ſhorter leg was a great deal beneath the ſurface of the ſalt water, and the ſuperficies of the freſh water in the longer leg was a pretty deal higher than that of the ſurrounding ſalt water, we unſtopped the orifice of the upper leg, whereby the water in the ſyphon, tending to reduce itſelf to an æquilibrium, or equality of height, in both legs, the water in the upper leg being much higher, and heavier, than that in the other, did, by ſubſiding, drive away the water in the ſhorter leg, and make it ſpring out at the orifice of the ſhorter leg, in ſpite of the breadth, and ſpecifick gravity of the ſalt water. And this impelling upwards of the freſh water laſted as long, as the ſurface of that water, in the longer leg, retained its due height above that of the ſurrounding ſea-water; which circumſtance I expreſly mention, becauſe there being a difference amounting to between a fortieth and fiftieth part, betwixt the ſpecifick gravity of our ſea water and common freſh water, by reaſon of the ſalt, which makes the former the heavier, the freſh water, in the longer leg of the ſyphon, ought to be between a fortieth and fiftieth part higher than the ſurface of the ſea water, to maintain the æquilibrium betwixt theſe two liquors.

To make the fore-mentioned experiment the more viſible, I thought fit to perform it with freſh water tinged with braſil, or logwood; but that it might not be objected, that thereby the ſpecifick gravity of the liquor would be altered, or increaſed, I afterwards choſe to make it with claret-wine, which being a liquor lighter than common water, and of a conſpicuous colour, is very convenient for our purpoſe.

And when I made this trial, by placing the orifice of the ſhorter leg at a convenient diſtance below the ſurface of the ſea-water, it was not unpleaſant to obſerve, how, upon the removal of the finger, that ſtopped the orifice of the longer leg, the quick deſcent of the wine contained in that leg impelled the coloured liquor in the ſhorter leg, and made it ſpring up, at its orifice, into the incumbent ſea-water, in the form of little red clouds, and ſometimes of very ſlender ſtreams. And as this ſhorter leg of the ſyphon was raiſed more and more towards the ſurface of the water, ſo there iſſued out more and more wine at the orifice of it; the liquor in the longer leg proportionably ſubſiding, but yet continuing manifeſtly higher than the ſurface of the ſalt water, than which it was in ſpecie much lighter.

¶ But here I muſt give an advertiſement to prevent a miſtake; for if the ſyphon be not exceeding ſlender, after the wine in the longer leg is fallen down to its due ſtation, a heedful obſerver may perceive, after a while, that though the ſyphon be kept in the ſame place, there will iſſue out of the ſhorter leg a little red ſtream, which proceeds not from the former impulſe of the wine in the longer leg, but from the ingreſs of the ſea-water, which being much heavier in ſpecie than wine, ſinks into the cavity of the ſyphon, and as it comes in on one ſide, thruſts up as much wine on the other ſide of the ſame cavity. But the red liquor, that aſcends on this account, may be diſcerned to do ſo, by its riſing more ſlowly, and after another manner, than that, which is impelled up by the ſudden fall of the tall cylinder of wine in the longer leg.

The SECOND SECTION.

CHAP. I.

As to the cauſe of the ſaltneſs of the ſea, I therein agree with the learned *Gaſſendus*, and ſome other modern writers, that the ſea derives its ſaltneſs from the ſalt, that is diſſolved in it: but I take that ſaltneſs to be ſupplied, not only from rocks, and other maſſes of ſalt, which at the beginning were, or in ſome places may yet be found, either at the bottom of the ſea, or at the ſides, where the water can reach them, but alſo (to ſay nothing here of what may, perhaps, be contributed by ſubterraneal ſteams) from the ſalt, which the rains, rivers, and other waters, diſſolve in their paſſage through divers parts of the earth, and at length carry along with them into the ſea. For not only it is manifeſt enough, that ſeveral countries afford divers ſalt ſprings, and other running waters, that at length terminate their courſe in the ſea; but I have ſometimes ſuſpected, that very frequently the earth itſelf is impregnated with corpuſcles, or, at leaſt, rudiments of common ſalt, though no ſuch thing be vulgarly taken notice of. Which ſuſpicion may be confimed (to omit what I have elſewhere delivered on another occaſion) partly by the obſervation of ſome eminent chemiſts, who affirm themſelves to have found a not inconſiderable quantity of exceeding ſaline liquor upon the evaporation of large quantities of ſome waters, (for in ſome others I could not find it) and principally by the quantity of common ſalt, that is uſually found in the refining of ſalt-petre; though that be a ſalt, which Sir *Francis Bacon*, and other experienced writers, teach, that almoſt every fat earth, kept from the ſun and rain, and from ſpending itſelf in vegetation, will afford.

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[*] Vid. *Stevinum*, Prop. 10. Lib. IV. Statices. And ſee the author's Hydroſtotical Paradoxes.

BUT having, on another occaſion, ſufficiently ſhewed, † that the earth does abound with common ſalt, in many more places than are wont to be taken notice of; and that it is probable, that by maturation, or otherwiſe, ſalt may daily grow in the earth, it will not be neceſſary to add, in this place, any thing to what I have ſaid already to prove, that our common terreſtrial ſalt, being diſſolved, may ſuffice to make the ſea-water brackiſh; and the rather, if we call to mind what has been formerly ſaid about the poſſibility of ſprings riſing beneath the ſurface of the ſea, and of lumps of ſalt, that were taken up by divers, undiſſolved, at the bottom of the ſea; the ocean may receive ſupplies of ſalt from rocks, and ſprings latent in its own boſom, and unſeen even by philoſophers. And this may be one reaſon, I conceive, (for I deny not but that there may be others, as the very unequal heat of the ſun, &c.) why ſome ſeas are ſo much ſalter than others, or, at leaſt, why in ſome places the ſea-water may be much ſalter than in others.

AND as we have ſeen, that our common terreſtrial ſalt may be copiouſly enough communicated to the ſea, to impregnate it with as much ſaltneſs, as we obſerve it to have; ſo I do not ſee, that the difference between that ſalt and ſea-ſalt is ſo great, but that it may well be ſuppoſed to be derived from thoſe changes, that the terreſtrial ſalt may be liable to, when it comes into the ſea. For that the marine ſalt, and the terreſtrial, do very well agree in the main things, may be argued from the reſemblance both in ſhape, taſte, &c. that may be obſerved between the grains, that will be produced, if we expoſe each of them in a diſtinct glaſs to ſuch a heat, as may ſlowly carry off the ſuperfluous moiſture, and ſuffer them to coagulate into cubical, or almoſt cubical grains; and the leſſer differences, that may be met with between theſe two ſalts, may well enough be ſuppoſed producible by the plenty of nitrous, urinous, and other ſaline, to which, in ſome places, may be added, bituminous bodies, that by land-floods, and otherwiſe, are from time to time carried into the ſea, and by ſeveral things, that happen to it there, eſpecially by the various agitation it is put into by tides, winds, currents, &c. and (which I would by no means omit) by its being in vaſt quantities expoſed to the ſun and air.

CHAP. II.

WE may juſtly be the more careful to determine, whether the ſaltneſs of the ſea-water proceed from common ſalt diſſolved in it, becauſe if it appeared to be ſo, we might the more hopefully attempt to obtain by diſtillation ſweet water from ſea-water; ſince, if this liquor be made by the bare diſſolution of common ſalt in the other, it is probable, that a ſeparation may be made of them, by ſuch a heat, as will eaſily raiſe the aqueous parts of ſea-water, without raiſing the ſaline, whoſe diſtillation requires a vehement heat, as chemiſts well know to their coſt. And ſuch a method of ſeparating freſh water from that, which was ſalt, would make our doctrine of uſe, and be very beneficial to navigation, and conſequently to mankind. For in long voyages, it is but too common for the makers of them, to be liable to hazards and inconveniencies, for want of freſh and ſweet-water, whereby they are ſometimes forced to drink corrupt brackiſh water, which gives them divers diſeaſes, as particularly the ſcurvy, and the uſual effect of drinking ſalt-water, the dropſy. And ſeamen are wont to receive ſo many other incommodities by the want of freſh water, that, to prevent or ſupply it, they are oftentimes forced to change their courſe, and ſail ſome hundreds of miles to a coaſt, not only out of their way, but unſafe in it ſelf, and perhaps more dangerous, by being infeſted by pirates, or in the hands of enemies or ſavage people; by which means, they often loſe the benefit of their Monſouns, and much more eaſily other winds, and frequently their voyage. And theſe are inconveniencies, which might be in good meaſure prevented, if potable, and at leaſt tolerably wholeſome water could be obtained by diſtillation, in the midſt of the ſea it ſelf, to ſerve the ſea-men, till they could be ſupplied with naturally freſh water. To make ſome trials of this, I remember I took ſome Engliſh ſea-water, whence I was able to ſeparate betwixt a thirtieth and fortieth part of dry ſalt; and having diſtilled it in a glaſs head and body, with a moderate fire, till a conſiderable portion of it was drawn over, we could not diſcern any ſaltneſs in it by the taſte; and beſides that I found it ſpecifically lighter than ſuch water, as is daily drank by perſons of quality at *London*, I expoſed it to a more chemical examen, and did not by that find any thing of ſea-ſalt in it, though I have at ſeveral times, by the ſame way, manifeſtly diſcovered a ſaltneſs in in-land waters, that are drank obviouſly for ſweet waters. If I would have employed a ſtronger heat, and veſſels larger and lower, or otherwiſe better contrived for copious diſtillation, I might in a ſhorter time have obtained much more diſtilled water; but whether ſuch liquors will be altogether ſo wholeſome, experience muſt determine. Yet that ſea-water diſtilled even in no very artificial way, may be ſo far wholeſome, as not in haſte to be ſenſibly noxious, but at a pinch uſeful, at leaſt for a while, may be gathered from (what occurs to me ſince the writing of the laſt paper) the teſtimony of that famous navigatior, Sir *R. Hawkins*, who commanded a fleet in the *Indies* for Queen *Elizabeth*. For he, in the judicious account he gave the world of his voyage, wherein they were diſtreſſed, even in the admiral's ſhip, for want of freſh water, has this memorable paſſage, as I find it verbatim in our diligent *Purchas**.

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† In a Tract of Subterraneal Menſtruums.

"ALTHOUGH our freſh water had failed us
" many days (before we ſaw the ſhore) by rea-
" ſon of our long navigation without touching
" any land, and the exceſſive drinking of the
" ſick and diſeaſed (which could not be excu-
" ſed;) yet with an invention I had in my ſhip,
" I eaſily drew out of the water of the ſea ſuf-
" ficient quantity of freſh water, to ſuſtain my
" people with little expence of fuel: for with
" four billets I ſtilled a hogſhead of water, and
" therewith dreſſed meat for the ſick and
" whole. The water ſo diſtilled we found to
" be wholeſome and nouriſhing."

AND becauſe the potableneſs of ſea-water may concern the healths and lives of men, I ſhall here add to what I elſewhere deliver about my ways of examining, whether other waters participate of ſalt, two or three obſervations I made upon thoſe few diſtilled liquors, I had occaſion to draw from ſea-water. Having then upon ſome of the diſtilled liquor dropped a little oil of tartar per deliquium, I perceived no clouds at all, or precipitation to be made; whereas a ſmall proportion of that liquor being dropped into the undiſtilled ſea-water itſelf, it would preſently trouble and make it opacous, and, though but ſlowly, ſtrike down a conſiderable deal of a whitiſh ſubſtance (which, of what nature it is, I need not here declare;) I found alſo, that a very ſmall proportion of an urinous ſpirit, ſuch as that of ſal armoniac, would produce a whitiſh and curled ſubſtance (but not near ſo copious a one as the other liquor) in ſea-water, not yet expoſed to diſtillation, but not in the liquor drawn from it: which argued, that there were but few or no ſaline particles of ſea-ſalt aſcended with the water: for elſe theſe alkalizate and urinous ſalts would in all likelihood have found them out, and had a viſible operation on them. And I farther remember, that when the diſtillation was made in glaſs veſſels, with an eaſy fire, not only the firſt running, but the liquor, that came over afterwards, was not perceived to be brackiſh, but good and potable. To which agrees very well, that by a hydroſtatical trial I found our diſtilled ſea-water to be lighter in ſpecie than common conduit water, though it exceeded that in ſpecifick levity, leſs than it was ſurpaſſed in the ſame quality by diſtilled rain-water.

BUT to return to the ſubject, whence we have ſomewhat, but, I hope, not uſeleſly, digreſſed; I know it may be objected, that if the terreſtrial ſalts carried by ſprings, rivers, and land-floods into the ſea, were the cauſe of its ſaline taſte, thoſe waters themſelves muſt be made ſalt by it, before they arrive at the ſea. But beſides that this objection will not reach the ſprings and rivers of ſalt water, that in ſeveral places, either immediately or mediately, diſcharge themſelves into the ſea; it might conclude againſt him, that ſhould affirm this imported ſaltneſs to be the only cauſe of that of the ſea; but it will not be of force againſt me, who take it to be only a partial cauſe, that by its acceſſion contributes to the degree of ſaltneſs we obſerve in the ſea, where this imported ſalt may join itſelf with the ſalt it finds there already, and being detained by it contribute to the brinyneſs of the water.

IF it be urged, that from hence it will follow, that the ſea, from time to time, increaſes in ſaltneſs, I may ſuſpend my anſwer, till it appear by competent obſervation, that it does not; which, I think, men have not yet made trials, that may warrant them to aſſert. And if the matter of fact were certain, I think it were poſſible to give a farther anſwer, and ſhew probable ways, how ſo ſmall an acceſſion of ſalt may be diſperſed by nature, and kept from increaſing too much.

CHAP III.

BUT now it is ſeaſonable to conſider, that the taſte of ſea-water is not ſuch a ſimple ſaline taſte, as ſpring-water would receive from ſal gem, or ſome other pure terreſtrial ſalt, diſſolved in it, but a bitteriſh taſte, that muſt be derived from ſome peculiar cauſe, that authors are not wont to take notice of. I am not aſſured by any obſervations of my own, that this receſſion, from a purely ſaline taſte, is likely to be of the very ſame kind, and to be equally, or very near equally, met with in all ſeas; (not to add a doubt, whether it be at all ſenſible in ſome.) The cauſe both of the bitterneſs; and ſaltneſs too, of the ſea-water, is ſaid to be affirmed by learned Mr. *Lidyat*, to be aduſt, and bituminous exhalations aſcending out of the earth into the ſea. But that there is abundance of actual ſalt in the ſea-water, to give it its ſaline taſte, and ponderouſneſs, the ſalt, that the ſun does in many places copiouſly ſeparate from the ſaltleſs wateriſh parts, ſufficiently manifeſts. But as to the bitteriſh taſte, I think it no eaſy matter to give a true account of it, but am prone to aſcribe it, partly, to the operation of ſome catholick agents upon that vaſt body of the ocean, and partly, to the alteration, that the ſalt receives from the mixture of ſome other things, among which bitumen may be one of the principal.

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* In lib. VII. p. 1378 of *Purchas*, out of Sir *R. Hawkins*'s voyage.

BUT though I have, in another * paper ſhewn, that in ſome places of the ſea there are conſiderable quantities of bitumen, or bituminous matter to be met with; yet I dare not derive the bitterneſs of the ſea only from bituminous exhalations, but in good part, at leaſt, in ſome places, from the liquid, and other bitumen, that is imported by ſprings, and other waters, into the ſea; of which we have an eminent inſtance in that, which our Engliſh call *Barbadoes* tar, according to the relation I had of it from an inquiſitive gentleman, who is one of the chief planters of the iſland, and took pleaſure to obſerve this liquid bitumen to be carried in conſiderable quantities from the rocks into the ſea; and I think it poſſible enough, that ſome of the ſprings, that riſe under the ſurface of the ſea, may carry up with them bituminous matter, which may help to make the ſaltneſs of the ſea degenerate, (of which more perhaps elſewhere;) as I not long ſince made mention of ſprings, as well of hot, as cold water, riſing beneath the ſurface of the ſea. And this minds me to intimate here, that I have ſuſpected, that in ſome places the ſulphureous exhalations, and other emiſſions from the ſubmarine parts of the earth, may ſometimes contribute to change the ſaline taſte of the ſea-water: for I have elſewhere related, how, not only ſulphurous ſteams, but ſometimes actual flames, have broken through from the lower parts of the ſea to the uppermoſt; and have ſometimes taken pleaſure to make, by art, a rude imitation of that phænomenon. And partly ſome experiments of my own, and partly ſome other inducements, have perſuaded me, that divers times (for I do not ſay always) ſea-ſalt does not obſcurely participate of combuſtible ſulphur, of which I may ſpeak farther on another occaſion. But in regard, that the taſte of the ſea-water is not in all parts of the ocean uniform, it may here ſuffice to take notice in general, that this difference of taſte may partly be cauſed by adventitious bodies of ſeveral kinds, of which it is probable, that, in differing places, the ſea-water does variouſly partake. And not to mention here the fragrant ſmell of violets, which has, by ſeveral, and particularly by an eminent perſon, of whom I enquired about it, been obſerved, in ſome hot countries, to proceed from ſea-ſalt; I have divers other inducements to think, that it is uſually no ſimple ſalt, nor free from mixture. For by more ways than one, and particularly by cohobating from it its own ſpirit, we have obtained a dry ſublimate, which ſeemed to be no pure, but a compounded body.

AND now to come to that, which I intimated might be one of the cauſes, why the taſte of ſea-water is not the ſame with that of common ſalt, diſſolved in freſh water; I ſhall add, that I have ſuſpected, that the various motion of the ſea, and its being expoſed to the action of the air and ſun, may contribute to give it a taſte other than ſaline; which ſuſpicion might be confirmed, by the obſervation I elſewhere mention of the ſea-ſalt, which, by barely being expoſed for many months to the air, and ſometimes perhaps put into a gentle agitation by a digeſtive heat, I found to have a very manifeſtly differing taſte from the ſimple ſolution of ſea-ſalt in common water.

I might here endeavour the farther confirmation of my diſcourſe, by what I have learned by enquiry from navigators, about the manifeſtly differing colours, and other qualities, of the differing parts of the ſea, which ſeem to argue, that it is not every where of ſuch a uniform ſubſtance as men vulgarly imagined, and that vaſt tracts of it are imbued with ſtupendous multitudes of adventitious corpuſcles, which, by ſeveral ways diverſifying its parts, keep it from being a ſimple ſolution of ſalt. But of this ſubject I have not leiſure to diſcourſe here; only becauſe it is generally thought, that the ſea-water is, by reaſon of the ſaltneſs it abounds with, uncapable of putrefaction, I will add, that having kept a pretty quantity of ſea-water, that I had cauſed to be purpoſely taken up between the Engliſh and French ſhores, in a good new rundlet, in a place, where the ſummer ſun beat freely upon it, it did, in a few weeks, acquire a ſtrongly ſtinking ſmell; though that the experiment had been more ſatisfactory, I wiſhed, that it had been made in a veſſel of glaſs, or earth, inſtead of wood. But a much better obſervation I procured from a much eſteemed navigator of my acquaintance, who having ſailed often in the Indian and African ſeas, I enquired of him, whether he had ever, in thoſe hot climates, where the ſea is ſuppoſed to be very ſalt, obſerved it to ſtink, for want of agitation, or otherwiſe: to which he anſwered, that once being, though it was but in *March*, becalmed, in a place he named to me, for twelve or fourteen days, the ſea, for want of motion, and by reaſon of the heat, began to ſtink, inſomuch that he thinks, if the calm had continued much longer, the ſtench would have poiſoned him: they were freed from it as ſoon as the wind began to agitate the water, and broke the ſuperficies, which alſo drove away ſtore of the ſea tortoiſes, and a ſort of fiſh, whoſe Engliſh name I know not, that before lay baſking themſelves on the top of the water.

AND to this agrees very well the notable obſervation, that I ſince met with, of the elſewhere commended Sir *R. Hawkins*, who, among other conſiderable things he takes notice of in his relations, has this paſſage, to our preſent purpoſe. † "Were it not for the moving of the ſea by the force of winds, tides, and currents, it would corrupt all the world. The experience I ſaw, *anno* 1590, lying with a fleet about the iſlands of *Azores*, almoſt ſix months, the greateſt part of the time we were becalmed; with which all the ſea became ſo repleniſhed with ſeveral ſorts of gellies, and forms of ſerpents, adders, and ſnakes, as ſeemed wonderful, ſome green, ſome black, ſome yellow, ſome white,

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* In the Tract of Subterraneal Menſtruums.

† *Purchaſe*'s Pilgrims in Sir *R. Hawkin*'s Obſervations.

"ſome of divers colours, and many of them "had life; and ſome there were a yard and a "half, and two yards long; which, had I not "ſeen, I could hardly have believed. And "hereof are witneſſes all the company of the "ſhips, which were then preſent, ſo that hardly a man could draw a bucket of water clear "of ſome corruption. In which voyage, towards the end thereof, many of every ſhip "fell ſick of this diſeaſe, and began to die "apace, but that the ſpeedy paſſage into our "country was a remedy to the crazed, and "a preſervative for thoſe, that were not "touched."

The THIRD SECTION.

CHAP. I.

As for the various degrees of the ſaltneſs of the ſea, authors are wont to be ſilent of it, ſave that ſome navigators tell us, that they obſerved ſome ſeas to have a more, and others a leſs ſaline taſte; which, you will eaſily believe, has not afforded me much ſatisfaction. And, on the other ſide, my want of opportunity to make trials myſelf will confine me to acquaint you with no more than the few following obſervations.

1. To a learned man, that was to ſail to places of differing latitudes in the Torrid Zone, I delivered a glaſs inſtrument, elſewhere deſcribed, fitted by the greater or leſſer emerſion of the upper part, to ſhew, accurately enough for uſe, the greater, or leſs ſpecifick gravity of the ſalt water it was put to ſwim in. This he put, from time to time, into the ſea-water, as he ſailed towards the *Indies*, whence he wrote me word, "That he found, by the glaſs, "the ſea-water to increaſe in weight, the "nearer he came to the line, till he arrived at "a certain degree of latitude, as he remembers, it was about the thirtieth; after which, "the water ſeemed to retain the ſame ſpecifick gravity, till he came to the *Barbadoes*, "or *Jamaica*.

2. Another obſervation I obtained by enquiry of an ingenious perſon, and a ſcholar, at his return out of the *Eaſt-Indies*, who affirmed to me, that he, and a gentleman of my acquaintance, took up bottles full of ſea-water, both under the Equinoctial, and alſo off the *Cape of good Hope*, which lies in about thirty-four degrees of ſouthern latitude, and found the waters of theſe diſtant parts of the ocean to be of the ſame weight. And though it may well be doubted, whether this obſervation, being made with ordinary bottles, were ſo exact as could be wiſhed; yet the perſons being curious, and making it for their own ſatisfaction; and my relator having, in both the recited places, filled with the ſea-water he took up, and weighed; having, I ſay, filled the ſame bottles, ſince this veſſel held two quarts (which muſt be above four pounds of ſalt-water) if the diſparity of weight had been conſiderable, it would, in likelihood, have been found, at leaſt manifeſtly ſenſible, in ſuch a weight of liquor.

3. Enquiring of an obſerving perſon, that had been at *Moſambique*, which is thought to be one of the hotteſt places in the world, whether he did not there find the ſea to be more than ordinarily ſalt? he anſwered me, that coming thither in a great carack, when he came back from the town to the ſhip, he obſerved near two hands breadth of the veſſel to be above the ordinary part, to which it uſed to ſink; inſomuch, that he took notice of it to the captain, as fearing, that part of the lading had been by ſtealth carried to the ſhore: but the pilot, who had made thirteen or fourteen voyages to the *Indies*, aſſured him, what he had obſerved about the ſhip, was not unuſual in that place, where the taſte itſelf diſcovered the water to be exceeding ſalt.

Nor need we ſcruple to think, that ſome ſea-waters may be very much more impregnated with ſalt than ours; for water will naturally diſſolve, and retain a far greater proportion of ſalt, than that, which is commonly met with in the ſea. For whereas a thirty-fifth, or thirtieth, or at moſt a twenty-fifth part of ſalt, will make water more ſaline than is found in many ſeas, I am, by a friend of mine, that is maſter of a ſalt work, informed, that the water of his ſprings afford him a twelfth part of good white ſalt, and that another ſpring, not far off, yields no leſs than an eighth part. To which (to avoid anticipation) I ſhall not here add, what I ſhall hereafter have occaſion to ſay of the fulleſt impregnation of water with common ſalt.

[Whilst I was reviewing theſe papers, there came ſeaſonably to my hands a letter written from *Muſilapatan*, on the gulf of *Bengala* in the *Eaſt-Indies*, by an ingenious gentleman, Sir *William Langhorn*, that is entruſted with the care of the Engliſh factories in thoſe parts; out of which letter the following paſſage is *verbatim* tranſcribed. "I did, in "order to your command, cauſe ſome water "to be ſaved under the line, at our firſt acceſs to it, intending, for want of good ſcales "and weights, (being none to be come at aboard the ſhip) to have kept it, until it could "be weighed, but by the forgetfulneſs of a "ſervant, it was thrown away. Off the *Cape*, "in 37 d. 00 m. ſouthern latitude, I ſaved "ſome again, and, through the ſame want of "weights, was fain to keep it, until I came to "the line again; and then made the beſt ſhift "I could for weights, and compared it with "the water there, filling the ſame bottle again to the ſame height by a mark, and "found it exactly the ſame weight. The "weight I have taken; but accounting this "a journey of buſineſs, left thoſe notes, and "moſt of the like nature, behind me; in my "next it ſhall be inſerted."]

CHAP. II.

IT remains now, that, according to my promiſe, I ſet down what I obſerved myſelf concerning the ſaltneſs of our ſea between *England* and *France*; not in compariſon with the ſaltneſs of other ſeas, whoſe waters I had not to compare with, but as to the proportion of ſalt contained in it to the water. And though one would think it very eaſy to make trials of this ſort, for a perſon not unacquainted with hydroſtatical practices, nor unfurniſhed with inſtruments, yet, I confeſs, that three or four trials that I made, not all of them the ſame way, made me find it more difficult, than was imagined, to arrive at any thing of certainty in this enquiry.

THIS you will eaſily believe, if I annex the ſubſtance of ſome experiments, that I remember I made about the gravity of ſea water, which I had ordered to be taken up, ſome at the depth of about fifteen fathom ſomewhat near our ſhore, and ſome in another place of the channel, between *England* and *France*.

THE ſum of the firſt experiment is this: we took a vial, fitted with a long and ſtrait neck, purpoſely made for ſuch trials, and having counterpoiſed it, filled it to a certain height with common conduit water: we noted the weight of that liquor, which being poured out, the vial was filled to the ſame height with ſea water, taken up at the ſurface, and by the difference between the two weights, the ſea water appeared to be about a forty-fifth part heavier than the other.

THE ſecond trial (which was for more accurately made hydroſtatically,) I find regiſtered to this effect: We carefully counterpoiſed in the ſcales, formerly made uſe of, a piece of ſulphur in the upper ſea-water, formerly mentioned; it weighed ʒß + *gr*. x. ß and being alſo weighed in the ſea water fetched from the bottom, gave us the ſame weight ʒß + *gr*. x. ß which ſhewed thoſe two waters to be of the ſame ſpecifick gravity: and then to compare this with the gravity of common water, we weighed the ſame ſulphur in common conduit-water, and found it ʒß + *gr*. xv. ß: by which it appeared, that the ſea-water was but about a fifty-third part heavier than this water: which is ſuch a difference from the proportion found out by the former way of trial, that I could not well imagine, what to attribute it to, unleſs the ſea-water by long ſtanding in a veſſel, which, though covered, was expoſed to the hot ſun, may both have been rarified, and have had ſome ſeparation made of its ſaline or other heavier parts, on which ſcore that portion we took up for our trial, might appear lighter than elſe it would have done; or unleſs the experiment having been made in *London*, where great and ſudden rains and other accidents will ſometimes viſibly vary the conſiſtence of common water, the liquor, I then employed without examining it, might be more ponderous at that time than at another. To which latter ſuſpicion I was the more inclined, becauſe, having afterwards weighed the ſame piece of ſulphur by help of the ſame balance in diſtilled rain water, I found the weight of the former liquor to exceed that of the latter by a good deal leſs than a thirty-fifth part; which ſeemed to make it probable, that if the water, we chanced to employ, had been free from all ſaline and other heavy particles, the difference formerly mentioned betwixt this obſervation and the foregoing would not have been near ſo great as it was.

THE laſt way I made uſe of to examine the proportion betwixt ſea-water and freſh, was chemical; whereof my regiſter affords me this account.

A pound avoirdupoiſe weight of the upper ſea-water was weighed out, and put into a head and body to be diſtilled in a digeſtive furnace *ad ſiccitatem*; and the diſtillation being leiſurely made, the bottom of the glaſs was almoſt covered with fair grains of ſalt, ſhot into cubical figures, and more white than was expected: in the reſt of the coagulated matter, we took not notice of any determinate ſhape. The ſalt being weighed, amounted to ʒß, averdupois, and *gr*. x. At which rate, the proportion of the ſalt to the water will be that of 30 and $\frac{72}{100}$ to one, and ſo will amount to near the thirtieth part; which was ſo much greater than the former ways of trial made us expect, that I know not, whether it may not be worth while to try, whether ſuch a ſlow abſtraction, as we employ of the ſuperfluous water, and our doing it in cloſe veſſels, may not have afforded us more ſalt, than elſe we ſhould have obtained.

To this relation I find this note ſubjoined: ſuſpecting, that there may have ſomewhat elſe concurred to our finding ſo great a proportion of ſalt, I ſuffered that, which had been weighed, to continue a-while in the ſcale, and ſoon perceived, that, according to my conjecture, that ſcale began manifeſtly to preponderate, and that conſequently ſome of the unexpected weight of ſalt may be due to the moiſture of the air, imbibed after the ſalt was taken out of the glaſs, and laid by to be weighed: wherefore, cauſing it to be very well heated and dried in a crucible, we found it to weigh ʒiij. + ß. (that is 210 grains,) upon which account the proportion of ſalt contained in the water, was a thirty-ſixth part, and ſomewhat above half of thoſe parts, and to expreſs it in the neareſt whole number, a thirty-ſeventh part.

FROM whence this greater proportion of ſalt by diſtillation, than our other trials invited us to expect, proceeded, ſeems not ſo eaſy to be determined; unleſs it be ſuppoſed (as I have ſometimes ſuſpected) that the operation the ſea-water was expoſed to in diſtillation, made ſome kind of change in it, other and greater than before-hand one would have looked for; and that, though the grains of ſalt we gained out of the ſea-water, ſeemed to be dry before we weighed it, yet the ſaline corpuſcles, upon their concreting into cubes, did ſo intercept between them many ſmall particles of water, as not to ſuffer them to be driven away by a moderate warmth; and conſequently ſuch grains of ſalt may have upon this account been leſs pure and more ponderous than elſe they would have been. And I might here add, that I ſometimes make a certain artificial ſalt, which, though being diſſolved in water, it will ſhoot into cryſtals finely ſhaped, and dry enough to be reducible into powder, yet coagulates water enough with it to make the water almoſt, if not quite, as heavy again as before. And I have been aſſured by a very learned eye-witneſs, that there is a ſort of ſea-ſalt, which they bring to ſome parts of *England*, from the coaſt of *Spain* or *Portugal*, which being here diſſolved, and reduced by purification and filtration to a much whiter ſalt, will yield by meaſure ſomewhat above two buſhels for one. But to ſatisfy the ſcruples and ſuſpicions I could ſuggeſt, would require more trials than I have now time or opportunity to make. What has been already delivered, may give at leaſt as ſcrupulous an account of the ſaltneſs of our Engliſh ſea-waters, as moſt other experimenters would have thought it needful to give. And to make a determination with any certainty about the degrees of the ſea's ſaltneſs in general, a great number of obſervations, made in different climates, and in diſtant parts of the ocean, would be neceſſary.

CHAP. III.

I KNOW not, whether I may be ſo indulgent to my ſuſpicions, as to wiſh, that obſervations were heedfully made, whether in the ſame ſea, and about the ſame part of it, the waters be always equally ſalt? For, though that be taken for granted, yet ſince we have no good obſervations long ſince made to ſilence the ſuſpicion, one may ſuſpect, that, at leaſt in many places, the ſaltneſs of the ſea may continually, though but very ſlowly, increaſe by the acceſſion of thoſe ſaline corpuſcles, that are imported by ſalt-ſprings, and thoſe, which rivers and land-floods do from time to time rob the earth of. And I ſuſpect it to be not impoſſible, that this, or that part of the ſea, may be ſometimes extraordinarily, and perhaps ſuddenly, impregnated with an additional ſaltneſs from ſaline ſteams plentifully aſcending into it, from thoſe ſubterraneal fires, about which I have made it elſewhere probable, that they may burn beneath the bottom of the ſea, and ſometimes ſend forth copious exhalations into it. But it may prove the more difficult to diſcern this adventitious ſaltneſs, unleſs the taſte, as well as balance, be employed about it; becauſe the ſalt, that produces it, may be of ſuch a nature, as to be much lighter in ſpecie than common ſea ſalt. And the mention of this leads me to give you here the advertiſement I promiſed you not long ago.

THAT though the weight of ſea-water be as good a way as is yet employed (and better than ſome others) to determine what ſea-water does moſt abound in ſalt; and though it be poſſible, that in our ſea, and perhaps, in almoſt all others, this way be not liable to any conſiderable uncertainty; yet I think it not impoſſible, that it may ſometimes deceive us, eſpecially in very hot regions; becauſe I have obſerved, that there may be volatile ſalts, which, though by reaſon of their activity, they make ſmart impreſſions on the tongue, and give the water imbued with them a ſtrong ſaline taſte, yet they add very little, and much leſs than one would think, to its ſpecifick gravity: as I have tried, by hydroſtatically examining diſtilled liquors, abounding in volatile and urinous ſalts, ſome of which I found very little heavier than common water, and conſequently nothing near ſo much heavier, as they would have been made, if they had been brought to ſo ſharp a taſte, by having nothing but common ſea ſalt diſſolved in them: ſo that, if in any particular place, by any other way, or from the ſteams of the earth beneath, ſome of which, I elſewhere ſhew, may be very analogous to thoſe afforded by ſal armoniack) the ſea ſhould be copiouſly impregnated with ſuch kind of light ſalts, the ſea-water may be much more ſalt to the taſte, and yet be very little heavier. For confirmation of which I find among my notes, that weighing a ſealed bubble of glaſs, made heavy by an included metal, firſt in ſpirit of ſal armoniack, that taſted much ſtronger than ſea-water, it weighed ʒiij + *gr.* 51 ¼, and weighing this ſame body in fair water, it weighed but ʒiij + *gr.* 45 ¾; ſo that notwithſtanding, its great ſaltneſs, the ſpirit was lighter than common water; though a good part of that comparative levity may probably be aſcribed to the liquor, wherein the ſaline particles ſwarm, which, by diſtillation, was grown more defecated and light, than common, though clean, water.

BUT for a farther proof, we took a hard lump of ſal armoniack; and though we could not weigh it in water, becauſe that would have diſſolved part of it, yet by a way (I elſewhere teach) I found, that weighing in the ſame liquor this lump of ſal armoniack, and a lump of good white ſea ſalt, (brought me as a curioſity out of the Torrid Zone) the proportion of the latter to a bulk of the liquor equal to it, was ſomething (though exceeding little) above that of two and a quarter to one, and the proportion of ſal armoniack to as much water, as was equal likewiſe to it, did not above a centeſm exceed that of one and 7/10 to one; which falls ſo ſhort of the other proportion, as may juſtly ſeem ſtrange, eſpecially if it be conſidered, that the factitious ſal armoniack, the chemiſts generally uſe, and we employ, conſiſts in good part of ſea ſalt, which abates much of the comparative levity it might have, if it were made up only of urinous and fuliginous ſalts, which were its other ingredients.

IT were indiſcreet for me to propoſe any more ſuſpicions and trials fitted to clear them, unleſs I knew thoſe I have already mentioned would not paſs for extravagancies; and therefore, I ſhall here diſmiſs the ſubject of this tract of the ſaltneſs of the ſea, but that, ſince I have been diſcourſing of the degrees of it, it will not be impertinent to add, what is the greateſt meaſure of ſaltneſs, that I have brought water to, without the help of external heat. On this occaſion, I employed two differing ways: the one was, by putting into a well-counterpoiſed vial two ounces of common-water, and then putting into it, well-dried and white, common ſalt, and ſhaking them together, till the liquor would, whilſt cold, diſſolve no more: this liquor, thus glutted with ſalt, weighed 1150 grains, from which two ounces being deducted, the overplus of weight, ariſing from the diſſolved ſalt, amounted to 190 gr. ſo that a parcel of ſalt will, without heat, be diſſolved in about five times its weight, or very little more, of common water. By which proportion we made ſo ſtrong a brine, that divers pieces of amber being purpoſely let fall into, emerged, and floated on it. The other and better way, yet more tedious, that we made uſe of, was to let ſea-ſalt run *per deliquium*, (as the chemiſts ſpeak) that is, to ſet it in ſome moiſt place, till it was diſſolved by the aqueous vapours, that ſwim in the air. In this liquor we weighed a piece of ſulphur, which we alſo weighed in ſea-water, wherein, finding it to weigh much more than in the former liquor, it appeared, that the ſea-water was, in ſpecie, much lighter than the other; though how much their gravities differed, I cannot find among my notes, nor be informed by my memory.

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* In the Tracts of Subterraneal Fires and Steams.

And becauſe I have not, in any author, met with the proportion of ſea-ſalt to water of the ſame bulk, nor perceive, that hydroſtaticians themſelves have yet attemped any way to inveſtigate it, (probably deterred by the eaſy diſſolubleneſs of ſalt in water,) I ſhall here ſubjoin, that by the help of an expedient, I have elſewhere taught, I have examined a hard dry lump of ſea-ſalt, and found its proportion in weight, to common water of the ſame bulk, to be almoſt as 2 to 1, (for it exceeded the *ratio* of 1 $\frac{9}{10}$ to 1.) And, I remember, I found the ſpecifick weight of a hard and figured lump of *ſal gem.* (which ſort of ſalt I ſuppoſe to be ſomewhat more pure and ponderous than ſea-ſalt) to be to that of water (very near) as 2 $\frac{1}{8}$ to 1.

THE FOURTH SECTION

Belonging to the TRACT, entitled,

Relations about the Bottom of the SEA.

The preſence of the air is not only ſo neceſſary to the life of many ſorts of animals, but it hath likewiſe ſo great a ſtroke in the growth of vegetables, eſpecially of the larger ſorts, that, after what I had experimented about theſe matters, (of which this is not the proper place to give an account) I thought fit to make enquiry about the vegetation and growth of plants of conſiderable bulk, in thoſe ſubmarine regions, where, if there grow any, they muſt do it remote from the free contact of an ambient air. And having not now the leiſure to repeat what botaniſts (of whoſe books I am not now provided) deliver about leſſer plants growing under water, I ſhall now only preſent you with what information I could procure from navigators, about trees and fruits growing at the bottom of the ſea.

To what I have elſewhere had occaſion to ſay to their opinion, that will not allow coral to be really a ſtony plant, but a liveleſs concrete, that is always hard and brittle under water; I ſhall now add, that, enquiring lately of an eminent and inquiſitive perſon, that had ſpent ſome time upon the coaſt of *Africa*, where he had been preſent at the fiſhing of coral, and learning from his anſwer, that he had ſeen it not far from *Algiers*; I aſked him, whether he had himſelf obſerved the coral to be ſoft, and not red, when it was newly brought from the bottom of the ſea. To which he replied, that he had found it ſoft and flexible; and that, as for the colour, it was for the moſt part very pale, but with an eye of red, the bark being worſe coloured than the ſubſtance it covered was; but when the bark was taken off, and the other part expoſed to the air, the expected redneſs of the coral diſcloſed itſelf.

When I demanded, whether he had obſerved, that any inky ſap aſcended to nouriſh the ſtony plant? and whether he had ſeen any thing like berries upon it? he ingenuouſly confeſſed to me, he had not been ſo curious, as purpoſely to make enquiry into thoſe particulars; but that he remembered, that having broken ſome of the large pieces of coral, he took notice, that the more internal ſubſtance was much paler than the other, and very whitiſh; and that at the extream parts of ſome branches, or ſprigs, he obſerved little blackiſh knobs, which he did not then know what to make of: and when I enquired, what depth the ſea was of in that place? he anſwered, that it was nine or ten fathom. But as to the fruit of ſome kinds of coral, if I do not much miſ-remember, I was, not long ſince, aſſured by a ſcholar, that navigated much in the eaſt, that they divers times meet with in thoſe ſeas a certain ſort of coral, but not white, which bears a ſmall fruit like a round berry, of a pleaſant colour, and eſteemed as rarities.

Discoursing with a perſon, that made diving his trade, whether he had not met with any trees or fruit in the depths of the ſea? he told me, that in a great ſhip, whereinto he deſcended, to recover thence ſome ſhipwrecked goods, he was ſurprized to find in ſeveral places a certain ſort of fruit, that he knew not what to make of, for he found them of a ſlimy and ſoft conſiſtence, about the bigneſs of apples, but not ſo round in ſhape; and when he brought them up into the air, as he did many of them, they ſoon began to ſhrink up like old rotten apples, but were much harder, and more ſhrivelled. And it is remarkable, that this happened in a cold northern ſea.

One, that made a conſiderable ſtay about *Manar*, a place I have often mentioned, anſwered me, that he learned from the divers, that in ſome places thereabouts there grows at the bottom pretty ſtore of a certain ſort of trees, bearing leaves almoſt like thoſe of laurel, as alſo a certain fruit; but of what virtue, or other uſe, he had not the curioſity to enquire.

I was alſo informed by an eye-witneſs, that near the famous coaſt of *Moſambique* in *Africa*, there grows at the bottom of the ſea ſtore of trees, that bear a certain fruit, which he deſcribes to be very like that, which, in *America*, they are wont to call *Acayu*, the leaves alſo reſembling thoſe of that tree.

But the beſt welcomeſt information I could procure about ſubmarine plants, is that, which concerns the famous Maldivian nut, or Cocoa, which is ſo highly eſteemed in the eaſt, that ſome write, it is a great preſent from one king to another, and even much extolled in *Europe* by experienced phyſicians: for the origin of this dear drug is almoſt as much converted, as the alexiterial virtues are extolled. Having then once the good fortune to meet with a man of letters, that had reſided in thoſe unfrequented iſlands, I found he had been as inquiſitive, as I could reaſonably expect, about theſe admired productions of the ſea, and that he had often learned from the divers, that they are real nuts, or fruits, borne by a ſort of cocoa-trees, that grow at the bottom of the ſea, and are thence, either torn off, by the agitation of the water, or gathered by the divers. Theſe fruits are ſmaller than moſt other ſorts of cocoa's, whoſe maturity they do not ſeem to arrive at. He thinks, the ſpecies may have been very differing from what it is, and may have come from nuts fallen into the ſea, together with the ruin of ſome little iſlands undermined by the water, and ſo ſubmerged; of which he told me, he ſaw, at leaſt, three or four inſtances during his ſtay there. He told me, that whilſt the fruit was under water, they obſerved no diſtinct ſhell and kernel, but the entire nut was ſo ſoft, that it may eaſily enough cut with a knife, and was eaten like their other fruits; but being kept about a week in the hot air, it grows ſolid, and ſo hard, as to require good ſteel tools to work upon it. He added, that though, even upon the place, the fairer ſort be of very great eſteem, yet not of any ſuch prodigious price, as is given out. And he preſented me one about the bigneſs of a large egg, and a fragment of another, which are both very hard; but as for their virtues, I can yet ſay nothing upon trial, for want of having had fitting opportunities.

Other obſervations made at the bottom of the ſea may hereafter follow.

A PARADOX OF THE NATURAL AND PRETERNATURAL STATE OF BODIES, Eſpecially of the AIR.

I KNOW, that not only in living, but even in inanimate bodies, of which alone I here diſcourſe, men have univerſally admitted the famous diſtinction between the natural and preternatural, or violent ſtate of bodies, and do daily, without the leaſt ſcruple, found upon it hypotheſes and ratiocinations, as if it were moſt certain, that, what they call nature, had purpoſely framed bodies in ſuch a determinate ſtate, and were always watchful, that they ſhould not by any external violence be put out of it.

But notwithſtanding ſo general a conſent of men in this point, I confeſs, I cannot yet be ſatisfied about it in the ſenſe wherein it is wont to be taken. It is not, that I believe, that there is no ſenſe, in which, or in the account upon which, a body may be ſaid to be in its natural ſtate; but that I think the common diſtinction of a natural and violent ſtate of bodies has not been clearly explained, and conſiderately ſettled, and both is not well grounded, and is oftentimes ill applied. For, when I conſider, that whatever ſtate a body be put into, or kept in, it obtains or retains that ſtate, according to the catholick laws of nature, I cannot think it fit to deny, that in this ſenſe the body propoſed is in a natural ſtate; but then, upon the ſame ground it will be hard to deny, but that thoſe bodies, which are ſaid to be in a violent ſtate, may alſo be in a natural one, ſince the violence, they are preſumed to ſuffer from outward agents, is likewiſe exerciſed no otherwiſe than according to the eſtabliſhed laws of univerſal nature. It is true, that when men look upon a body as in a preternatural ſtate, they have an idea of it differing from that, which they had whilſt they believed it to be in a natural ſtate: but perhaps this difference ariſes chiefly from hence, that they do not conſider the condition of the body, as it reſults from the catholic laws ſettled among things corporeal, and relates to the univerſe, but eſtimate it with reference to what they ſuppoſe is convenient, or inconvenient, for the particular body it ſelf. But however it ſeems to me, that men's determining a body to be in a natural or preternatural ſtate has much more in it, either of caſual, or of arbitrary, or both, than they are aware of. For oftentimes we think a body to be brought into a violent ſtate, not becauſe really the former was not ſo, but becauſe there is a notable change made in it by ſome agent, which we alſo take notice of; whereas before the action of that agent, if the body were under any violence, it was exerciſed by uſual, but often immanifeſt agents, though perhaps their compulſion were not leſs, but only leſs heeded. And ſometimes alſo no more is to be underſtood by a body's being forced from its natural ſtate, than that it has loſt that, which it had immediately, or a pretty while before ſome notable change. Which conjectures I ſhall now endeavour to confirm, but with great brevity.

I have already ſhewn, that matter being devoid of ſenſe and appetite, cannot be truly and properly ſaid to affect one ſtate or condition more than another, and conſequently has no true deſire to continue in any one ſtate, or to recover it when once loſt; and inanimate bodies are ſuch, and in ſuch a ſtate, not as the material parts they conſiſt of, elected or deſired to make them, but as the natural agents, that brought together and ranged thoſe parts, actually made them. As a piece of wax is unconcerned, whether you give it the ſhape of a ſphere, or a cone, or a pillar, or a boat; and whether, when it has that form, you change it into any other; the matter ſtill retaining without willingneſs or unwillingneſs, becauſe without perception, that figure, or ſtate, which the laſt action of the agents (your fingers or inſtruments) determined it to, and left it in.

But this will be beſt underſtood, as well as confirmed, by particular examples. I need not tell you, that the moſt uſual inſtance alledged to ſhew, that a ſtate is natural to a body, and that being put out of it by external cauſes it will upon the ceſſation of their violence be reſtored thereunto, is, that water being heated by the fire, as ſoon as that adventitious heat vaniſhes, returns to its native coldneſs; and ſo when, by an exceſs of cold, it is congealed into ice, it does upon a thaw loſe that preternatural hardneſs, and recover the fluidity, that naturally belongs to it: and the ſame may be likewiſe ſaid of butter, which, being melted by external heat into a liquor, does upon the ceſſation of that heat grow a conſiſtent body again. But perhaps theſe inſtances will rather countenance our paradox than diſprove it. For as to the coldneſs, whereto water heated by the fire returns, when it is removed thence, it may be ſaid, that the acquired heat conſiſting but in the various and briſk agitation of the corpuſcles of the water by an external agent, it need be no wonder, that when that agent ceaſes to operate, the effect of its operation ſhould ceaſe too, and the water be left in its former condition, whether we ſuppoſe it to have been heated by the actual pervation of the corpuſcles of the fire, which muſt by degrees, fly away into the air; or that the heat proceeds from an agitation imparted by the fire to the aqueous corpuſcles, which muſt, by degrees, loſe that new agitation, by communicating it little by little to the contiguous air and veſſel; ſo that, if the former agitation of the particles of the water were, as is uſual, much more languid than that of our organs of feeling, in which faintneſs of motion the coldneſs of water conſiſted, there will be no need of any poſitive internal form, or any care of nature to account for the water's growing cold again. This will be confirmed by the conſideration of what happens to ice, which is ſaid to be water brought into a preternatural ſtate by an exceſs of cold. For I doubt, it will not be eaſily demonſtrated, that in reference to the nature of things, and not to our arbitrary ideas of them, ice is water preternaturally hardened by cold, and not water ice preternaturally thawed by heat. For if you urge, that ice left to it ſelf will, when the frigorific agents are removed, return to water; I ſhall readily anſwer, that, not to mention the ſnow and ice, that lies all the ſummer long unthawed upon the tops of the Alps and other high mountains, I have learned, by enquiry purpoſely made, from a doctor of phyſick, who for divers years practiſed in *Muſcovy*, that in ſome parts of *Siberia* (a large province belonging to the Ruſſian emperor) the ſurface of the ground continues more months of the year frozen, by what is called the natural temperature of the climate, than thawed by the heat of the ſun; and that a little beneath the ſurface of the ground, the water, that chances to be lodged in the cavities of the ſoil, continues frozen all the year; ſo that, when in the heat of ſummer the fields are covered with corn, if then you dig a foot or two, perhaps leſs, you ſhall eaſily find ice and a frozen ſoil: ſo that a man born and bred in the inland part of that country, and informed only by his own obſervation, may probably look upon water as ice violently melted by that celeſtial fire, the ſun, whoſe heat is there ſo vehement in their ſhort ſummer, as to ripen their harveſt in leſs time than in our temperate climates will eaſily be credited.

On the other ſide, we in *England* look upon melted butter, as brought into a violent ſtate by the operation of the fire, and therefore think, that when being removed from the fire it becomes a conſiſtent body again, it has but recovered its native conſtitution. Whereas there are divers parts of the *Eaſt Indies*, and, I doubt not, of other hot countries, whoſe inhabitants, if they ſhould ſee conſiſtent butter (as ſometimes by the care and induſtry of the Europeans they may do) they would think it to be brought to a preternatural ſtate, by ſome artificial way of refrigeration. For in thoſe parts of the *Indies* I ſpeak of, (though not in all others) the conſtant temper of the air being capable to entertain as much of agitation as ſuffices for fluidity in the parts of what, in our climate, would be butter, it would be in vain to expect, that, by being left to it ſelf in the air, it ſhould become a conſiſtent body. And I have learned by diligent enquiry of ſea-men and travellers, both Engliſh and others, that were eye-witneſſes of what they told me, that, in divers parts of thoſe hot regions, butter, unleſs by the Europeans, or their diſciples, purpoſely made in the cold, is all the year fluid, and ſold, or diſpenſed, not as conſiſtent bodies, by weight, but as liquors, by meaſure. To ſtrengthen this obſervation, I ſhall add, what was affirmed to me by a learned man, that practiſed phyſick in the warmer parts of *America*, namely, that he met in ſome places with ſeveral drugs, which, though they there ſeem to be balſoms, as turpentine, &c. are with us, and retained that conſiſtence in thoſe climates; yet, when they come into our colder regions, harden into gums, and continue ſuch both winter and ſummer. On the other ſide, enquiring alſo of a traveller, verſed in phyſical things, about the effects of great heat in the inland parts of *Africa*, where he had lately been; he told me, among other things, that raiſin of jalap, which, when he carried it out of *England*, was of a conſiſtence, not only dry, but brittle, did, when, and a while before, he came to *Morocco*, melt into a ſubſtance like turpentine; ſo that ſome of it, that he had made up into pills, would no more at all retain that ſhape, but remain, as it were, melted all the while he ſtayed in that city, and the neighbouring country, though when he came back to the borders of *Spain*, it returned to its former conſiſtence. Which I the leſs wondered at, becauſe, having had the curioſity to conſider ſome parcels of gum lacca, (of which ſealing-wax is made) newly brought aſhore from the *Eaſt Indies*, though it be a hard and ſolid gum, yet I found by ſeveral inſtances, that, paſſing through the Torrid Zone, divers pieces of it, notwithſtanding the ſhelter afforded it by the great ſhip it came in, had been, by the heat of the climate, melted, and made to ſtick together, though afterwards they regained their former conſiſtence, though not altogether their former colour. And on this occaſion I ſhall add, that I learned by enquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place, where it was made, among other things, furniſhed himſelf with a quantity of the beſt aloes, he obſerved, that whilſt he ſailed through very hot climates, it was ſo ſoft, that, like liquid pitch, it would often have fallen out of the wide-mouthed veſſel he kept it in, if he had not from time to time been careful to prevent it. But when he came within a hundred leagues of the coaſt of *England*, it grew hard, and ſo continued, though this were in a very warm ſeaſon of the year, being about the dog-days.

For a further confirmation of what has been hitherto diſcourſed, be pleaſed to conſider with me that moſt obvious body the air, or the atmoſphere we live and breathe in. For though ſeveral opinions and argumentations are founded upon what their authors call the natural and preternatural or violent ſtate of the air, yet he, that conſiders, ſhall find it no eaſy thing to determine, what ſtate of the air ought to be reputed its truly natural ſtate, unleſs in the ſenſe I formerly told you I employ that expreſſion in. I will not inſiſt on the heat and coldneſs of the air; for that being manifeſtly very differing in the heart of winter, and in the heat of ſummer, and in differing regions of the air, as at the top and bottom of high mountains, at the ſame time, and conſtantly in differing regions of the earth, as in *Barbary* and *Greenland*, it will not be ſo eaſy to determine, what ſtate is natural to the air. But that only, which I ſhall now conſider, is its ſtate or tone in reference to rarity and denſity. For ſince the air is believed to be condenſed by cold, and expanded by heat, I demand, at what time of the year, and in what country, the air ſhall be reputed to be in its natural ſtate? For if you name any one time, as the winter or the ſummer, I will aſk, why that muſt be the ſtandard of the tone of the air rather than another ſeaſon, or at leaſt excluſively to all others? And the like difficulty may be made about the climate or the place. And theſe ſcruples are the more allowable to be propoſed, becauſe learned men have delivered, that in ſome countries the mercury in the Torricellian experiment is kept higher than in others, (as in *Sweden* than in *Italy*,) and our baroſcopes inform us, that oftentimes, in the ſame place and day, the quick-ſilver in the ſame inſtrument does conſiderably vary its height; which ſhews, that the air or atmoſphere muſt neceſſarily vary its weight, and therefore probably its degree of rarity or denſity.

But I have yet to propoſe a further conſideration in this affair: For what if it ſhall appear, that neither in winter nor in ſummer, in *Sweden* or in *Italy*, or in whatever country, region, or ſeaſon you pleaſe, the air we breathe in is in any other than a preternatural ſtate? nay, that even when we have vehemently agitated and expanded it by an intenſe heat of the fire, it is not yet violently rarified, but yet violently conſtipated, unleſs, in our ſenſe before declared, you underſtand with me the preternatural ſtate of rarefaction in the air, in reference to the tone it had before the laſt notable change was produced in it. This will, I queſtion not, ſeem a ſurprizing, if not a wild, paradox; but yet, to make it probable, I ſhall only deſire you to reflect upon two or three of my phyſico-mechanical experiments; and there you will ſee, firſt, that the air being a body abounding with ſpringy particles, not devoid of gravity, the inferior muſt be compreſſed by the weight of all the incumbent. And next, that this compreſſion is ſo great, that though by the heat of the fire neither others, nor we, could bring a portion of included air to be expanded to above fourſcore times its former ſpace; yet without heat, by barely taking off the preſſure of the ſuperior air, by the help of our pneumatical engine, the air was rarified more than twice as much: and ſince thoſe experiments were publiſhed, I more than once rarified it to above five hundred times its uſual dimenſions; ſo that, if, according to what is generally agreed on and taught, a body be then in a preternatural ſtate, when, by an external force, it is kept in a condition, from which it inceſſantly tends to get free; and if it be then moſt near its natural ſtate, when it has the moſt proſperouſly endeavoured to free it ſelf from external force, and comply with its never ceaſing tendency; if this be ſo, I ſay, then the air we live in is conſtantly in a preternatural ſtate of compreſſion by external force. And when it is moſt of all rarified by the fire, or by our engine, its ſpring having then far more conveniency than before to diſplay themſelves, which they continually tend to do, it anſwerably approaches to its natural ſtate, which is to be yet leſs compreſſed or not at all. And I have carefully tried, for many months together, that when the air has been rarified much more than even a vehement heat will bring it to be, yet if it were fenced from the preſſure of the external air, it would not ſhrink to its former dimenſions, as if it had been put into a violent ſtate, from whence nature would reduce it to them, but continued in that great and ſeemingly preternatural degree of extenſion, as long as I had occaſion to obſerve it. One might here ſhew, that this odd conſtitution of the air is ſo expedient, if not neceſſary for the motion, reſpiration, and other uſes of animals, and in particular of men, that the providence and goodneſs of the wiſe author of the univerſe is thereby ſignally declared; if it were not improper, in ſuch a paper as this, to employ final cauſes. Wherefore, to avoid the imputation of impertinence, I will conclude, by taking notice, that, from what has been delivered, we may learn two things conſiderable enough, if not in themſelves, yet to ſome paſſages of the treatiſe, whereof this paper makes a part. And firſt, we may deduce from what has been ſaid of the air, that, according to what is noted above, that may ſometimes generally be granted, and believed to be the natural ſtate of a body, not which it really affects to be in, or (to ſpeak more properly) has a tendency to attain, but that which it is brought into, and kept in by the action, or reſiſtance of neighbouring bodies, or by ſuch a concourſe of agents and cauſes, as will not ſuffer it to paſs into another ſtate. And the ſecond thing we may hence learn, is, that whatever men ſay of nature's never miſſing her aim, and that nothing violent is durable; yet, bating an inconſiderable portion of aerial particles at the upper ſurface, for aught we know, the whole maſs of the air we live in, and which invirons the whole terraqueous globe, has been, from the world's beginning, and will be to its end, kept in a ſtate of violent compreſſion.

A STATICAL HYGROSCOPE

Propoſed to be further Tried,

Together with

A BRIEF ACCOUNT

OF THE

UTILITIES of HYGROSCOPES.

A STATICAL HYGROSCOPE, &c.

In a LETTER to H. OLDENBURG, Eſq; Secretary to the ROYAL SOCIETY.

SIR,

THOUGH I writ to you from *Stanton*, an account of thoſe hygroſcopes, whereof I now preſent you one; yet ſince I remember, that it was in the year 1665, that I ſent you that paper, I fear you may, by this time, have forgotten much of what it contained, and thereby made it fit for me, in this letter, both to remind you of ſome former paſſages, and to add ſome obſervations, that lately occurred to me; and this the rather, becauſe I do not preſent you with this trifle, merely to gratify your curioſity, but that you, and ſome of your ingenious friends, may, by your remarks, help me to diſcover, to what inconveniences our inſtrument is liable, how far they may be avoided, or leſſened, or what the uſes, or advantages, of it may be, notwithſtanding its inevitable inconveniences, or imperfections.

HAVING had occaſion, amongſt other ſubjects relating to the air, to conſider its moiſture, and its dryneſs, I eaſily diſcerned, that they had no ſmall influence upon divers bodies, and, among the reſt, upon thoſe of men, as the ambient air we breath in, either paſſes from one of thoſe qualities to the other, or even from one degree to the other, in the ſame quality.

WHEREFORE, I began to caſt about, ſomewhat ſollicitouſly, for a way, that might, better than any I had yet tried, or elſewhere met with, diſcover the changes of the air, as to moiſture and dryneſs, and the degrees of either quality. For which purpoſe, it ſeemed to me, that if a ſtatical hygroſcope could be had, it would be very convenient, in regard of its fitneſs, both to determine the degrees of the moiſture, or dryneſs of the air, and to tranſmit the obſervations made of them to others. Whereupon, conſidering further, that among bodies, otherwiſe well qualified for ſuch a purpoſe, that was likelieſt to give the ſenſibleſt informations of the changes of the air, which, in reſpect of its bulk, had the moſt of its ſurface expoſed thereunto; I quickly pitched upon a fine ſpunge, as that, which is eaſily portable, not eaſy to be divided, or diſſipated, which, by its readineſs to ſoak in water, ſeemed likely to imbibe the aqueous particles, that it may meet with diſperſed in the air, and which, by its great porouſneſs throughout, has much more of ſuperficies, in reference to its bulk, than any body, not otherwiſe leſs fit for the intended uſe, that came into my thoughts.

IF you recal to mind, when, and whence I firſt gave you notice, that I employed our little inſtrument, you will eaſily believe, that the inducements I had to pitch upon it, were, that I ſhould need but ſuch light and parable things, as I could eaſily both procure in the country (where I then was) and carry about with me, in the frequent removes I was obliged to make; and therefore, that I did not repreſent this trifle as the beſt hygroſcope, that could be deviſed, or even as the beſt, that, perhaps, I myſelf could have propounded, if I would have framed an elaborate engine with wheels, ſprings, or equivalent weights, pullies, indices, and other contrivances, ſome of which I divers years ago made uſe of. For I little doubt, but that mechanical heads may frame hygroſcopes much curiouſer and perfecter, than that I now ſend you, or any other I have uſed, or, ſeen, if they may be accommodated with ſufficient room, and dextrous artificers, that will work exactly according to directions; whereas, my deſign being not ſo much to make a machinal, or engine-like, as a ſtatical hygroſcope, and ſuch an one as may be ſimple, cheap, contained, and ſet up in a little room, eaſy to be made and tranſported, I thought it might be of ſome uſe, eſpecially to thoſe, that are not furniſhed with curioſities and mechanical accommodations, if, among the ſeveral forms of hygroſcopes, that I had in my mind, I choſe one, that being ſtatical and eaſy, might be as commodious by its ſimplicity, as ſome others by their elaborateneſs; eſpecially, if we conſider, that, as ſlight an inſtrument as it ſeems, it may be applied to various uſes, ſome of which are not ſlight, as will ere long be made probable.

IF I ſhould be here told by one, that grants the preferableneſs of ſtatical hygroſcopes in the general, that there are divers bodies, other than that pitched upon by me, whoſe weight may vary, when the temperature of the air is conſiderably altered, as to dryneſs and moiſture, and that, perhaps, among theſe, ſome one may be found, that may imbibe the aqueous particles of the air better than our ſpunge; I ſhall not reſolutely deny it, and therefore ſhall leave you to make trials with what other bodies you ſhall think fit, contenting myſelf to have ſuggeſted, in general, the conveniency of making hygroſcopes, where the differing changes of the air may be eſtimated by weight; but this I ſhall tell you, in favour of our ſpunge, that when I was conſidering, what bodies were the fitteſt to be employed for the making of ſtatical hygroſcopes, I made trial of more than one, that ſeemed not the leaſt promiſing. I know, that common, or ſea-ſalt, will much relent in moiſt air, and ſalt of tartar will do it much more; but then thoſe ſalts, eſpecially the latter, will not ſo eaſily, as they ſhould, part with the aqueous corpuſcles they have once imbibed, and are in other regards, (which it were not worth while to inſiſt on,) leſs convenient than a ſpunge. I made trial alſo with lute-ſtrings, which were purpoſely choſen very ſlender, that they might have the greater ſurface, in reſpect of their bulk: theſe I found, at firſt, to do very well, as to the imbibing of the moiſture of the air, but afterwards, they did not continue to anſwer my expectation. I cauſed likewiſe to be turned out of a light wood a cup, which, that it might leſs burden a tender balance, had, inſtead of a foot, a little button, to which a hair might be tied, to ſuſpend it by; and this cup being purpoſely turned very thin, that it might have much ſurface expoſed to the air, proved for a pretty while ſo good a hygroſcope, as invited me to make divers obſervations with it, ſome of which I have ſtill by me. It agreed alſo with ſeveral trials, that I had made on other occaſions, of the porouſneſs of ſuch bodies, that white ſheeps leather, ſuch as ſurgeons uſed to ſpread plaiſters upon, would be very convenient for my purpoſe. And indeed I found by many obſervations, whoſe ſucceſs you may command a ſight of, that if this leather were a ſubſtance as little obnoxious to corruption as a ſpunge, it would, by its copious imbibitions, and emiſſions of the aerial moiſture, be a fitter matter, than any other I had employed for a hygroſcope.

BUT taking all things together, I found no body ſo convenient for my purpoſe as a ſpunge; which you will, perhaps, the more eaſily believe, if I add, that to help me to make ſome eſtimate of the poroſity of it, [we weighed out a drachm of fine ſpunge, and having ſuffered it to ſoak up what water it could, it was held in the air, not only whilſt the weight of the water would eaſily make it run out, but till it dropped ſo very ſlowly, that a hundred was reckoned after one drop, before another fell; then putting it into the balance it had been weighed in before, we found, that as its dimenſions were increaſed to the eye, ſo its weight was increaſed upon the ſcale, amounting now to ſomewhat above two ounces and two drachms; ſo that one drachm of ſpunge, though it ſeemed not altogether ſo fine as the portion we had choſen out for our hygroſcopes, did imbibe and retain ſeventeen times its weight of water.]

Now, when one is reſolved to employ a ſpunge, there will not need to be much added about the turning it into a hygroſcope. For, having weighed it, when the air is of a moderate temperature, it requires but to be put into one of the ſcales of a good balance, ſuſpended on a gibbet, as they call it, or ſome other fixed and ſtable ſupporter. For the ſpunge being carefully counterpoiſed, at firſt, with a metalline weight (becauſe that alters not ſenſibly with the changes of the air) it will, by its decrement, or increaſe of weight, ſhew, how much the neighbouring air is grown drier, or moiſter, in the place, where the inſtrument is kept. The weight of the ſpunge may be greater, or leſs, according to the bigneſs and goodneſs of the balance, and the accurateneſs you deſire in the diſcoveries it is to make you. For my part, though I have, for curioſity's ſake, with very tender ſcales employed, for a good while, but half a drachm of ſpunge, and I found it to anſwer my expectation well enough; and though, when I uſed a bulk divers times as great, in a ſtronger, but proportionably leſs accurate balance, I found not the experiment ſucceſsleſs; yet, after trials with differing quantities of ſpunge, I preferred, both to a greater and leſſer weight, that of a drachm, as not being heavy enough to overburden the finer ſort of goldſmiths ſcales, and yet great enough to diſcover changes conſiderably minute, ſince they would turn, diſcernibly, with a ſixteenth, or twentieth part, and manifeſtly, with half a quarter of a grain.

WITH ſuch hygroſcopes as theſe (wherein the balance ought to be ſtill kept ſuſpended and charged) I made ſeveral trials, as my removes and accommodations would permit, ſometimes in the ſpring, and ſometimes in the autumn, and ſometimes alſo in the ſummer and winter. But nevertheleſs, it would be very welcome to me, if you, and ſome of your friends, would be pleaſed to make trials yourſelves, and compare them with mine, and eſpecially take notice, if you can, whether, in any reaſonable tract of time, there will be any loſs, worth noting, of the ſubſtance of the ſpunge itſelf; I having not hitherto diſcovered any. In the mean time, to invite you to give yourſelves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a drachm of ſpunge ſuſpended during a whole ſpring, and a great part of the preceding winter, and ſubſequent ſummer, I did not think my pains loſt, though divers of the obſervations they afforded me have unhappily been ſo, among many other memorials about experiments of differing kinds; notwithſtanding which unſeaſonable loſs, I ſhall venture to ſuggeſt ſome things to you, that occurred to me about the utilities of the inſtruments I am treating of.

A
BRIEF ACCOUNT
OF THE
UTILITIES of HYGROSCOPES.

THE uſe of a hygroſcope is either general, or particular; the former is almoſt coincident with the qualifications to be wiſhed for, and aimed at, in the inſtrument itſelf; the latter points out the particular applications, that may be made of it, when it is duly qualified. Of each of theſe, I ſhall briefly ſubjoin what readily occurs to me.

The general uſe of a hygroſcope is, "To "eſtimate the changes of the air, as to moi"ſture, and dryneſs, by ways of meaſuring "them, eaſy to be known, provided, and "communicated."

I might here pretend, that as theſe are the principal things, that have been deſired in hygroſcopes, ſo it is obvious, from the deſcription and account we have given of our inſtrument, that theſe advantages belong to it in no very deſpicable degree: and that to make ſuch hygroſcopes, as will perform all theſe things in perfection, whatever it may ſeem to a mental contriver, will, I fear, prove no eaſy taſk to thoſe, that really attempt it. To theſe things I might add, that if ſuch allowances be made, as what I have repreſented may invite you to grant, the qualifications lately mentioned, as deſirable in a hygroſcope, may, in a tolerable meaſure, be found in ours, when we ſhall come to mention the particular uſes of it. And as for that of conveying to others the obſervations made with it, you may pleaſe to conſider, that the things I employ to meaſure the degrees of dryneſs and moiſture in the air, being grains, parts of grains, and greater weights, the acceſſions of moiſture, which the ſpunge receives, or the loſſes, that it ſuffers, can be eaſily, and at the ſame time, both found and determined. And as the weights employed to determine theſe differences are eaſily procurable; ſo the obſervations made with them may (together with patterns, if it ſhould be needful, of the weights themſelves) with the ſame facility be communicated by letters even to remote parts. In which conveniency, whether, and how far, our inſtrument has the advantage of that made with an oaten beard, and ſome others, that I have employed, I leave you to conſider.

I might farther alledge on the behalf of our inſtrument, that whereas, beſides the qualifications above-mentioned, there is another, namely, durableneſs, which, though not ſo neceſſary to conſtitute a hygroſcope, yet is neceſſary, as will ere long appear, to ſome of the conſiderableſt uſes of it: and whereas ſuch a durableneſs is wiſhed, as may not only keep the inſtrument from having its ſubſtance rotted, or corrupted by the air, but may alſo preſerve it in a capacity to continue pretty uniformly its informations of the air's moiſture, even when that increaſes very much, or laſts very long; whereas, I ſay, theſe things are much deſired in a hygroſcope, our ſpunge ſeems herein preferable to the oaten beard, lute-ſtrings, &c. For in thoſe, and the like bodies, the ſelf-contracting, or relaxing power, (as it is ſuppoſed) or the diſpoſition to imbibe, and part with the moiſture of the air uniformly, or after a due manner, is wont to be in no very long time altered, or impaired; and particularly, when they have imbibed much aerial moiſture, they are very faintly affected by the ſupervening degrees of it, and ſo the operation is too diſproportionate to what the like cauſe would have produced, when the inſtrument was well diſpoſed; whereas, in our ſpunge, neither the degree of ſpringineſs, nor any ſuch-like quality is conſidered, and it is capable of imbibing ſo much more of the aqueous particles, than even moiſt airs and ſeaſons are wont to ſupply it with, that there is little fear, that it will be glutted, or have its pores choked up with them, ſo that the decrements, and acceſſions of weight, will be more proportionate to the degree of moiſture in the air, and more reducible to known and determinate meaſures.

BUT though theſe, and the like ſpecious things, may be repreſented in favour of our ſtatical hygroſcope; yet, to deal ingenuouſly with you, I much fear, that it will be very difficult to bring either ſtatical ones, or perhaps any other, to be ſo complete, as to ſatisfy a nice and ſevere critick. And you would perhaps eaſily aſſent to my opinion, if it were not too tedious to entertain you with all the ſpeculative doubts and ſcruples, as well mechanical as phyſical, which my accuſtomed diffidence has now and then ſuggeſted to me. But becauſe ſuch a ſceptical diſcourſe would be too tedious, and alſo ſomewhat improper, to be propoſed by one, that would recommend hygroſcopes, I ſhall only now take notice of one great imperfection, which all, that I have been acquainted with, are liable to; namely, that men have not yet found, nor perhaps ſo much as dreamed of ſeeking a ſtandard of the dryneſs and moiſture of the air, by relation to which hygrometers may at firſt be adjuſted, and ſo be compared with one another, as we ſee many of thoſe ſealed thermoſcopes, that have been made and juſtned by Mr. *Shotgrave*, the dextrous operator of the Royal Society. I deny not, that, by virtue of a ſtandard to eſtimate moiſture by, I have endeavoured to remedy this inconvenience; but, as my hopes were but ſmall, ſo neither was my ſucceſs great, but I am not ſure, that happier wits, or I myſelf, at ſome other and luckier time, may not more proſperouſly attempt it. In the mean while, perchance, you will not think it altogether nothing, if the trifle I preſent you, perform, at leaſt, ſome of the things deſired in a hygrometer leſs imperfectly, than any you have yet met with. And that you may not be diſcouraged by what I have lately acknowledged of the defects of ſuch inſtruments, I think it now ſeaſonable to proceed to the mention of the particular uſes, for which, notwithſtanding any inevitable defects, a hygroſcope, and even ſuch a one as I now preſent you, may be made eaſily to ſerve.

USE I.

To know the differing variations of weather in the ſame month, day and hour.

IT may be uſeful for divers purpoſes, to know, both that the air is wont to be leſs moiſt at one part of the artificial day (and ſo of the night,) than at any other, and at what particular time of the day or night it moſt uſually is ſo. And on this occaſion I remember, that uſually, when the weather was at a ſtand, it was obſerved, that the ſpunge had manifeſtly gained in the night, though it were kept in a bed-chamber, and grew lighter again between the morning and noon. This obſervation, which was made towards the end of winter, would not hold, in caſe froſty nights or ſome other powerful cauſe intervened. It were not amiſs alſo to obſerve, whether there be not a correſpondence betwixt the hygroſcope and baroſcope; and if there be, in what kind of weather or conſtitution of air it is moſt or leaſt to be diſcerned. And this enquiry ſeems the more dubious, becauſe the ſame changes of the atmoſphere may, upon differing accounts, have either the like, or quite contrary operations upon theſe two inſtruments. For in ſummer, when the atmoſphere is uſually heavier, the hygroſcope is uſually lighter; ſome ſtrong winds, as with us the north-weſt, may make both the atmoſphere and baroſcope lighter, whereas ſoutherly winds, eſpecially if accompanied with rain, often make the atmoſphere lighter and the ſpunge heavier. And on the other ſide I obſerve, that eaſterly winds, eſpecially when they begin to blow in winter, though, by reaſon of their dryneſs, they are wont to make the hygroſcope lighter, yet they are wont, at leaſt here at the weſt-end of *London*, to make the baroſcope ſhew the air to be heavier. It were likewiſe fit to be obſerved, particularly by thoſe, that live on the ſea-coaſt, whether the daily ebbing or flowing of the ſea do not ſenſibly alter the weight of the hygroſcope. It were very well worth while alſo to take notice, at what time of the day or night, *cæteris paribus*, the air is the moſt damp and moſt dry, and not only in ſeveral parts of the ſame day, but in ſeveral days of the ſame month; eſpecially on thoſe days, wherein the full and new moons happen. And this ſeems a more hopeful way of diſcovering, whether the full moon diffuſes a moiſture in the air, than thoſe vulgar traditions of the plumpneſs of oyſters and ſhell-fiſh, and brains in the heads of ſome animals, and of marrow in their bones, and divers other phænomena, which, as I have ſhewn in another paper, it is not eaſy to be ſure of. It may alſo be noted, whether monthly ſpring-tides, eſpecially when they fall out near the middle of *March* or *September*, have any ſenſible operation upon our inſtrument.

USE II.

To know, how much one year and ſeaſon is dryer or moiſter than another.

THIS cannot be ſo well performed by the hygroſcope made of an oaten beard, if they, that have made uſe of them more than I, do complain with reaſon, that after ſome months (for I cannot tell you preciſely how many) they begin to dry up and ſhrink; ſo that their ſenſe of the varying degrees of the moiſture of the air is not ſo quick as before, and the informations they give of the degrees of it, eſpecially towards the outmoſt bounds of their power, to ſhew the air's alterations, recede more and more from uniformity. But the laſtingneſs and other convenient qualifications of our ſpunge making its capacity of doing ſervice more durable, may the better help us to compare the greateſt moiſture and dryneſs, both of the ſame ſeaſon, and of the ſeaſons of one year with the correſpondent ones of another. And if the weight of the ſpunge at a convenient time, when the temperature of the air is neither conſiderably moiſt, nor conſiderably dry, be taken for a ſtandard, a perſon, that ſhould think it worth his pains, may, by computing how many days at ſuch an hour, and how much at that hour, it was heavier or lighter than the ſtandard, and alſo by comparing the reſult of ſuch an account in one year with the reſult of the like account in another year, be aſſiſted to make a more particular and near eſtimate of the differing temperature of the air, as to moiſture and dryneſs, in one year than in another, and in any correſpondent ſeaſon or month, aſſigned in each of the two years propoſed. And how much the collation, or continuance of ſuch obſervations, both in the ſame place, and alſo in differing countries and climates, may be of uſe to phyſicians in reference to thoſe diſeaſes, where the moiſture and dryneſs of the air has much intereſt; and the huſbandman to foreſee what ſeaſons will prove friendly or unkind to ſuch and ſuch ſoils and vegetables; it muſt be the work of time to teach us, though in the mean while we have no reaſon to deſpair, that the uſes to be made of ſuch obſervations may prove conſiderable. And the rather, becauſe if by help of the reſult of many obſervations men be enabled to foreſee (though at no great diſtance off) the temperature of a year, or even of a ſeaſon, it may advantage not only phyſicians and plowmen, but other profeſſions of men, who receive much profit or prejudice by the dryneſs or exceſſive moiſture of the ſeaſons. And not to mention thoſe, who cultivate hops, ſaffron, and other plants, that are tender and bear a great price; ſuch a foreſight, as we are ſpeaking of, may be of great uſe to ſhepherds, who, in divers parts of *England*, are oftentimes much damnified, if not quite undone, by the rot of ſheep, which uſually happens through exceſs of moiſture in certain months of the year. And in order to the providing of foundations, whereupon to build predictions, it may not be amiſs to regiſter the number, bigneſs, and duration of the conſiderabler ſpots, that may at this or that time of the year happen to appear, or be diſſipated on or near the ſun, or to take notice of any extraordinary abſence of them, and to obſerve, whether their apparition, or diſſipation, produce any changes in the hygroſcope: which curioſity I ſhould not venture to propoſe, but that (as I elſewhere note) I find, that eminent aſtronomers have caſually obſerved great dryneſſes to attend the extraordinary abſence or fewneſs of the ſolar ſpots. And thoſe perſons, that are aſtrologically given, may, if they pleaſe, extend their curioſity in the uſe of this inſtrument to obſerve, whether eclipſes of the ſun and moon, and the great conjunctions of the ſuperior planets, have any notable operation upon it.

USE III.

To diſcover and compare the changes of the temperature of the air made by winds, ſtrong or weak; froſty, ſnowy, and other weather.

THIS may conveniently enough be done, as to winds, either by our whole inſtruments, or (perhaps better and more ſafely) by the ſpunge alone, which may be taken off and hung by a ſtring, for as long time, as is thought fit, in the wind, and then reſtored to its former place. For I found by removing it into the wind, that it ſoon received a very conſiderable alteration in point of weight, as alſo it did, when removed out of a room into a garden where the ſun ſhined; for though the ſeaſon were not warm, it being then the month of *January*; yet in three quarters of an hour the ſpunge loſt the 24th part of its weight. We may alſo in ſome caſes uſefully ſubſtitute to a ſpunge a ſomewhat broad piece of good ſheeps-leather diſplayed to the wind. For this having, by reaſon of its thinneſs (or very ſmall depth,) in proportion to its breadth, a very large ſuperficies immediately expoſed to the wind, we found it to be notably altered thereby, in ſo much, that half an ounce of well prepared ſheeps-leather, (that we had long employed as an hygroſcope) being kept an hour in a place, where the ſun-beams might not beat upon it, did, in a ſtrong wind, vary in that ſhort time an eighteenth part of its original weight. But though I think it very poſſible to make ſuch obſervations of the temperature of particular winds, as will frequently enough prove ſo true as to be uſeful, at leaſt to thoſe, that live in the places where they are made; yet I am of opinion, that, to be able to ſettle rules any thing general, to determine with any certainty the qualities of winds, according to the corners, whence they blow, as from the eaſt or weſt, north-eaſt, ſouth-weſt, &c. there will be a great deal of warineſs required; and he, that has not ſome competent ſkill in phyſicks and coſmography, will eaſily be ſubject to miſtakes in forming his rules. To countenance which advertiſement, I ſhall now make uſe but of theſe two conſiderations, whereof the firſt is; that winds, that blow from the ſame quarter, are not in ſome countries of the ſame quality, that they are in moſt others, the wind participating much of the nature of the region, over which it blows in its paſſage to us. At the famous port of *Archangel* they obſerve, that whereas a northerly wind, almoſt every where elſe without the tropicks, produces froſt in winter, there it is wont to be attended with a thaw, ſo as to make the eves to drop. Of which the reaſon ſeems to be, that this wind comes over the ſea, which lies north from that place; and on the contrary, a ſoutherly wind, blowing over a thouſand or twelve hundred miles of frozen land, does rather increaſe the froſt than bring a thaw. This was by the inhabitants averred to the Ruſſian emperor's phyſician, who was more than once at *Archangel*, and from whom I had the account. The northern winds, that are elſewhere wont to be drying, are ſaid in *Egypt* to be moiſt. I remember Mr. *Sandys*, in his excellent Travels, giving an account of what he obſerved about the largeſt of the famed Egyptian pyramids, has this conſiderable paſſage; "Yet this hath been too great a morſel for "time to devour, having ſtood, as may be "probably conjectured, about three thouſand "and two hundred years, and now rather old "than ruinous: yet the north ſide moſt worn, "by reaſon of the humidity of the northern "wind, which here is the moiſteſt." *Sandys* in *Purchas*'s Pilgrimage. And it is yet more conſiderable to our purpoſe what I find related by monſieur *de Serres*, in his uſeful book of huſbandry, ſince by that it appears, that even in not very diſtant provinces of the ſame kingdom, the winds, that blow from the ſame quarter, may have very differing qualities and effects. For, ſpeaking of the changes of the air, in reference to huſbandry, in ſeveral parts of *France*, he informs us, that it is obſerved, that in the quarters about *Tholouſe* the ſouth-wind dies the ground, and the north gives rains. Whereas on the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this laſt named wind cauſes dryneſs, and the other brings moiſture. And this may ſuffice for my firſt conſideration. My ſecond is this, that the vehemence or the faintneſs of the winds, though blowing over the ſame country, may much diverſify its operation on the hygroſcope; and the ſame wind, which, when it blows but faintly, or even moderately, is wont to appear moiſt by the hygroſcope, may, when vehement or impetuous, make the inſtrument grow lighter, diſcuſſing and driving away more vapours by the agitation of parts it makes in the ſpunge, than is countervailed by thoſe aqueous vapours, that are brought along with it. But on ſuch things as theſe I have not leiſure to inſiſt, and therefore I ſhall proceed to take notice, in very few words, of ſome other operations of differing weathers on our inſtrument, and tell you, that froſty weather often made the hygroſcope grow lighter even at night: ſnowy weather, which laſted not long, added ſomething to the weight of the ſpunge. And it has been obſerved, that miſts and foggy weather uſed to add weight to it, even notwithſtanding froſt.

Lib. VI. cap. 8. Sect. 3.

Theat. d'Agricult. lib. I. cap. 7.

To which may be added an obſervation made by my amanuenſis, who having a convenienter chamber than mine, (wherein a fire was daily made,) was diligent and curious to ſet down the changes of the hygroſcope, that was left in his lodging; for this obſervation makes it probable, that a tranſient cloud in fair weather may be (for I ſay not, that it always is) manifeſtly obſervable by our inſtrument. For by his diary it appears, that the ninth of *September*, being for the moſt part a very fair ſun-ſhiny day, though about ten a clock in the morning, the ſun ſhone brightly, the ſpunge began to preponderate, which unexpected phænomenon made him look out at the window, where he diſcovered a cloud, that darkened the ſun, but after a while, that being paſt, the balance returned to an æquilibrium. On this occaſion I ſhall intimate, that I have more than once or twice obſerved, eſpecially in ſummer, that when the air grew heavier, the hygroſcope either continued at a ſtand, or perhaps, alſo grew lighter; as if, when ſuch caſes happen, the effluvia, that get into the air, either from the terreſtrial, or ſome other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the ſpunge, and ſo were corpuſcles of another nature, with which, when we find by the baroſcope, that the air is plentifully ſtocked, it may be worth while to obſerve, whether any, and if any, what kind of meteor, as wind, or rain itſelf, or hail, or in the winter ſnow or froſt, will commonly be ſignified and produced.

USE IV.

To compare the temperature of differing houſes, and differing rooms in the ſame houſe.

As this is of great uſe, both in reſpect of mens health, eſpecially if they be of a tender, or ſickly conſtitution, and in reſpect of conveniency for the keeping fleſh, ſweat-meats, and ſeveral ſorts of wares and goods, and even houſhold-ſtuff, that are ſubject to be indamaged by moiſt air; ſo it is readily and manifeſtly derivable from our inſtrument. For, by removing it into ſeveral houſes, or into ſeveral parts of the ſame houſe, and letting it ſtand in each a competent time, to be affected with the temperature of the air of that particular place, we have divers times obſerved a notable difference, as you may gueſs by the two or three notes I met with among ſome old papers.

October 13. [THREE or four days ago, a piece of fine ſpunge being taken out of a cabinet, and clipped, till it came to weigh juſt half a drachm in a nice pair of ſcales, and a warm room, was afterwards removed into a neighbouring room deſtitute of a chimney, (and yet within three or four yards of a chimney ſeldom without fire:) this ſtatical hygroſcope, conſiſting of the ſcales and the frame they hung on, was yeſterday night removed into the former room, and the ſpunge was found to have gained three grains and $\frac{1}{8}$, or better, and conſequently more than a tenth part, in reference to its firſt weight; but being ſuffered to ſtand in this warm room, in leſs than twelve hours, it loſt a grain and about $\frac{1}{6}$ of its former weight, though the time it ſtood in this room were, for the moſt part, night and rainy weather.]

[WE took a piece of very fine ſpunge, which formerly had weighed juſt a drachm, but having been many months kept in a very warm room, where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring houſe, and weighing it in tender ſcales, in the evening it was found to want of a drachm, four grains, and $\frac{1}{4}$ of a grain; and though there was a fire in the room, and the ſcales ſtood not far from it, yet, in a ſhort time, (the day being foggy and rainy,) the ſpunge viſibly depreſſed its ſcale ‡, and the next morning was found to want but one grain and a half of a drachm, ſo that it had gained about three grains and a quarter, and the following evening, being the ſecond of *January*, it weighed one drachm, a grain, and almoſt half a grain. So that in about one natural day, the ſpunge had acquired ſix grains from the moiſture of the air, that is, a tenth part of its firſt weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moiſt, the weight exceeded two grains, but about three or four of the clock in the afternoon, it began to loſe of that great weight, which diminiſhed more by the next morning, the weather having changed that night, and become ſomewhat froſty.]

IN another paper I alſo find this note. [The drachm of a ſpunge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room, where fire is not wont to be kept, the weather being extraordinarily foggy: this morning, being brought into the former room, though now the weather be clear (yet not froſty) it appears to have gained in weight about eleven grains; yet it ſoon loſt two grains by ſtanding in this room all the while in the balance.]

USE V.

To obſerve in a chamber the effects of the preſence, or abſence of a fire in a chimney or ſtove.

THIS is eaſily done, and the more eaſily, if the room be ſmall. For in ſuch chambers, I have often obſerved a moderate fire to alter the weight of the inſtrument, placed at a diſtance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not ſeaſonably renewed with freſh fuel, the decay of it would, in no long time, begin to be diſcernible by the inſtrument.

USE VI.

To keep a chamber at the ſame degree, or at an aſſigned degree of dryneſs.

SUPPOSING the alteration of weight in our ſpunge to depend only upon the degree of the moiſture of the air, the laſt named uſe will be but an obvious corollary of the former. For, if a convenient part of the room be choſen for the hygroſcope, and it be kept conſtantly there, it is eaſy, by caſting one's eye on it from time to time, to perceive, when it will be requiſite to encreaſe or moderate the fire, ſo as to keep the ſpunge at that weight it was of, when the temperature of the air of the chamber, as to dryneſs and moiſture, was ſuch as was deſired. I will not trouble you with ſome ſcruples, which, I confeſs, the conſideration of this uſe of our inſtrument ſuggeſted to me, becauſe I have not now the leiſure to diſcuſs them. I had thoughts to try, whether, and how far a good quantity of ſalt of tartar, or even dried ſea-ſalt, being kept in a cloſet, or ſome cloſer room, might by imbibing leſſen the moiſture of the air in it, but I did not perfect any obſervation of this kind. But I will add to what I have already referred to this ſixth head, that I have ſometimes noted with pleaſure, how manifeſt and great a change in the weight of our ſpunge would be made, when the room was waſhed, and a good while after, notwithſtanding that a good fire was kept in it, to haſten the drying of it.

BESIDES the hitherto mentioned uſes of our hygroſcope, I know not, whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of ſhewing us ſome difference betwixt ſteams of differing natures, as thoſe of water, ſpirit of wine, chemical oils, and perhaps new kinds of ſubſtances (ſuch as we have not yet taken notice of) in the air, in which I confeſs, I ſuſpect there may ſometimes be diſperſed ſtore of corpuſcles, that I do not yet well know what to think of. For I have more than once obſerved (not without ſome wonder) the hygroſcope not to be affected with the alteration of weather, anſwerably to what the manifeſt conſtitutions, or variations of it ſeem plainly to require: whether unobſerved corpuſcles performed this, by making the other ſteams in point of figure, or ſize, incongruous to the minute pores of the ſpunge, and ſo unfit to enter them; or by diſſipating, or otherwiſe procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not ſure, but by aſſociating this inſtrument with the thermoſcope, baroſcope, and ſome others, that may be propoſed, it might be ſo improved, as to help us to foreſee divers conſiderable things, that either are themſelves changes of the air, or are wont to be conſequences of them: as ſickly and healthful conſtitutions of the air, both as to man and cattle; and healthful, barren, or plentiful ſeaſons in particular places or countries; and perhaps alſo ſtrong hurricanes, earthquakes, inundations, and their ill effects, eſpecially thoſe accidents, that depend much upon the ſurcharge of the air, with other exhalations and moiſt vapours, which operate before ſenſibly upon our inſtrument, and therefore may be diſcernible by it a good while before they arrive at that height, that makes them formidable meteors. And if it were but the foretelling approaching rain, this very thing may, on divers occaſions, prove very ſerviceable, and recommend our inſtrument, which often receives much earlier impreſſions from the ſteams, that ſwim up and down in the air, than our ſenſes do; ſo that I have been able to foreſee a ſhower of rain, eſpecially in dry weather, a not inconſiderable while before it fell.

AND here I ſhould diſmiſs our ſubject, which I have already dwelt on longer than I deſigned, but that remembering a caution I gave you, when I was ſpeaking of winds, I think it but fit to add two or three lines, to keep you from being by that advertiſement diſcouraged from See USE III.

endeavouring to make, in the general, ſuch hygroſcopical obſervations, as may be reduced to hypotheſes. For as I elſewhere diſcourſed concerning barometrical theories, if I may ſo call them; ſo I ſhall here repreſent, concerning hygroſcopical ones, that if a theory or hypotheſis, that is itſelf rational, be found agreable to what happens the moſt uſually in obſervation; it ought not lightly to be rejected, or ſo much as laid aſide, though ſometimes we find particular inſtances, that ſeem to call it in queſtion. For it is very poſſible, that the theory or hypotheſis may be as good, as a wiſe man would require about ſo mutable a ſubject as the weather. And the cauſe aſſigned by the hypotheſis may really act ſuitably to what that requires, though a contrary effect enſue by reaſon of that cauſe's being accidentally maſtered and over-ruled by ſome more powerful cauſe or agent, that happens for that time to invade the air. As we know, that tides do for the main correſpond with the motions of the moon, (whoſe phaſes are therefore argued from them,) and do generally ebb and flow at ſuch times, and in ſuch meaſures, as the theory, that has been grounded on that correſpondency, requires; but yet ſeamen find, that in this, or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other caſually intervening cauſes, do ſometimes both very much diſturb the regular courſe of the tides, and encreaſe or leſſen them.

A
NEW EXPERIMENT
AND
OTHER INSTANCES
OF THE
EFFICACY of the AIR's MOISTURE.

Subjoined by Way of
APPENDIX to his STATICAL HYGROSCOPE.

SINCE it may probably recommend hygroſcopes to you, if that quality of the air, which theſe inſtruments are uſeful to give us an account of, be made appear to be more powerful, and have conſiderabler effects, than is commonly believed; it will not be from my purpoſe to preſent you here ſome inſtances, that have led me to think, that the effects of the moiſture of the air may be conſiderable, not only upon men's healths, but upon ſubjects far leſs tender, and leſs curiouſly contrived, than human bodies. But I hope you will eaſily believe, that by the moiſture of the air, I mean not a mere and abſtracted quality, but moiſt air itſelf, or rather thoſe humid corpuſcles, (chiefly of an aqueous nature,) that abound, and rove to and fro, in our common air.

THAT the moiſture of the air may have no ſmall influence, and uſually a bad one, upon men's healths, is that, which, though experience did not ſo often teach us, I ſhould venture to argue from what I have obſerved of the operation of moiſt air upon the dry and firmly contex parts of animal, and even in thoſe caſes, where, for want of time, or other impediments, this moiſture cannot produce any ſenſible degree of putrefaction.

THAT the ſkins of animals may be eaſily invaded by the moiſt particles of the air, is the more probable, becauſe of the numerouſneſs of their pores, which may be concluded from their hairineſs, or their ſweat, or both. And I formerly obſerved to you, that I found ſheeps-leather to imbibe the moiſture of the air, and encreaſe in weight upon it, as plentifully as almoſt any body I expoſed to it.

BUT to ſhew you, that much cloſer membranes, and which nature made to be impervious to ſuch a liquor as urine itſelf, may be affected by the vapours of the air, I ſhall add, that having purpoſely taken pieces of bladders, fine and well blown, and, as far as appeared, of a very cloſe contexture, and counterpoiſed them in a good balance, I found, according to expectation, that they would conſiderably encreaſe their weight in moiſt, and looſe it again in dry weather; ſo that I might have employed the moſt membranous part of a bladder (for I thought not fit to make uſe of the neck or the adjoining part) to make a ſtatical hygroſcope.

AND as for other membranes and fibres, I ſhall have by and by occaſion to take notice, that even when they are ſtrongly and artificially wreathed together into gut-ſtrings, they may imbibe enough of the moiſture of the air to be broken by it. And I remember, I formerly told you, that I had obſerved lute-ſtrings to grow heavier in moiſt air.

AND whereas bones are by all confeſſed to be the firmeſt and ſolideſt parts of animals, and as it were, the pillars, by which the fabrick is ſuſtained; yet it ſeems, that even they may be pierced into, and ſenſibly affected by the moiſture of the air. For I remember, that having cauſed the ſkeleton of a human body to be ſo made by a famous and very ſkilful artiſt, that, by the help only of ſlender wires artificially ordered, the motions, which the muſcles make of the bones of a living body, might be well imitated in the ſkeleton, I obſerved, that though in dry and fair weather the flexures of the limbs might be readily made, yet in very moiſt weather the joints were not eaſily bent, as if the parts were grown ſtiff and rigid; which ſeemed to proceed hence, that moiſt particles of the air, having plentifully inſinuated themſelves at the pores into the bones, had every way diſtended them, and thereby made the parts bear hard againſt one another, (which they did not at all before) at the junctures or articulations.

BUT it will be the more readily believed, that the moiſture of the air may operate conſiderably upon the tender and curiouſly contrived bodies of men and other animals, if, proceeding to the obſervations I chiefly deſign, I make it appear, that the moiſtening particles, that rove up and down in the air, are able to exerciſe a notable (and, if I may ſo call it, a mechanical) force, even upon inanimate and inorganical bodies: which may well ſuggeſt a ſuſpicion, that hygroſcopes being the proper inſtruments to diſcover a quality in the air, whoſe efficacy reaches farther than is commonly taken notice of, they may in time be found uſeful to divers other purpoſes, beſides thoſe, that relate to the health of men.

THAT wood, eſpecially when it has been ſeaſoned, is a ſolid of a ſtrong and firm contexture, if it were not obvious by the daily uſe made of it in building ſhips, houſes, &c. might be eaſily concluded from the weight or force required to alter its contexture, by making any conſiderable, or, perhaps, ſenſible compreſſion of it. And yet that wood may ſuffer a kind of divulſion of the multitude of its parts, and be manifeſtly diſtended by aqueous corpuſples getting into its pores, I remember, I proved by this experiment. I got a piece of ſound and ſeaſoned wood of about an inch (or an inch and half) in diameter, to be by a ſkilful artiſt made cylindrical, and alſo a ring of ſome ſolid matter, as braſs or ivory, to be exactly turned to fit this cylinder, ſo that it might, without much eaſe, or much difficulty, be put on and taken off again: then we put the turned piece of wood into fair water, and left it to ſoak there for many hours; at the end of which it was viſibly ſwelled: and though I cannot now tell you, (for want of a paper concerning that experiment,) how much it was encreaſed in diameter, yet I well remember, the increment was conſiderable, and that the ring, that was adjuſted to it before, was manifeſtly too little to be put again upon it, or with its orifice to cover the whole baſis of the diſtended cylinder, which afterwards being dried in the air ſhrunk into a capacity of entering the ring again. And in this experiment I took notice, that the great intumeſcence of the wood was not produced all at once, or ſoon after it was put into water, but it ſwelled by degrees, and lay ſoking there many hours, before it arrived at its utmoſt diſtention, the aqueous corpuſcles requiring, it ſeems, ſo much time to inſinuate themſelves ſufficiently into the wood; which argues, that the internal parts were likewiſe affected, though, when even they came to ſwell, they had a good thickneſs of wood about them to hinder their dilatation.

I expect you ſhould now tell me, that this diſtention of ſo firm a body was made by water it ſelf, and not by the humid vapours of the air. On which occaſion I might repreſent to you, that by the ſweating (as men commonly call the adheſion of wateriſh drops to the ſurface) of poliſhed marble and ſome other cold and ſmooth bodies, that ſometimes happens even in the heat of ſummer, if they be cold, and the ambient air be moiſt enough, it appears, that both in hot weather the air may be plentifully ſtocked with aqueous vapours, and that theſe vapours need to do no more, than convene together, to conſtitute viſible and tangible water. And on this occaſion, if I were ſure I had not told you of it already, I ſhould ſubjoin an experiment, which would detect the vulgar error of thoſe, that think the adhering drops, lately mentioned, to ſome come from internal moiſture derived by its preſſion, or percolation, from the marble or the other body they are faſtened to; and at the ſame time I ſhall ſhew (what is not wont to be imagined) that in the heat of ſummer the air is furniſhed with inviſible and yet aqueous ſteams. The experiment I long ſince tried in winter with ſnow and ſalt, included in a glaſs veſſel, and then put to diſſolve in a balance. But becauſe neither ice nor ſnow is at all eaſy to be come by among us in *England* in ſummer; and becauſe at that ſeaſon, the air in fair weather is preſumed to be dry as well as hot, I choſe, within ſome days of Midſummer, and in clear ſun-ſhiny weather, to make the following trial.

WE took a pint glaſs-bottle, and having put into it a convenient quantity of water (for room muſt be left for the ſalt) we placed them and four ounces of beaten ſal armoniack in one ſcale of a good balance, and a counterpoiſe in the other; and then, putting the ſalt into the water, I obſerved, that tho' for a while, the æquilibrium remained, yet when the frigorific mixture had ſufficiently cooled the outſide of the bottle, the roving vapours of the air, that chanced to paſs along the ſurface of the veſſel, were, by the contact of that cold body, arreſted, and turned into a kind of a dew, which from time to time made the ſcale, that held the glaſs, preponderate more and more, and at length the drops growing greater and greater, ran down in ſmall rivulets the ſides of the glaſs, and in leſs than an hour, by my eſtimate, the condenſed ſteams amounted to near a drachm, which weight was afterwards much encreaſed within about two hours more: whereby it ſufficiently appears, both that this dew came from without, (ſince if it had been a tranſudation, it would not have added weight to the ſcale, that received it,) and that there is, even in clear ſummer weather, a vaſt number of moiſt particles diſperſed through the air, ſince in about an hour's time, ſuch a multitude of them, as the liquor produced, may be ſuppoſed to conſiſt of, and may by heat be actually reſolved into, could in courſe come to touch ſo ſmall a ſurface, as that of that part of ſo ſmall a bottle, which contained the frigorific mixture. For the reſt of the veſſels ſurface was not cold enough to condenſe the vapours into liquor. But to return to what we were ſaying of wood ſwelled by water; becauſe, notwithſtanding theſe conſiderations, I am willing to allow, that the experiment of the cylinder does not fully come home to our purpoſe, and that I produced it not ſo much to prove directly the force of moiſt air, as to countenance what I am about to ſay, by ſhewing, what a ſufficient number of aqueous corpuſcles may do in the ſolid wood they penetrate, I ſhall now add ſome inſtances of the force theſe particles may exerciſe upon ſolids, when they invade them but in the form of vapours.

THAT in this form the multitude, figures, and motions of theſe inſinuating particles may enable them to diſplay no ſmall force in their operations on ſome bodies, we have one inſtance, that often happens, though but ſeldom reflected on, in the breaking of the ſtrings of muſical inſtruments, firſt brought to a good tenſion, upon the ſupervening of rainy weather. For the cauſe ſeems to be, that the vapours, that then wander through the air, inſinuating themſelves into theſe ſtrings, (which the muſician often forgets to let down or relax after having ſkrewed them up,) diſtend and ſwell them, and thereby endeavour to ſhorten them, and that ſo forcibly, that they not ſeldom break with a ſmart noiſe and great violence; which, becauſe it happens without any viſible efficient, men commonly think and ſay, that ſuch ſtrings break of themſelves. But to take no farther notice of this popular ſurmize, if we conſider, how much weight ſome of thoſe bigger ſtrings, eſpecially of baſe viols, that have been obſerved to break in rainy weather, will require to ſtretch any of them to a rupture, you will eaſily be induced to think, that this operation of the moiſt air exacts, and therefore argues more than a languid force.

BUT here, probably, you will tell me, that the inſtances you expected were concerning wood, which is a far ſolider body than gut-ſtrings. To this I ſay, that the newly-recited inſtance belongs directly to the title of this paper, and, being above referred to, ought not to be pretermitted. And as to your expecting inſtances concerning wood, I might content myſelf to refer you to what is obſerved about the uneaſy opening and ſhutting ſome doors well adjuſted to the door-caſe in very rainy weather. But though this obſervation favours my deſign, yet I had rather give you inſtances in wood, purpoſely and carefully ſeaſoned. And therefore I ſhall now inform you of theſe two things; one, that I found by trial, as I have elſewhere noted, that wood, counterpoiſed in a good balance, would grow ſenſibly heavier in wet weather, and lighter again in dry; and the other, that, to ſatisfy myſelf yet further, I conſulted an ancient muſician, to whom I had once been a diſciple, and a famous organ-maker, to know, whether they had not obſerved, that the wood itſelf, &c. of muſical inſtruments, would receive ſuch alterations from the moiſture of the air, as might be diſcerned by the ear? upon which enquiries, the maſter of muſick anſwered me, that though metalline ſtrings will not change with the weather, like gut-ſtrings; yet virginals (for inſtance) though furniſhed with wire-ſtrings, will, for the moſt part of them, (for ſome he has obſerved to be ſo well ſeaſoned, that they are not altered by the weather,) be out of tune in wet weather, the ſtrings generally then affording their notes ſharper than they ſhould, or are wont to do. And the organ-maker confeſſed to me, that, upon great changes of weather, divers organs would (after they had been long ago tuned) grow out of tune, and that not only the wooden pipes would be thereby ſwelled, but the metalline pipes untuned.

BUT if bodies be of ſuch a conſtitution, as not only to admit, but aſſiſt the operation of the moiſt air, the penetrancy and efficacy of this may be found much more conſiderable than in the fore-going inſtances. For there are ſome kinds of thoſe marchaſites, that yield vitriol, which, whilſt they lie under ground, or are covered with the ſea-water, on whoſe ſhores they are, in ſome places, to be found, retain a ſtone-like hardneſs, and are often taken for meer ſtones; and yet ſome credible perſons, that are converſant about vitriol, have caſually obſerved, that theſe, being expoſed to the air, would, in tract of time, be ſo penetrated by the moiſt particles of it, though perhaps not meerly as moiſt, that (probably by the help of the vitriolate corpuſcles they met with among the ſtony matter) theſe hard and ſolid marchaſites are brought to ſwell ſo much as to burſt. That this will happen to ſuch kind of ſtones (though they be of a cloſe and heavy nature) by the help of rain, experience has perſuaded me; and that it may alſo happen even to very hard and ſtone-like marchaſites, (for many are not ſuch,) when they are meerly expoſed to the air, I am apt to think upon ſome trials of my own. For from ſhining marchaſites, though but kept in my chamber window, I have had vitriolate efflorescencies, that ſeemed to be produced by the action of the piercing moiſture of the air upon the mineral. And I remember, that very hard and heavy lumps, that were of a marchaſitical ſubſtance, though not at all gliſtering, which ſeemed to be ſtony, were ſo diſpoſed to be wrought on by the air, that though they were kept partly in my own chamber, and partly in other covered places, yet in no very long time they were ſo penetrated by the moiſt corpuſcles of the air, that they were not only burſt, but broken into many pieces; inſomuch that many of them did of themſelves fall off from one another, and ſeveral of the divided portions would eaſily be crumbled betwixt one's fingers. And of ſome of theſe I have obſerved with pleaſure, that a vitriolate ſubſtance was produced more copiouſly in their innermoſt parts, than on, or near their outſide. So that, when I conſidered how great, an external force would have been requiſite to make ſuch a comminution of minerals ſo ſolid and hard, it was obvious for me to look upon the air's moiſture, as capable, when it meets with fitly-diſpoſed bodies, to exerciſe a far greater force, than is wont to be conceived.

To theſe phænomena I might add ſome others to the ſame purpoſe; but becauſe the marchaſites, and other bodies, required to the producing of them, are not eaſy to be come by, and the ſucceſs often exacts a good length of time, I ſhall conclude this paper, by ſubjoining a far ſhorter experiment, that I deviſed not only to ſhew in general, that the moiſture of the air may have a conſiderable efficacy, but to aſſiſt a virtuoſo to make ſome eſtimate in known meaſures of the mechanical force of the aerial moiſture. And though I now find, to my trouble, that I want ſome of the notes, that concern the circumſtances, and the progreſs of the trial, yet enough having eſcaped to furniſh me with the following account of it, what I ſhall ſet down, may, I hope, at leaſt put you in the way of repairing my misfortune.

THINKING it then probable, that ropes themſelves would conſiderably imbibe, and diſmiſs the moiſture of the air, and that ſo, as to ſhrink in rainy weather, though clogged with a weight faſtened at the lower end, I was diſcouraged from attempting the following trial, by conſidering, that the weight would ſtretch the rope, and conſequently hinder the preſumed effect of the air's moiſture to be perceived. For I ſuppoſed, that after a time, this unuſual ſtretch of the rope would ceaſe; and when the weight, as ſuch, could not lengthen it any more, it would then be capable of being contracted or relaxed, according as the weather ſhould be moiſt or dry, and ſo afford me a kind of hygroſcope. Upon theſe grounds, I firſt cauſed a rope, that was about twenty or twenty-two yards in length, but of no great thickneſs, to have one of its ends faſtened to an immoveable body at a convenient height from the ground, and then cauſed a pully to be ſo faſtened to another ſtable body, at the diſtance of eighteen, or twenty yards from the firſt, that the rope, reſting upon the pully, lay almoſt horizontally. But to the end of that part of the rope, which from the pully reached within two or three foot of the ground, was faſtened, by a ring, a leaden weight of at leaſt fifty pound. To which was alſo faſtened a light index placed horizontally, whoſe end moved along an erected board, which, by tranſverſe lines, was divided into inches, and parts of inches, reaching both a good way upwards and downwards, that the index might, within thoſe bounds, have room to play up and down, according to the alterations of the weather.

IT being then ſummer, this trial was made in a garden, though partly under a pent-houſe, that the rope might be more expoſed to the air, than it would have been within doors; and two or three days, if I miſ-remember not the time, were ſpent, before the weight had brought the rope to the utmoſt ſtretch it was able to give it, after which, it began manifeſtly to ſhrink, and lengthen, according to the weather. And I find, in one of my notes, that once I looked, when I was ready to go to bed, upon the ſuſpended weight, and marked, how low it reached upon the divided board; and that a great part of the night having been rainy, looking again about half an hour after eight in the morning, I found the cord ſo ſhrunk, that the weight was raiſed above five inches, and yet the day growing dry and windy, and ſometimes warm, the weight had at night ſtretched the rope more than the moiſture had contracted it the day before.

AFTERWARDS having procured a far greater weight, but therefore unapt to be near ſo much raiſed, I ſubſtituted it in the place of that formerly mentioned; and having ſuffered it to ſtretch the rope as far as it could, I made and regiſtered ſome obſervations, two whereof having been preſerved, I ſhall tranſcribe them juſt as I find them.

June the 4th. AT half an hour after nine of the clock at night, I looked upon the hundred pound weight, that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board, where the bottom of the weight touched; I perceived the ſky, a while after, to grow cloudy and overcaſt, but without rain; wherefore, going to view the weight again, I found it to be riſen a quarter of an inch, or more, and looking on my watch, perceived there had paſſed an hour and quarter ſince the mark was made.

June the 6th. BEING not well yeſterday, the weight was obſerved by two of my ſervants, and it then reſted at the eleventh inch of the erected board. This morning, about eight of clock, I viſited it myſelf, and found it to be riſen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and duſty. The weather being more overcaſt, within ſomewhat leſs than an hour afterwards, I viſited the weight again, (ſome ſcattered drops of rain then beginning to fall) and found it to be riſen about half an inch above the newly-mentioned eighth mark. How much more the rope would have been contracted in ſuch laſting moiſt weather, as uſually happens in winter, I cannot ſay, having been reduced to break off the experiment, upon a removal, I was, long before that ſeaſon, obliged to make.

I am ſorry I cannot add my other obſervations; but theſe I hope may ſuffice to let you ſee, that the force of the air's moiſture is not ſmall, ſince it could raiſe ſuch a weight as an hundred pound, eſpecially conſidering the ſlenderneſs of the rope it affected. For having meaſured the diameter near the weight, I found it (as one of my notes informs me) to be but about the [*] third part of an inch.

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[*] It was $\frac{3}{10}$ and 4 decimal parts of $\frac{1}{10}$.

THE
EXCELLENCY
OF
THEOLOGY,
COMPARED WITH
NATURAL PHILOSOPHY,
(As both are OBJECTS of MEN'S STUDY.)
DISCOURSED OF
In a LETTER to a FRIEND.
TO WHICH ARE ANNEXED
Some Occaſional Thoughts about the EXCELLENCY and GROUNDS of the MECHANICAL HYPOTHESIS.

The PUBLISHER's ADVERTISEMENT to the READER.

WHEN I ſhall have told the reader, that the following diſcourſe was written in the year 1665, while the author, to avoid the great plague, that then raged in *London*, was reduced, with many others, to go into the country, and frequently to paſs from place to place, unaccompanied with moſt of his books; it will not, I preſume, be thought ſtrange, that in the mention of ſome things taken from other writers, as his memory ſuggeſted them, he did not annex in the margent the preciſe places, that are referred to. And upon the ſame ſcore, it ought not to ſeem ſtrange, that he has not mentioned ſome late diſcoveries and books, that might have been pertinently taken notice of, and would well have accommodated ſome parts of his diſcourſe; ſince things, that may thus ſeem to have been omitted, are of too recent a date to have been known to him when he writ. But if it be demanded, why then a diſcourſe finiſhed ſo long ago, did not come abroad much ſooner? I muſt acquaint the reader, that it was chiefly his real concern for the welfare of the ſtudy he ſeems to depreciate, that kept theſe papers ſo long by him. For he reſiſted for ſeveral years the deſires of perſons, that have much power with him, and ſuppreſſed the following diſcourſe, whilſt he feared it might be miſapplied by ſome enemies to experimental philoſophy, that then made a noiſe againſt it, without ſuffering theſe papers to come abroad, till the addreſſes and encomiums of many eminent foreign virtuoſi, and their deſire to be admitted into the Royal Society, had ſufficiently manifeſted, how little its reputation was prejudiced, or like to be endangered, by the attempts of ſome envious or miſinformed perſons. And to this reaſon muſt be added the author's backwardneſs to venture abroad a diſcourſe of an unuſual nature, on which account, among others, he declined to have his name prefixed to it; though now the book is printed, he finds cauſe to fear, that it will not be long concealed; ſince he meets with ſome marginal references to other tracts of his, which (theſe papers having long lain by him) he forgot to have been ſet down for private uſe, and which ſhould not have been expoſed to publick view.

THE

The AUTHOR's PREFACE.

I AM not ſo little acquainted with the temper of this age, and of the perſons, that are likelieſt to be peruſers of the following tract, as not to foreſee it to be probable enough, that ſome will aſk, for what reaſon a diſcourſe of this nature was written at all; and that others will be diſpleaſed, that it has been written by me.

THOSE, that would know, by what inducements my pen was engaged on this ſubject, may be in great part informed by the epiſtle it ſelf, in divers places whereof, as eſpecially about the beginning, and at the cloſe, the motives, that invited me to put pen to paper, are ſufficiently expreſſed. And though ſeveral of thoſe things are peculiarly applied, and (if I may ſo ſpeak) appropriated to the perſon the letter is addreſſed to; yet that under-valuation, I would diſſuade him from, of the ſtudy of things ſacred, is not his fault alone, but is grown ſo rife among many (otherwiſe ingenious) perſons, eſpecially ſtudiers of phyſicks, that I wiſh the enſuing diſcourſe were much leſs ſeaſonable than I fear it is.

BUT I doubt, that ſome readers, who would not think a diſcourſe of this nature needleſs or uſeleſs, may yet not be pleaſed at its being written by one, whom they imagine the acceptance his endeavours have met with, ought to oblige to ſpend his whole time in cultivating that natural philoſophy, which in this letter he would perſuade to quit the precedency, they think it may well challenge, before all other ſorts of learning.

I am not unſenſible of the favourable reception, that the philoſophical papers I have hitherto ventured abroad, have had the happineſs to receive from the curious: but I hope, they will not be diſpleaſed, if I repreſent, that I am no lecturer, or profeſſor of phyſicks, nor have ever engaged myſelf, by any promiſe made to the publick, to confine myſelf, never to write of any other ſubject; nor is it reaſonable, that what I did, or may write, to gratify other men's curioſity, ſhould deprive me of mine own liberty, and confine me to one ſubject; eſpecially, ſince there are divers perſons, for whom I have a great eſteem and kindneſs, who think they have as much right to ſollicit me for compoſures of the nature of this, that they will now have to go abroad, as the virtuoſi have to exact of me phyſiological pieces. And though I be not ignorant, that, in particular, the following diſcourſe, which ſeems to depreciate the ſtudy of nature, may, at firſt ſight, appear ſomewhat improper for a perſon, that has purpoſely written to ſhew the excellence and uſefulneſs of it; yet I confeſs, that upon a more attentive conſideration of the matter, I cannot reject, no, nor reſiſt their reaſons, who are of a quite differing judgment.

AND I. My condition, and my being a ſecular perſon (as they ſpeak) are looked upon as circumſtances, that may advantage an author, that is to write upon ſuch a ſubject as I have handled. I need not tell you, that as to religious books in general, it has been obſerved, that thoſe penned by lay-men, and eſpecially gentlemen, have (*cæteris paribus*) been better entertained, and more effectual, than thoſe of eccleſiaſticks. And indeed it is no great wonder, that exhortations to piety, and diſſuaſions from vice, and from the luſts and vanities of the world, ſhould be the more prevalent for being preſſed by thoſe, who have, and yet decline, the opportunities to enjoy plentifully themſelves the pleaſures they diſſuade others from. And (to come yet cloſer to our preſent purpoſe) though I will not venture to ſay with an excellent divine, that whatever comes out of the pulpit, does with many paſs but for the fooliſhneſs of preaching; yet it cannot well be denied, but that if all other circumſtances be equal, he is the fitteſt to commend divinity, whoſe profeſſion it is not; and that it will ſomewhat add to the reputation of almoſt any ſtudy, and conſequently to that of things divine, that it is praiſed and preferred by thoſe, whoſe condition and courſe of life exempting them from being of any particular calling in the common-wealth of learning, frees them from the uſual temptations to partiality to this or that ſort of ſtudy, which others may be engaged to magnify, becauſe it is their trade or their intereſt, or becauſe it is expected from them; whereas theſe gentlemen are obliged to commend it, only becauſe they really love and value it.

BUT there is another thing, that ſeems to make it yet more fit, that a treatiſe on ſuch a ſubject ſhould be penn'd by the author of this: for profeſſed divines are ſuppoſed to be buſied about ſtudies, that even, by their being of an higher, are confeſſed to be of another nature, than thoſe, that treat of things corporeal. And ſince it may be obſerved, that there is ſcarce any ſort of learned men, that is more apt to undervalue thoſe, that are verſed only in other parts of knowledge, than many of our modern naturaliſts, (who are conſcious of the excellency of the ſcience they cultivate,) it is much to be feared, that what would be ſaid of the pre-eminences of divinity above phyſiology, by preachers (in whom the ſtudy of the latter is thought either but a preparatory thing, or an excurſion) would be looked upon as the deciſion of an incompetent, as well as intereſted judge; and their undervaluations of the advantages of the ſtudy of the creatures would be (as their depreciating the enjoyment of the creatures too often is,) thought to proceed but from their not having had ſufficient opportunities to reliſh the pleaſures of them. But theſe prejudices will not lie againſt a perſon, who has made the indagation of nature ſomewhat more than a parergon, and having, by a not-lazy, nor ſhort enquiry, manifeſted, how much he loves and can reliſh the delight it affords, has had the good fortune to make ſome diſcoveries in it, and the honour to have them publickly, and but too complimentally, taken notice of by the virtuoſi. And it may be not impertinent to add, that thoſe, who make natural philoſophy their miſtreſs, will, probably, be the leſs offended to find her in this tract repreſented, if not as an handmaid to divinity, yet as a lady of a lower rank; becauſe the inferiority of the ſtudy of nature is maintained by a perſon, who, even whilſt he aſſerts it, continues, if not a paſſionate, an aſſiduous courter of nature: ſo that, as far as his example can reach, it may ſhew, that as on the one ſide a man need not be acquainted with, or unfit to reliſh the leſſons taught us in the book of the creatures, to think them leſs excellent than thoſe, that may be learned in the book of the ſcriptures; ſo on the other ſide, the preference of this laſt book is very conſiſtent with an high eſteem and an aſſiduous ſtudy of the firſt.

And if any ſhould here object, that there are ſome paſſages, which I hope are but very few, that ſeem a little too unfavourable to the ſtudy of natural things; I might alledge for my excuſe the great difficulty, that there muſt be in comparing two ſorts of ſtudies, both of which a man much eſteems, ſo to behave one's ſelf, as to ſplit a hair between them, and never offend either of them: but I will rather repreſent, that in ſuch kind of diſcourſes, as the enſuing, it may juſtly be hoped, that equitable readers will conſider, not only what is ſaid, but on what occaſion, and with what deſign it is delivered. Now it is plain by the ſeries of the following diſcourſe, that the phyſeophilus, whom it moſt relates to, was by me looked upon as a perſon, both very partial to the ſtudy of nature, and ſomewhat prejudiced againſt that of the ſcripture; ſo that I was not always to treat with him, as with an indifferent man, but according to the advice given in ſuch caſes by the wiſe, I was (to uſe *Ariſtotle*'s expreſſion) to bend the crooked ſtick the contrary way, in order to the bringing it to be ſtrait, and to depreciate the ſtudy of nature ſomewhat beneath its true value, to reduce a great over-valuer to a juſt eſtimate of it. And to gain the more upon him, I allowed myſelf now and then to make uſe of the contempt he had of the peripatetick and vulgar philoſophy, and in ſome paſſages to ſpeak of them more ſlightingly, than my uſual temper permits, and than I would be forward to do on another occaſion; that, by ſuch a complaiſance for his opinions, I might have riſes to argue with him from them.

But to return to the motives, that were alledged to induce me to the publication of theſe papers, though I have not named them all, yet all of them together would ſcarce have proved effectual, if they had not been made more prevalent by the juſt indignation I conceived, to ſee even inquiſitive men depreciate that kind of knowledge, which does the moſt elevate, as well as the moſt bleſs, mankind, and look upon the nobleſt and wiſeſt employments of the underſtanding, as ſigns of weakneſs in it.

It is not, that I expect, that whatever can be ſaid, and much leſs what I have had occaſion to ſay here, will make proſelytes of thoſe, that are reſolved againſt the being made ſo, and had rather deny themſelves the excellenteſt kinds of knowledge, than allow, that there can be any more excellent, than what they think themſelves maſters of: but I deſpair not, that what is here repreſented, may ſerve to fortify in a high eſteem of divine truths thoſe, that have already a juſt veneration for them, and preſerve others from being ſeduced by injurious, though ſometimes witty inſinuations, to undervalue that kind of knowledge, that is as well the moſt excellent in itſelf, as the moſt conducive to man's happineſs. And for this reaſon I am the leſs diſpleaſed to ſee, that the following letter is ſwelled to a bulk far greater than its being but a letter promiſes, and than I firſt intended. For I confeſs, that when the occaſion happened, that made me put pen to paper, as I chanced to be in a very unſettled condition (which I fear has had too much influence on what I have written) ſo I did not deſign the inſiſting near ſo long upon my ſubject as I have done; but new things ſpringing up, if I may ſo ſpeak, under my pen, I was content to allow them room in my paper, becauſe writing as well for my own ſatiſfaction, as for that of my friend, I thought it would not be uſeleſs to lay before my own eyes, as well as his, thoſe conſiderations, that ſeemed proper to juſtify to myſelf, as well as to him, the preference I gave divine truths (before phyſiological ones) and to confirm myſelf in the eſteem I had for them. And though I freely confeſs, that the following diſcourſe doth not conſiſt of nothing but ratiocinations, and conſequently is not altogether of an uniform contexture; yet that will, I hope, be thought no more than was fit in a diſcourſe, deſigned not only to convince, but to perſuade: which if it prove ſo happy as to do, as I hope the peruſer will have no cauſe to regret the trouble of reading it, ſo I ſhall not repent that of writing it.

The INTRODUCTION.

SIR,

I Hoped you had known me better, than to doubt in good earneſt, how I reliſhed the diſcourſe your learned friend entertained us with yeſter-night. And I am the more troubled at your queſtion, becauſe your way of enquiring, how much your friend's diſcourſe obtained of my approbation, gives me cauſe to fear, that you vouchſafe it more of yours than I could wiſh it. But before I can ſafely offer you my ſenſe of the diſcourſes, about which you deſire to know it, I muſt put you in mind, that they were not all upon one ſubject, nor of the ſame nature: and I am enough his ſervant to acknowledge, without the leaſt reluctancy, that he is wont to ſhew a great deal of wit, when he ſpeaks like a naturaliſt, only of things purely phyſical; and when he is in the right, ſeldom wrongs a good cauſe by his way of managing it. But as for thoſe paſſages, wherein he gave himſelf the liberty of diſparaging the learned Dr. *N.* only becauſe that doctor cultivates theological, as well as phyſical ſtudies, and does both oftentimes read books of devotion, and ſometimes write them; I am not ſo much a courtier, as to pretend, that I liked them. 'Tis true, he did not deny the doctor to be a learned, and a witty man, as indeed the wiſe providence of God has ſo ordered it, that to ſtop the bold mouths of ſome, who would be eaſily tempted to imagine, and more eaſily to give out, that none are philoſophers, but ſuch as, like themſelves, deſire to be nothing: elſe our nation is happy in ſeveral men, who are as eminent for humane, as ſtudious of divine learning; and as great a veneration as they pay to *Moſes*, and St. *Paul*, are as well verſed in the doctrine of *Ariſtotle*, and of *Euclid*; nay, of *Epicurus* and *Des Cartes* too, as thoſe, that care not to ſtudy any thing elſe. But though, for this reaſon, Mr. *N.* had not the confidence to deſpiſe the doctor, and ſome of his reſemblers, whom he took occaſion to mention; yet he too plainly diſcloſed himſelf to be one of thoſe, who, though they will not deny, but that ſome, who own a value for theology, are men of parts; yet they talk, as if ſuch perſons were ſo, in ſpight of their being religiouſly given; that being, in their opinion, ſuch a blemiſh, that a man muſt have very great abilities otherwiſe, to make amends for the diſadvantage of valuing ſacred ſtudies, and ſurmount the diſparagement it procures him. Wherefore, ſince this diſdainful humour begins to ſpread, much more than I could wiſh it did, among different ſorts of men, among whom I ſhould be glad not to find any naturaliſts; and ſince the queſtion you aſked me, and the eſteem you have for your friend, makes me fear you may look on it with very favourable eyes; I ſhall not decline the opportunity you put into my hands of giving you, together with a profeſſion of my diſlike of his practice, ſome of my reaſons for that diſlike; and the rather, becauſe I may do it without too much exceeding the limits of an epiſtle, or thoſe which the haſte, wherewith I muſt write this, does preſcribe to me. For your friend does not oppoſe, but only undervalue theology; and profeſſing to believe the ſcriptures, (which I ſo far credit, as to think he believes himſelf when he ſays ſo) we agree upon the principles: ſo that I am not to diſpute with him, as againſt an atheiſt, that denies the author of nature, but only againſt a naturaliſt, that over-values the ſtudy of it. And the truths of theology are things, which I need not bring arguments for, but am allowed to draw arguments from them.

But though, as I juſt now intimated, I deſign brevity; yet, for fear the fruitfulneſs, and importance of my ſubject, ſhould ſuggeſt things enough to me, to make ſome little method requiſite to keep them from appearing confuſed; I ſhall divide the following epiſtle into two diſtinct parts. In the former of which, I ſhall offer you the chief poſitive conſiderations, by which I would repreſent to you the ſtudy of divinity, as preferable to that of phyſick: And, in the ſecond part, I ſhall conſider the allegations, that I foreſee your friend may interpoſe, in favour of natural philoſophy. From which diſtribution you will eaſily gather, that the motives on the one hand, and the objections on the other, will challenge to themſelves diſtinct ſections, in the reſpective parts whereto they belong. So that, of the order of the particulars you will meet with, I ſhall not need to trouble you with any further account.

THE EXCELLENCY OF THEOLOGY:

OR,

The PRE-EMINENCE of the STUDY OF DIVINITY, above that of NATURAL PHILOSOPHY.

THE FIRST PART.

TO addreſs myſelf then, without any farther circumſtance, or preamble, to the things themſelves, that I mainly intend in this diſcourſe, I conſider in the general, that as there are ſcarce any motives accounted fitter to engage a rational man in a ſtudy, than that the ſubject is noble, that it is his duty to apply himſelf to it, and that his proficiency in it will bring him great advantages; ſo there is not any of theſe three inducements, that does not concur, in a very plentiful meaſure, to recommend to us the ſtudy of theological truths.

SECTION I.

AND firſt, the excellency and ſublimity of the object we are invited to contemplate, is ſuch, that none, that does truly acknowledge a deity, can deny, but that there is no ſpeculation, whoſe object is comparable, in point of nobleneſs, to the nature and attributes of God. The ſouls of inquiſitive men are commonly ſo curious, to learn the nature and condition of ſpirits, as that the over-greedy deſire to diſcover ſo much, as that there are other ſpiritual ſubſtances, beſides the ſouls of men, has prevailed with too many to try forbidden ways of attaining ſatisfaction; and many have choſen, rather to venture the putting themſelves within the power of dæmons, than remain ignorant whether or no there are any ſuch beings: as I have learned by the private acknowledgments made me of ſuch unhappy (though not unſucceſsful) attempts, by divers learned men, (both of other profeſſions, and that of phyſick,) who themſelves made them in differing places, and were perſons neither timorous, nor ſuperſtitious: (but this only upon the by.) And certainly that man muſt have as wrong, as mean a notion of the deity, and muſt but very little conſider the nature and attributes of that infinitely perfect Being, and as little the nature and infirmities of man, who can imagine the divine perfections to be ſubjects, whoſe inveſtigation a man may (inculpably) deſpiſe, or be ſo much as fully ſufficient for. Not only the ſcripture tells us, That his greatneſs is incomprehenſible, and Pſal. cxlv. his wiſdom is inſcrutable; That he humbles Pſ. cxlvii. 5. himſelf to look into (or upon) the heavens and the earth; Pſ. cxiii. 6. and, That not only this, or that man, but all the nations of the world are, in Iſa. xl. 15. compariſon of him, but like the ſmall drop of a bucket, or the ſmaller duſt of a balance: but even the heathen philoſopher, who wrote that eloquent book *de Mundo*, aſcribed to *Ariſtotle* in his riper years, ſpeaks of the power, and wiſdom, and amiableneſs of God, in terms little leſs lofty, though neceſſarily inferior to ſo infinitely ſublime a ſubject; which they, that think they can, eſpecially without revelation, ſufficiently underſtand, do very little underſtand themſelves.

BUT perhaps your friend will object, that, to the knowledge of God there needs no other than natural theology; and I readily confeſs, being warranted by an apoſtle, that the γνωστὸν τοῦ Θεοῦ was not unknown to the heathen philoſophers; Rom. i. 19. and that ſo much knowledge of God is attainable by the light of nature, duly employed, as to encourage men to exerciſe themſelves, more than moſt of them do, in that nobleſt of ſtudies, and render their being no proficients in it, injurious to themſelves, as well as to their maker. But notwithſtanding this, as God knows himſelf infinitely better than purblind man knows him, ſo the informations he is pleaſed to vouchſafe us, touching his own nature, and attributes, are exceedingly preferable to any account, that we can give ourſelves of him, without him. And, methinks, the differing proſpects we may have of heaven, may not ill adumbrate to us the differing diſcoveries, that may be made of the attributes of its maker. For as though a man may, with his naked eye, ſee heaven to be a very glorious object, enobled with radiant ſtars of ſeveral ſorts; yet, when his eye is aſſiſted with a good teleſcope, he cannot only diſcover a number of ſtars, (fixed and wandering,) which his naked eye would never have ſhewn him; but thoſe planets, which he could ſee before, will appear to him much bigger, and more diſtinct: ſo, although bare reaſon, well improved, will ſuffice to make a man behold many glorious attributes in the deity; yet the ſame reaſon, when aſſiſted by revelation, may enable a man to diſcover far more excellencies in God, and perceive them, he contemplated before, far greater and more diſtinctly. And to ſhew how much a dim eye, illuminated by the ſcriptures, is able to diſcover of the divine perfections, and how unobvious they are to the moſt piercing philoſophical eyes, that enjoy but the dim light of nature; we need but conſider, how much more ſuitable conceptions and expreſſions concerning God are to met with in the writings of thoſe fiſhermen and others, that penned the new teſtament, and thoſe illiterate Chriſtians, that received it, than amongſt the moſt civilized nations of the world (ſuch as anciently the Greeks and Romans, and now the Chineſes and Eaſt-Indians) and among the eminenteſt of the wiſe-men and philoſophers themſelves, (as *Ariſtotle*, *Homer*, *Heſiod*, *Epicurus*, and others.)

BESIDES that the book of ſcripture diſcloſes to us much more of the attributes of God, than the book of nature, there is another object of our ſtudy, for which we muſt be entirely beholden to theology: for though we may know ſomething of the nature of God by the light of reaſon, yet we muſt owe the knowledge of his will, or poſitive laws, to his own revelation. And we may gueſs, how curious great princes and wiſe men have been to inform themſelves of the conſtitutions eſtabliſhed by wiſe and eminent legiſlators; partly by the frequent travels of the ancient ſages and philoſophers into foreign countries, too bſerve their laws and government, as well as bring home their learning; and partly by thoſe royal and ſumptuous expences, at which that great and learned monarch *Ptolomeus Philadelphus* ſtuck not to procure an authentick copy of the law of *Moſes*, whom he conſidered but as an eminent legiſlator. But certainly that, and other laws recorded in the bible, cannot but appear more noble and worthy objects of curioſity to us Chriſtians, who know them to proceed from an omniſcient deity, who being the author of mankind, as well as of the reſt of the univerſe, cannot but have a far perfecter knowledge of the nature of man, than any other of the law-givers, or all of them put together can be conceived to have had.

BUT there is a farther diſcovery of divine matters, wherewith we are alſo gratified by theology: for beſides what the ſcripture teaches us of the nature and the will of God, it contains divers hiſtorical accounts (if I may ſo call them) of his thoughts and actions. The great *Alexander* thought himſelf nobly employed, when he read the Grecian actions in *Homer*'s verſes; and, to know the ſentiments of great and wiſe perſons upon particular occaſions, is a curioſity ſo laudible, and ſo worthy of an inquiſitive ſoul, that the ſouthern queen has been more praiſed than admired, for coming from the remoter parts of the earth, to hear the wiſdom of *Solomon*. Now the ſcripture does in many places give our curioſity a nobler employment, and thereby a higher ſatiſfaction, than the king of *Macedon*, or the queen of *Sheba* could enjoy; for in many places it does, with great clearneſs and ingenuity, give us accounts of what God himſelf hath declared of his own thoughts, of divers particular perſons and things, and relates, what he, that knows and commands all things, was pleaſed to ſay and do upon particular occaſions. Of this ſort of paſſages are the things recorded to have been ſaid by God to *Noah*, about the ſinful world's ruin, and that juſt man's preſervation; and to *Moſes* in the caſe of daughters of *Zelophebad*. And of this ſort are the conferences, mentioned to have paſſed betwixt God and *Abimelech*, concerning *Abraham*'s Wife; betwixt God and *Abraham*, touching the deſtruction of *Sodom*; betwixt God and *Solomon*, about that king's happy choice; betwixt God and *Jonah*, about the fate of the greateſt city of the world: and above all theſe, thoſe two ſtrange and matchleſs paſſages, the one in the firſt book of *Kings*, touching the ſeducing ſpirit, that undertook to ſeduce *Ahab*'s prophets; and the other, that yet more wonderful relation of what paſſed betwixt God and Satan, wherein the deity vouchſafes, not only to praiſe, but (if I may ſo ſpeak with reverence) to glory in a mortal. And the being admitted to the knowledge of theſe tranſactions of another world, (if I may ſo call them) wherein God has been pleaſed to diſcloſe himſelf ſo very much, is an advantage afforded us by the ſcripture, of ſo noble a nature, and ſo unattainable by the utmoſt improvement we ourſelves can make of our own reaſon, that, did the ſcripture contain nothing elſe, that were very conſiderable, yet that book would highly deſerve our curioſity and gratitude.

Geneſ. vi.
Numb. xxvii. 7.
Geneſ. xx.
Geneſ. xviii.
1 Kings iii.
Jonah iv.
1 Kings xxii. from ver. 19. to ver. 24.
Job i. 6, 7, &c.
Job ii. 3.

AND on this occaſion, I muſt by no means leave unobſerved another advantage, that we have from ſome diſcourſes made us in the bible, ſince it too highly concerns us, not to be a very great one; and it is, that the ſcripture declares to us the judgment, that God is pleaſed to make of ſome particular men, upon the eſtimate of their life and deportment. For though reaſon alone, and the grounds of religion in general, may ſatiſfy us in ſome meaſure, that God is good and merciful, and therefore it is likely he may pardon the ſins and frailties of men, and accept of their imperfect ſervices; yet beſides that we do not know, whether he will pardon, unleſs we have his promiſe of it; beſides this, (I ſay) though by vertue of general revelation, ſuch as is pretended to in divers religions, we may be aſſured, that God will accept, forgive, and reward thoſe, that ſincerely obey him, and perform the conditions of the covenant, whether it be expreſs, or implicite, that he vouchſafes to make with them; yet ſince it is he, that is the judge of the performance of the conditions, and of the ſincerity of the perſon; and ſince he is omniſcient, and a Καρδιογνώστης and ſo may know more ill of us, than even we know of ourſelves; a concerned

See Heb. v. 9.
Pſal. ciii. 17, 18.
Acts i. 21.

1 Joh. iii. 20. conſcience may rationally doubt, whether in God's eſtimate any particular man was ſo ſincere as to be accepted: But when he himſelf is pleaſed to give elogiums (if I may with due reſpect ſo ſtyle them) to *David*, *Job*, *Noah*, *Daniel*, &c. whilſt they were alive, and to others after they were dead, (and conſequently having finiſhed their courſe, were paſſed into an irreverſible ſtate) we may learn with comfort, both that the performance of ſuch an obedience, as God will accept, is a thing really practicable by men; and that even great ſins and miſdemeanors are not (if ſeaſonably repented of) certain evidences, that a man ſhall never be happy in the future life. And it ſeems to be for ſuch an uſe of conſolation to frail men, (but not at all to encourage licentious ones) that the lapſes of holy perſons are ſo frequently recorded in the ſcriptures. And bating thoſe divine writings, I know no books in the world, nor all of them put together, that can give a conſidering Chriſtian, who has due apprehenſions of the inexpreſſible happineſs or miſery of an immortal ſtate in heaven or in hell, ſo great and well grounded a conſolation, as may be derived from three or four lines in St. *John*'s apocalypſe, where he ſays, "That Revel. vii. 9. "he ſaw in heaven a great multitude, not to "be numbered of all nations, and tribes, and "people, and tongues, ſtanding before the "throne, and before the lamb, clothed in "white robes, with palms (the enſigns of "victory) in their hands;" and the praiſes of God and of the lamb in their mouths. For from thence we may learn, that heaven is not reſerved only for prophets, and apoſtles, and martyrs, and ſuch extraordinary perſons, whoſe ſanctity the church admires, but that, through God's goodneſs, multitudes of his more imperfect ſervants have acceſs thither.

THOUGH the infinite perfections and prerogatives of the deity be ſuch, that theology itſelf can, no more than philoſophy, afford us another object for our ſtudies, any thing near ſo ſublime and excellent, as what it diſcloſes to us of God; yet divinity favours us with ſome other diſcoveries, namely, about angels, the univerſe, and our own ſouls, which, though they muſt needs be inferior to the knowledge of God himſelf, are, for the nobleneſs of their objects, or for their importance, highly preferable to any, that natural philoſophy has been able to afford its votaries.

BUT before I proceed to name any more particulars, diſcloſed to us by revelation, it will be requiſite, for the prevention or removal of a prejudice, to mind you, that we ſhould not make our eſtimates of the worth of the things we owe to revelation, by the impreſſions they are wont now to make upon us chriſtians, who learned divers of them in our catechiſms, and perhaps have ſeveral times met with moſt of the reſt in ſermons, or theological books. For it is not to be admired, that we ſhould not be ſtrongly affected at the mention of thoſe truths, which (how valuable ſoever in themſelves) were for the moſt part taught us when we were either children, or too youthful to diſcern and prize their excellency and importance. So that though afterwards they were preſented to our riper underſtanding, yet their being by that time become familiar, and our not remembering, that we ignored them, kept them from making any vigorous impreſſions on us. Whereas, if the ſame things had been (with circumſtances evincing their truth) diſcovered to ſome heathen philoſopher, or other vertuous and inquiſitive man, who valued important truths, and had nothing but his own reaſon to attain them with, he would queſtionleſs have received them with wonder and joy. Which to induce us to ſuppoſe we have ſundry inſtances, both in the records of the primitive times, and in the recent relations of the converſion of men to Chriſtianity among the people of *China*, *Japan*, and other literate nations. For though bare reaſon cannot diſcover theſe truths, yet when revelation has once ſufficiently propoſed them to her, ſhe can readily embrace, and highly value divers of them; which being here intimated once for all, I now advance to name ſome of the revelations themſelves.

AND firſt, as for angels, I will not now queſtion, whether bare reaſon can arrive at ſo much as to aſſure us, that there are ſuch beings in *rerum naturâ*. For though reaſon may aſſure, that their exiſtence is not impoſſible, and perhaps too, not improbable; yet I doubt, whether 'twere to meer ratiocination, or clear experience, or any thing elſe but revelation, convey'd to them by imperfect tradition, that thoſe Matth. xxvi. 53. heathen philoſophers, who believed, that there Dan. vii. 10. were ſeparate ſpirits other than humane, owed that perſuaſion; and particularly as to good Joh. i. 3. angels, I doubt, whether thoſe antient ſages had Heb. i. 7. any cogent reaſons, or any convincing hiſtorical Luke xx. 35, 36. proofs, or, in ſhort, any one unqueſtionable evidence Col. i. 16. of any kind, to ſatisfy a wary perſon ſo Matth. xxiv. 36. much as of the being (much leſs to give a farther Mark xiii. 32. account) of thoſe excellent ſpirits. Whereas theology Matth. xviii. 10. is enabled by the ſcripture to inform us, that not only there are ſuch ſpirits, but a Iſa. vi. 2, 3. vaſt multitude of them; that they were made Matth. vi. 10. by God and Chriſt, and are immortal, and propagate not their ſpecies; and that theſe ſpirits 2 Sam. xiv. 20. have their chief reſidence in heaven, and enjoy Mark xiii. 32. the viſion of God, whom they conſtantly praiſe, and punctually obey, without having ſinned a- 2 King. xix. 35. gainſt him; that alſo theſe good angels are very 1 Theſſ. iv. 16. intelligent beings, and of ſo great power, that Jude ix. one of them was able in a night to deſtroy a vaſt Dan. x. 13, 21. army; that they have degrees among themſelves, are enemies to the devils, and fight a- Col. i. 16. gainſt them; that they can aſſume bodies ſhaped Revel. xii. 7. like ours, and yet diſappear in a trice; that they Acts xii. 7, 8, 9, 10. are ſometimes employed about human affairs, and that not only for the welfare of empires and Dan x. 13. kingdoms, but to protect and reſcue ſingle good men. And though they are wont to appear in a Acts xii. 11. dazling ſplendor, and an aſtoniſhing majeſty, 2 Kings vi. 17. yet they are all of them miniſtring ſpirits, employed for the good of the deſigned heirs of Luke xxiv. 4. ſalvation. And they do not only refuſe men's Judg. xiii. 6. adoration, and admoniſh them to pay it unto God; but, as they are in a ſenſe made by Jeſus Heb. i. 14. Chriſt, who was true man as well as God; ſo Rev. xix. 10. they do not only worſhip him, and call him ſim- Rev. xxii. 9.

Matth. xxviii. 6. Rev. xix. 10.

ply, as his own followers were wont to do, the Lord, but ſtile themſelves fellow ſervants to his diſciples.

Joh. i. 3. Coloſ. i. 16. Matth. viii. 7. Luke iv. 33. Joh. viii. 34. 1 Pet. v. 8. 2 Cor. xi. 3. Rev. xii. 9. Rev. xii. 7. Matth. xxv. 41. 1 Joh. iii. 8. Jude 6. Mark v. 9, 10, 13. Jam. iv. 7. 1 Pet. v. 9. 1 Cor. vi. Matth. xxv. 41. Jam. ii. 19. 2 Pet. ii. 4. Jude 6. 13. Matth. xxv. 41.

And as for the other angels, though the Gentiles, as well philoſophers as others, were commonly ſo far miſtaken about them, as to adore them for true gods, and yet many of them to doubt whether they were immortal; the ſcripture informs us, that they are not ſelf-originated, but created beings; that however a great part of mankind worſhip them, they are wicked and impure ſpirits, enemies to mankind, and ſeducers of our firſt parents to their ruin; that though they beget and promote confuſion among men, yet they have ſome order among themſelves, as having one chief, or leader; that they are evil ſpirits, not by nature, but apoſtacy; that their power is very limited, inſomuch that a legion of them cannot invade ſo contemptible a thing as a herd of ſwine, without particular leave from God; that not only good angels, but good men, may, by reſiſting them, put them to flight, and the ſincere Chriſtians, that worſted them here, will be among thoſe, that ſhall judge them hereafter; that their being immortal, will make their miſery ſo too; that they do themſelves believe, and tremble at thoſe truths, they would perſuade men to reject; and that they are ſo far from being able to confer that happineſs, which their worſhippers expect from them, that themſelves are wretched creatures, reſerved in chains of darkneſs to the judgment of the great day; at which they ſhall be doomed to ſuffer everlaſting torments, in the company of thoſe wicked men, that they ſhall have prevailed on.

We may farther conſider, that as to things corporeal themſelves, which the naturaliſt challenges as his peculiar theme, we may name particulars, and thoſe of the moſt comprehenſive nature, and greateſt importance, whoſe knowledge the naturaliſt muſt owe to theology. Of which truths I ſhall content my ſelf to give a few inſtances in the world itſelf, or the univerſal aggregate of things corporeal; that being looked upon as the nobleſt and chiefeſt object, that the phyſicks afford us to contemplate.

And firſt, thoſe that admit the truths revealed by theology, do generally allow, that God is not only the author, but creator of the world. I am not ignorant of what *Anaxagoras* taught, of what he called νȣ͂ς — (and *Tully* mentions) in the production of the world; and that what many other Grecians afterwards taught of the world's eternity, is peculiarly due to *Ariſtotle*, who does little leſs than brag, that all the philoſophers that preceded him were of another mind. Nor will I here examine (which I elſewhere do) whether, and how far, by arguments meerly phyſical, the creation of the world may be evinced. But whether or no meer natural reaſon can reach ſo ſublime a truth, yet it ſeems not, that it did actually, where it was not excited by revelation-diſcovery. For though many of the ancient philoſophers believed the world to have had a beginning, yet they all took it for granted, that matter had none; nor does any of them, that I know of, ſeem to have ſo much as imagined, that any ſubſtance could be produced out of nothing. Thoſe, that aſcribe much more to God than *Ariſtotle*, make him to have given form only, not matter to the world, and to have but contrived the pre-exiſtent matter into this orderly ſyſtem we call the univerſe.

Next, whereas very many of the philoſophers, that ſucceeded *Ariſtotle*, ſuppoſe the world to have been eternal; and thoſe, that believed it to have been produced, had not the confidence to pretend to the knowing how old it was; unleſs it were ſome extravagant ambitious people, ſuch as thoſe fabulous Chaldeans, whoſe fond acccount reached up to forty thouſand or fifty thouſand years: theology teaches us, that the world is very far from being ſo old by thirty or forty thouſand years as they, and by very many ages, as divers others have preſumed: and does, from the ſcripture, give us ſuch an account of the age of the world, that it has ſet us certain limits, within which ſo long a duration may be bounded, without miſtaking in our reckoning. Whereas philoſophy leaves us to the vaſtneſs of indeterminate duration, without any certain limits at all.

The time likewiſe, and the order, and divers other circumſtances of the manner, wherein the fabrick of the world was compleated, we owe to revelation; bare reaſon being evidently unable to inform us of particulars, that preceded the origin of the firſt man; and though I do not think religion ſo much concerned, as many do, in their opinion and practiſe, that would deduce particular theorems of natural philoſophy from this or that expreſſion of a book, that ſeems rather deſigned to inſtruct us about ſpiritual than corporeal things. I ſee no juſt reaſon to embrace their opinion, that would ſo turn the two firſt chapters of *Geneſis*, into an allegory, as to overthrow the literal and hiſtorical ſenſe of them. And though I take the ſcripture to be mainly deſigned to teach us nobler and better truths, than thoſe of philoſophy; yet I am not forward to condemn thoſe, who think the beginning of *Geneſis* contains divers particulars, in reference to the origin of things, which though not unwarily, or alone to be urged in phyſicks, may yet afford very conſiderable hints to an attentive and inquiſitive peruſer.

Τρόχος τῆς γενέσεως. Jam. iij. 6.

2 Pet. iii. 7, 10, 13.

And as for the duration of the world, which was by the old philoſophers held to be interminable, and of which the Stoicks opinion, that the world ſhall be deſtroyed by fire, (which they held from the *Jews*) was phyſically precarious; theology teaches us expreſly from divine revelation, that the preſent courſe of nature ſhall not laſt always, but that one day this world, or at leaſt, this vortex of ours, ſhall either be aboliſhed by annihilation, or, which ſeems far more probable, be innovated, and, as it were, transfigured, and that, by the intervention of that fire, which ſhall diſſolve and deſtroy the preſent frame of nature: ſo that either way, the preſent ſtate of things, (as well natural as political) ſhall have an end.

And as theology affords us theſe informations about the creatures in general; ſo touching the chiefeſt and nobleſt of the viſible ones, men, revelation diſcovers very plainly divers very important things, where reaſon muſt needs be in the dark.

AND firſt, touching the body of man; the Epicureans attributed its original, as that of all things elſe, to the caſual concourſe of atoms; and the Stoicks abſurdly and injuriouſly enough (but much more pardonably than their follower herein, Mr. *Hobbes*) would have men to ſpring up like muſhrooms out of the ground; and whereas other philoſophers maintain conceits about it, too wild to be here recited; the book of *Geneſis* aſſures us, that the body of man was firſt formed by God in a peculiar manner, Gen. ii. 7. of a terreſtrial matter; and it is there deſcribed, as having been perfected before the ſoul was united to it. And as theology thus teaches us, how the body of man had its firſt beginning; ſo it likewiſe aſſures us, what ſhall Acts xxiv. 15. become of the body after death, though bare natural reaſon will ſcarce be pretended to reach to ſo abſtruſe and difficult an article as that of a reſurrection; which, when propoſed by St. Acts xvii. 20, 32. *Paul*, produced among the Athenian philoſophers nothing elſe but wonder or laughter.

NOT to mention, that theology teaches us divers other things about the origin and condition of men's bodies; as, that all mankind Gen. ii. Acts xvii. 26. is the offspring of one man and one woman; that the firſt woman was not made of the ſame matter, nor after the ſame manner as the firſt man, but was afterwards taken from his ſide; Gen. ii. 21, 22. that both *Adam* and *Eve* were not, as many Epicureans and other philoſophers fancied, that the firſt men were firſt infants; whence they did, as we do, grow by degrees, to be mature and compleat human perſons, but were made Acts xxv. 15. ſo all at once; and that hereafter, as all mens bodies ſhall riſe again, ſo they ſhall all (or at Luke xx. 35, 36. leaſt, all thoſe of the juſt) be kept from ever dying a ſecond time.

AND as for the human ſoul, though I willingly grant, that much may be deduced from the light of reaſon only, touching its exiſtence, properties and duration; yet divine revelation teaches it us with more clearneſs, and with greater authority; as ſure he, that made our ſouls, and upholds them, can beſt know what they are, and how long he will have them laſt. And as the ſcripture expreſsly teaches us, that the rational ſoul is diſtinct from the body, as Matth. x. 28. not being to be deſtroyed by thoſe very enemies, that kill the body; ſo about the origin of this immortal ſoul (about which philoſophers can give us but wide and precarious conjectures) theology aſſures us, that the ſoul of man had not ſuch an origination, as thoſe of other animals, but was God's own immediate work- Gen. ii. 7. Zek. xii 1. manſhip, and was united to the body already formed: and yet not ſo united, but that upon Luke xx. 35, 36. their divorce, ſhe will ſurvive, and paſs into a Matth. xxv. 46. ſtate, in which death ſhall have no power over her.

I expect you will here object, that for the knowledge of the perpetual duration of ſeparate ſouls, we need not be beholden to the ſcripture, ſince the immortality of the ſoul may be ſufficiently proved by the ſole light of nature, and particularly has been demonſtrated by your great *Des Cartes*. But you muſt give me leave to tell you, that beſides that a matter of that weight and concernment cannot be too well proved, and conſequently ought to procure a welcome for all good mediums of probation; beſides this, I ſay, I doubt many Carteſians do, as well as others, miſtake both the difficulty under conſideration, and the ſcope of *Des Cartes*'s diſcourſe. For I grant, that by natural philoſophy alone, the immortality of the ſoul may be proved againſt its uſual enemies Atheiſts and Epicureans. For the ground, upon which theſe men think it mortal, being, that it is not a true ſubſtance, but only a modification of the body, which conſequently muſt periſh, when the frame or ſtructure of the body, whereto it belongs, is diſſolved; their ground being this, I ſay, if we can prove, by ſome intellectual operations of the rational ſoul, which matter, however modified, cannot reach, that it is a ſubſtance diſtinct from the human body, there is no reaſon, why the diſſolution of the latter ſhould infer the deſtruction of the former, which is a ſimple ſubſtance, and as real a ſubſtance as matter it ſelf, which yet the adverſaries affirm to be indeſtructible. But though by the mental operations of the rational ſoul, and perhaps by other mediums it may, againſt the Epicureans, and other mere naturaliſts, who will not allow God to have any thing to do in the caſe, be proved to be immortal in the ſenſe newly propoſed; yet the ſame proofs will not evince, that abſolutely it ſhall never ceaſe to be, if we diſpute with philoſophers, who admit, as the Carteſians and many others do, that God is the ſole creator and preſerver of all things. For how are we ſure, but that God may have ſo ordained, that though the ſoul of man, by the continuance of his ordinary and upholding concourſe, may ſurvive the body, yet, as it is generally believed not to be created, till it be juſt to be infuſed into the body; ſo it ſhall be annihilated, when it parts with the body, God withdrawing at death that ſupporting influence, which alone kept it from relapſing to its firſt nothing. Whence it may appear, that notwithſtanding the phyſical proofs of the ſpirituality and ſeparableneſs of the human ſoul, we are yet much beholden to divine revelation for aſſuring us, that its duration ſhall be endleſs. And now to make good what I was intimating above, concerning the Carteſians, and the ſcope of *Des Cartes*'s demonſtration, I ſhall appeal to no other than his own expreſſions to evince, that he conſidered this matter for the main as we have done, and pretended to demonſtrate, that the ſoul is a diſtinct ſubſtance from the body; but not that abſolutely ſpeaking it is immortal. *Cur* (anſwers that excellent author) *de immortalitate animæ nihil ſcripſerim, jam dixi in ſynopſi mearum Meditationum. Quod ejus ab omni corpore diſtinctionem ſatis probaverim, ſupra oſtendi. Quod vero additis, ex diſtinctione animæ à corpore non ſequi ejus immortalitatem, quia nihilominus dici poteſt, illam à deo talis naturæ factam eſſe, ut ejus duratio ſimul cum duratione vitæ corporeæ finiatur, fateor à me refelli non poſſe. Neque enim tantum mihi aſſumo, ut quicquam de iis, quæ à libera Dei voluntate dependent, humanæ rationis vi determinare*

Des Cartes reſponſione ad objectiones ſecundas, pag. m. 95.

minare aggrediar. Docet naturalis cognitio, &c. Sed ſi de abſoluta Dei poteſtate quæratur, an forte decreverit, ut humanæ animæ iiſdem temporibus eſſe deſinant, quibus corpora quæ illis adjunxit; ſolius Dei eſt, reſpondere. And if he would not aſſume to demonſtrate by natural reaſon ſo much as the exiſtence of the ſoul after death, unleſs upon a ſuppoſition; we may well preſume, that he would leſs take upon him to determine, what ſhall be the condition of that ſoul after it leaves the body. And that you may not doubt of this, I will give you for it his own confeſſion, as he freely writ it in a private letter to that admirable lady, the princeſs *Elizabeth*, firſt daughter to *Frederick* king of *Bohemia*, who ſeems to have deſired his opinion on that important queſtion, about which he ſends her this anſwer, *Pour ce qui, &c. i. e.* As to the ſtate of the ſoul after this life, my knowledge of it is far inferior to that of monſieur (he means Sir *Kenelm*) *Digby*. For, ſetting aſide that, which religion teaches us of it, I confeſs, that by mere natural reaſon we may indeed make many conjectures to our own advantage, and have fair hopes, but not any aſſurance. And accordingly in the next clauſe he gives the imprudence, of quitting what is certain for an uncertainty, as the cauſe why, according to natural reaſon, we are never to ſeek death.

NOR do I wonder he ſhould be of that mind. For all, that mere reaſon can demonſtrate, may be reduced to theſe two things; one, that the rational ſoul, being an incorporeal ſubſtance, there is no neceſſity, that it ſhould periſh with the body; ſo that, if god have not otherwiſe appointed, the ſoul may ſurvive the body, and laſt for ever: the other, that the nature of the ſoul, according to *Des Cartes*, conſiſting in its being a ſubſtance, that thinks, we may conclude, that though it be by death ſeparate from the body, it will nevertheleſs retain the power of thinking. But now, whether either of theſe two things, or both, be ſufficient to endear the ſtate of ſeparation after death, to a conſidering man, I think may be juſtly queſtioned. For immortality or perſeverance in duration, ſimply conſidered, is rather a thing preſuppoſed to, or a requiſite of felicity, than a part of it; and being in itſelf an adiaphorous thing, aſſumes the nature of the ſtate or condition, to which it is joined, and does not make that ſtate happy or miſerable, but makes the poſſeſſors of it more happy, or more miſerable, than otherwiſe they would be. And though ſome ſchoolmen, upon airy metaphyſical notions, would have men think it is more eligible to be wretched, than not to be at all; yet we may oppoſe to their ſpeculative ſubtilties the ſentiments of mankind, and the far more conſiderable teſtimony of the Saviour of mankind, who ſpeaking of the diſciple, that betrayed him, ſays, "That it had been good for that man, if "he had never been born." And eternity is generally conceived to aggravate no leſs the miſeries of hell, than it heightens the joys of heaven. And here we may conſider, firſt, that mere reaſon cannot ſo much as aſſure us abſolutely, that the ſoul ſhall ſurvive the body; for the truth of which we have not only *Carteſius*'s confeſſion, lately recited, but a probable argument, drawn from the nature of the thing, ſince, as the body and ſoul were brought together, not by any mere phyſical agents, and ſince their aſſociation and union, whilſt they continued together, was made upon conditions, that depended ſolely upon God's free and arbitrary inſtitution; ſo, for aught reaſon can ſecure us of, one of the conditions of that aſſociation may be, that the body and ſoul ſhould not ſurvive each other. Secondly, ſuppoſing, that the ſoul be permitted to outlive the body, mere reaſon cannot inform us, what will become of her in her ſeparate ſtate, whether ſhe will be vitally united to any other kind of body or vehicle; and if to ſome, of what kind that will be, and upon what terms the union will be made. For poſſibly ſhe may be united to an unorganized, or very imperfectly organized body, wherein ſhe cannot exerciſe the ſame functions ſhe did in her human body. As we ſee, that even in this life the ſouls of natural fools are united to bodies, wherein they cannot diſcourſe, or, at leaſt, cannot philoſophize. And it is plain, that ſome ſouls are introduced into bodies, which, by reaſon of paralytical and other diſeaſes, they are unable to move, though that does not always hinder them from being obnoxious to feel pain. So that, for aught we naturally know, a human ſoul, ſeparated from the body, may be united to ſuch a portion of matter, that ſhe may neither have the power to move it, nor the advantage of receiving any agreeable informations by its interventions, having upon the account of that union no other ſenſe than that of pain. But let us now conſider, what will follow, if I ſhould grant, that the ſoul will not be made miſerable, by being thus wretchedly matched. Suppoſe we then, that ſhe be left free to enjoy what belongs to her own nature; that being only the power of always thinking, it may be well doubted, whether the exerciſe of that power will ſuffice to make her happy. You will perchance eaſily believe, that I love, as well as another, to entertain my ſelf with my own thoughts, and to enjoy them undiſturbed by viſits and other avocations: I would, only accompanied by a ſervant and a book, go to dine at an inn upon a road, to enjoy my thoughts the more freely for that day. But yet, I think, the moſt contemplative men would, at leaſt in time, grow weary of thinking, if they received no ſupply of objects from without, by reading, ſeeing, or converſing; and if they alſo wanted the opportunity of executing their thoughts, by moving the members of their bodies, or of imparting them, either by diſcourſing, or writing of books, or by making of experiments. On this occaſion I remember, that I knew a gentleman, who was in *Spain* for a ſtate-crime, which yet he thought an heroic action, kept cloſe priſoner for a year in a place, where, though he had allowed him a diet not unfit for a perſon of note as he was; yet he was not permitted the benefit of any light, either of the day or candles, and was not accoſted by any human creature, ſave at certain

Mark xv. 21.

times by the goaler, that brought him meat and drink, but was ſtrictly forbidden to converſe with him. Now, though this gentleman, by his diſcourſe, appeared to be a man of a lively humour; yet being aſked by me, how he could do to paſs the time in that ſad ſolitude, he confeſſed to me, that though he had the liberty of walking to and fro in his priſon, and though, by often recalling into his mind all the adventures and other paſſages of his former life, and by ſeveral ways combining, and diverſifying his thoughts, he endeavoured to give his mind as much variety of employment as he was able; yet that would not ſerve his turn, but he was often reduced, by drinking large draughts of wine, and then caſting himſelf upon his bed, to endeavour to drown that melancholly, which the want of new objects caſt him into. And I can eaſily admit, he found a great deal of difference between the ſenſe he had of thinking when he was at liberty, and that, which he had, when he was confined to that employment, whoſe delightfulneſs, like fire, cannot laſt long, when it is, as his was, denied both fuel and vent. And, in a word, though I moſt readily grant, that thinking, interwoven with converſation and action, may be a very pleaſant way of paſſing one's time; yet man being by nature a ſociable creature, I fear that alone would be a dry and weariſome employment to ſpend eternity in.

Before I proceed to the next ſection, I muſt not omit to take notice, that though the brevity I propoſed to myſelf, keeps me from diſcourſing of any theological ſubjects, ſave what I have touched upon about the divine attributes, and the things I have mentioned about the univerſe in general, and the human ſoul; yet there are divers other things, knowable by the help of revelation, and not without it, that are of ſo noble and ſublime a nature, that the greateſt wits may find their beſt abilities both fully exerciſed, and highly gratified by making enquiries into them. I ſhall not name for proof of this the adorable myſtery of the trinity, wherein it is acknowledged, that the moſt ſoaring ſpeculators are wont to be poſed, or to loſe themſelves: but I ſhall rather mention the redemption of mankind, and the decrees of God concerning men. For though theſe ſeem to be leſs out of the ken of our natural faculties; yet it is into ſome things, that belong to the former of them, that
1 Pet. i. 12. the ſcripture tells us, *The angels deſire to pry*; and it was the conſideration of the latter of them, that made one, that had been caught up
Rom. xi. 33. into the manſion of the angels, amazedly cry out, ὦ βάθος, &c.

Nor are theſe the only things, that the ſcripture itſelf terms myſteries, though, for brevity ſake, inſtead of ſpecifying any of them, I ſhall content myſelf to repreſent to you in general; that ſince God's wiſdom is boundleſs, it may, ſure, have more ways than one to diſplay itſelf. And though the material world be full of the productions of his wiſdom; yet that hinders not, but that the ſcripture may be enobled with many excellent impreſſes, and, as it were, ſignatures of the ſame attribute. For, as I was beginning to ſay, it cannot but be highly injurious to the Deity, in whom all other true perfections, as well as omniſcience, are both united and tranſcendent, to think, that he can contrive no ways to diſcloſe his perfections, beſides the ordering of matter and motion, and cannot otherwiſe deſerve to be the object of men's ſtudies, and their admiration, than in the capacity of a creator.

And I think, I might ſafely add, that beſides theſe grand and myſterious points I came from mentioning, there are many other noble and important things, wherein unaſſiſted reaſon leaves us in the dark; which though not ſo clearly revealed in the ſcripture, are yet in an inviting meaſure diſcovered there, and conſequently deſerve the indagation of a curious and philoſophical ſoul. Shall we not think it worth enquiring, whether the ſatisfaction of Chriſt was neceſſary to appeaſe the juſtice of God, and purchaſe redemption for mankind? Or whether God, as abſolute and ſupreme governor of the world, might have freely remitted the penalties of ſin? Shall we not think it worth the enquiring, upon what account, and upon what terms, the juſtification of men towards God is tranſacted, eſpecially conſidering how much it imports us to know, and how perplexedly a doctrine, not in itſelf abſtruſe, is wont to be delivered? Shall not we enquire, whether or no the ſouls of men, before they were united to their bodies, pre-exiſted in a happier ſtate, as many of the ancient and modern Jews and Platoniſts, and (beſides Origen) ſome learned men of our times do believe? And ſhall not we be curious to know, whether, when the ſoul leaves the body, it do immediately paſs to heaven or hell (as it is commonly believed,) or for want of organs be laid, as it were, aſleep in an inſenſible and unactive ſtate, till it recover the body at the reſurrection, as many Socinians and others maintain? or whether it be conveyed into ſecret receſſes, where, though it be in a good or bad condition, according to what it did in the body, it is yet reprieved from the flames of hell, and reſtrained from the beatifick viſion till the day of judgment? (which ſeems to have been the opinion of many, if not moſt of the primitive fathers and Chriſtians.) Shall not we be curious to know, whether, at that great decretory day, this vaſt fabrick of the world, which all confeſs muſt have its frame quite ſhattered, ſhall be ſuffered to relapſe into its firſt nothing, as ſeveral divines aſſert? or ſhall be, after its diſſolution, renewed to a better ſtate, and as it were, transfigured? And ſhall not we enquire, whether or no, in that future ſtate of things, which ſhall never have Gen. ii.
an end, we ſhall know one another? (as *Adam*, 21, 22,
when he awaked out of his profound ſleep, 23.
knew *Eve*, whom he never ſaw before;) and whether thoſe perſonal friendſhips and affections, we had for one another here, and the pathetic conſideration of the relations (as of father and ſon, huſband and wife, chaſte miſtreſs and virtuous lover, prince and ſubject,) on which many of them were grounded, ſhall

continue? Or whether all thoſe things, as antiquated and ſlight, ſhall be obliterated, and, as it were, ſwallowed up? (as the former relation of a couſin a great way off is ſcarce at all conſidered, when the perſons come ſo to change their ſtate, as to be united by the ſtrict bonds of marriage.)

But it were tedious to propoſe all the other points, whereof the divine takes cognizance, that highly merit an inquiſitive man's curioſity; and about which, all the writings of the old Greek and other heathen philoſophers put together, will give us far leſs information, than the ſingle volume of canonical ſcripture. I foreſee indeed, that it may nevertheleſs be objected, that in ſome of theſe enquiries, revelation incumbers reaſon, by delivering things, which reaſon is obliged to make its hypotheſis conſiſtent with. But beſides, that this cannot be ſo much as pretended of all; if you conſider, how much unaſſiſted reaſon leaves us in the dark about theſe matters, wherein ſhe has not been able to frame ſo much as probable determinations, eſpecially in compariſon of thoſe probabilities, that reaſon can deduce from what it finds one way or other delivered in the ſcripture: if you conſider this, I ſay, you will, I preſume, allow me to ſay, that the revealed truths, which reaſon is obliged to comply with, if they be burdens to it, are but ſuch burdens, as feathers are to a hawk, which, inſtead of hindering his flight by their weight, enable him to ſoar toward heaven, and take a larger proſpect of things, than, if he had not feathers, he could poſſibly do.

And, on this occaſion, Sir, the greater reverence I owe to the ſcripture itſelf, than to its expoſitors, prevails upon me to tell you freely, that you will not do right, either to theology, or (the greateſt repoſitory of its truths) the bible, if you imagine, that there are no conſiderable additions to be made to the theological diſcoveries we have already, nor no clearer expoſitions of many texts of ſcripture, or better reflections on that matchleſs book, than are to be met with in the generality of commentators, or of preachers, without excepting the antient fathers themſelves. For there being, in my opinion, two things requiſite, to qualify a commentator to do right to his theme, a competency of critical knowledge, and a concern for the honour and intereſt of Chriſtianity in general, aſſiſted by a good judgment, to diſcern and ſelect thoſe things, that may moſt conduce to it; I doubt, there are not many expoſitors, as they are called, of the ſcripture, that are not deficient in the former, or the latter of theſe particulars, and I wiſh there be not too many, that are defective in both.

That the knowledge of at leaſt Greek and Hebrew is requiſite to him, that takes upon him to expound writings penned originally in thoſe languages, if the nature of the thing did not manifeſt it, you might eaſily be perſuaded to believe, by conſidering, with what groſs miſtakes the ignorance of languages has oftentimes blemiſhed, not only the interpretations of the ſchool-men and others, but even thoſe of the venerable fathers of the church. For though generally they were worthy men, and highly to be regarded, as the grand witneſſes of the doctrines and government of the antient churches; moſt of them very pious, many of them very eloquent, and ſome of them (eſpecially the two criticks, *Origen* and *Jerom*) very learned; yet ſo few of the Greek fathers were ſkilled in Hebrew, and ſo few of the Latin fathers either in Hebrew or Greek, that many of their homilies, and even comments, leave hard texts as obſcure as they found them; and, ſometimes miſled by bad tranſlations, they give them ſenſes exceeding wide of the true: ſo that many times in their writings they appear to be far better divines than commentators, and in an excellent diſcourſe upon a text, you ſhall find but a very poor expoſition of it; many of their eloquent and devout ſermons being much better encomiaſts of the divine myſteries they treat of, than unvailers. And though ſome modern tranſlations deſerve the praiſe of being very uſeful, and leſs unaccurate than thoſe, which the Latin fathers uſed; yet when I read the ſcriptures (eſpecially ſome books of the Old Teſtament) in their originals, I confeſs I cannot but ſometimes wonder, what came into the mind of ſome, even of our modern tranſlators, that they ſhould ſo much miſtake, and ſometimes injure certain texts as they do; and I am prone to think, that there is ſcarce a chapter in the bible (eſpecially that part of it, which is written in Hebrew,) that may not be better tranſlated, and conſequently more to the credit of the book itſelf,

This credit it miſſes of, not only by men's want of ſufficient ſkill in critical learning, but (to come to the ſecond member of our late diviſion) for want of their having judgment enough to obſerve, and concern enough to propoſe thoſe things in the ſcripture, and in theology, that tend to the reputation of either. For I fear there are too many, both commentators and other divines, that (though otherwiſe perhaps pious men) having eſpouſed a church or party, and an averſion from all diſſenters, are ſollicitous, when they peruſe the ſcripture, to take notice chiefly, if not only (I mean in points ſpeculative) of thoſe things, that may either ſuggeſt arguments againſt their adverſaries, or anſwers to their objections. But I meet with much fewer than I could wiſh, who make it their buſineſs to *ſearch the ſcriptures* for thoſe things (ſuch as unheeded prophecies, over-looked myſteries, and ſtrange harmonies,) which being clearly and judiciouſly propoſed, may make that book appear worthy of the high extraction it challenges (and conſequently of the veneration of conſidering men) and who are ſollicitous to diſcern and make out, in the way of governing and of ſaving men, revealed by God, ſo excellent an oeconomy, and ſuch deep contrivances, and wiſe diſpenſations, as may bring credit to religion, not ſo much as it is Roman, or proteſtant, or Socinian, but as it is Chriſtian. But (as I intimated before) theſe

good

Ἐρευνᾶντας γραφάς. John v. 39.

good affections for the repute of religion in general are to be aſſiſted by a deep judgment. For men, that want either that, or a good ſtock of critical learning, may eaſily over-ſee the beſt obſervations (which uſually are not obvious) or propoſed as myſteries, things that are either not grounded, or not weighty enough; and ſo (notwithſtanding their good meaning) may bring a diſparagement upon what they deſire to recommend. And I am willing to grant, that it is rather for want of good ſkill and good judgment, than good will, that there are ſo few, that have been careful to do right to the reputation of the ſcripture, as well as to its ſenſe. And indeed when I conſider, how much more to the advantage of thoſe ſacred writings, and of Chriſtian theology in general, divers texts have been explained and diſcourſed of by the excellent *Grotius*, by *Epiſcopius*, *Maſius*, Mr. *Mede*, and Sir *Francis Bacon*, and ſome other late great wits (to name now no living ones) in their ſeveral kinds, than the ſame places have been handled by vulgar expoſitors, and other divines: and when I remember too, that none of theſe newly named worthies was at once a great philoſopher, and a great critick; (the three firſt being not ſo well verſed in philoſophical learning, and the laſt being unacquainted with the eaſtern tongues:) I cannot but hope, that when it ſhall pleaſe God to ſtir up perſons of a philoſophical genius, well furniſhed with critical learning, and the principles of true philoſophy, and ſhall give them a hearty concern, for the advancement of his truths; theſe men, by exerciſing upon theological matters that inquiſitiveneſs and ſagacity, that has made in our age ſuch a happy progreſs in philoſophical ones, will make explications and diſcoveries, that will juſtify more than I have ſaid in praiſe of the ſtudy of our religion, and the divine books, that contain the articles of it. For theſe want not excellencies, but only ſkilful unvailers. And if I do not tell you, that you ſhould no more meaſure the wiſdom of God couched in the bible, by the gloſſes or ſyſtems of common expoſitors and preachers, than eſtimate the wiſdom he has expreſſed in the contrivance of the world by *Magirus*'s or *Euſtachius*'s phyſicks; yet I ſhall not ſcruple to ſay, that you ſhould as little think, that there are no more myſteries in the books of ſcripture, beſides thoſe, that the ſchool-divines and vulgar commentators have taken notice of, and unfolded; as, that there are no other myſteries in the book of nature, than thoſe, which the ſame ſchoolmen (who have taken upon them to interpret *Ariſtotle* and nature too) have obſerved and explained. All the fine things, that poets, orators, and even lovers have hyperbolically ſaid in praiſe of the beauty of eyes, will nothing near ſo much recommend them to a philoſopher's eſteem, as the ſight of one eye ſkilfully diſſected, or the unadorned account given of its ſtructure, and the admirable uſes of its ſeveral parts, in *Scheiner*'s *Oculus*, and *Des Cartes*'s excellent dioptricks. And though I do not think myſelf bound to acquieſce in, and admire every thing, that is propoſed, as myſterious and rare by many interpreters and preachers; yet I think, I may ſafely compare ſeveral things in the books we call the ſcripture, to ſeveral others in that of nature, in (at leaſt) one regard. For, though I do not believe all the wonders, that *Pliny*, *Ælian*, *Porta*, and other writers of that ſtamp, relate of the generation of animals; yet by peruſing ſuch faithful and accurate accounts, as ſometimes *Galen de uſu Partium*, ſometimes *Veſalius*, ſometimes our *Harvey* (*de Ovo*) and our later anatomiſts, and ſometimes other true naturaliſts, give of the generation of animals, and of the admirable ſtructure of their bodies, eſpecially thoſe of men, and ſuch other parts of zoology, as *Pliny*, and the other writers I named with him, could make nothing conſiderable of; by peruſing theſe, I ſay, I receive more pleaſure and ſatisfaction, and am induced more to admire the works of nature, than by all their romantick and ſuperficial narratives. And thus (to apply this to our preſent ſubject) a cloſe and critical account of the more vailed and pregnant parts of ſcripture, and theological matters, with ſuch reflections on them, as their nature and collation would ſuggeſt to a philoſophical, as well as critical ſpeculator, would far better pleaſe a rational conſiderer, and give him a higher, as well as a better grounded veneration, for the things explained, than a great many of thoſe ſlighter or ill founded remarks, wherewith the expoſitions and diſcourſes of ſuperficial writers, though never ſo florid or witty, gain the applauſe of the leſs diſcerning ſort of men.

AND here, on this occaſion, I ſhall venture to add, that I deſpair not, but that a farther uſe may be made of the ſcripture, than either our divines or philoſophers ſeem to have thought on. Some few theologues indeed have got the name of Supralapſarians, for venturing to look back beyond the fall of *Adam* for God's decrees of election and reprobation. But beſides that, their boldneſs has been diſliked by the generality of divines, as well as other Chriſtians, the object of their ſpeculation is much too narrow to be any thing near and adequate to ſuch an hypotheſis as I mean. For methinks, that the Encyclopedia's and Panſophia's, that even men of an elevated genius have aimed at, are not diffuſed enough to comprehend all, that the reaſon of a man, improved by philoſophy, and elevated by the revelations already extant in the ſcripture, may, by the help of free ratiocination, and the hints contained in thoſe pregnant writings (with thoſe aſſiſtances of God's ſpirit, which he is ſtill ready to vouchſafe to them, that duly ſeek them,) attain unto in this life. The goſpel compriſes indeed, and unfolds the whole myſtery of man's redemption, as far forth as Acts xx. 27. it is neceſſary to be known for our ſalvation: and the corpuſcularian or mechanical philoſophy ſtrives to deduce all the phænomena of nature from adiaphorous matter, and local motion. But neither the fundamental doctrine of Chriſtianity, nor that of the powers and effects of matter and motion, ſeems to be more than an epicycle (if I may ſo call it) of the great and univerſal ſyſtem of God's contrivances, and makes but a part of the more general theory of things, knowable by the light of nature, improved by the information of the ſcriptures: ſo that both theſe doctrines, though very general, in reſpect of the ſubordinate parts of theology and philoſophy, ſeem to be but members of the univerſal hypotheſis, whoſe objects I conceive to be the nature, counſels, and works of God, as far as they are diſcoverable by us (for I ſay not to us) in this life.

For thoſe, to whom God has vouchſafed the privilege of mature reaſon, ſeem not to enlarge their thoughts enough, if they think, that the omniſcient and almighty God has bounded the operations of his power, and wiſdom, and goodneſs, to the exerciſe, that may be given them for ſome ages, by the production and government of matter and motion, and of the inhabitants of the terreſtrial globe, which we know to be but a phyſical point in compariſon of that portion of univerſal matter, which we have already diſcovered.

For I account, that there are four grand communities of creatures, whereof things merely corporeal make but one; the other three, differing from theſe, are diſtinct alſo from one another. Of the firſt ſort are the race of mankind, where intellectual beings are vitally aſſociated with groſs and organical bodies. The ſecond are dæmons, or evil angels; and the third good angels; (whether in each of thoſe two kinds of ſpirits, the rational beings be perfectly free from all union with matter, though never ſo fine and ſubtile; or whether they be united to vehicles, not groſs, but ſpirituous, and ordinarily inviſible to us.)

Nor may we think, becauſe angels and devils are two names quickly uttered, and thoſe ſpirits are ſeldom or never ſeen by us, there are therefore but few of them, and the ſpeculation of them is not conſiderable. For, as their excellency is great, (as we ſhall by and by ſhew) ſo for their number, they are repreſented in ſcripture as an heavenly hoſt, ſtanding on the right and left hand of the throne of God. And of the good angels, our ſaviour ſpeaks of having Matth. xxvi. 53. more than twelve legions of them at his command. Dan. vii. 10. Nay, the prophet *Daniel* ſaith, that to the ancient of days, no leſs than millions miniſtred unto him, and hundreds of millions ſtood before him. And of the evil angels, the Mark v. 9. goſpel informs us, that enough to call them a Luke viii. 30. legion (which, you know, is uſually reckoned, at a moderate rate, to conſiſt of betwixt ſix and ſeven thouſand) poſſeſſed one ſingle man. For my part, when I conſider, that matter, how vaſtly extended, and how curiouſly ſhaped ſoever, is but a brute thing, that is only capable of local motion, and its effects and conſequents on other bodies, or the brain of man, without being capable of any true, or at leaſt any intellectual perception, or true love or hatred; and when I conſider the rational ſoul as an immaterial and immortal being, that bears the image of its divine maker, being endowed with a capacious intellect, and a will, that no creature can force: I am by theſe conſiderations diſpoſed to think the ſoul of man a nobler and more valuable being, than the whole corporeal world; which though I readily acknowledge it to be admirably contrived, and worthy of the almighty and omniſcient author, yet it conſiſts but of an aggregate of portions of brute matter, variouſly ſhaped and connected by local motion (as dough, and roles, and loaves, and cakes, and vermicelli, wafers, and pie-cruſt, are all of them diverſified meal; but without any knowledge either of their own nature, or of that of their author, or of that of their fellow-creatures.) And as the rational ſoul is ſomewhat more noble and wonderful, than any thing merely corporeal, how vaſt ſoever it can be, and is of a more excellent nature, than the curiouſeſt piece of mechaniſm in the world, the human body; ſo to enquire what ſhall become of it, and what fates it is like to undergo hereafter, does better deſerve a man's curioſity, than to know what ſhall befal the corporeal univerſe, and might juſtly have been to *Nebuchadnezzar* a more deſirable Dan. ii. 31, 32, &c. part of knowledge, than that he was ſo troubled for want of, when it was adumbrated to him in the myſterious dream, that contained the characters and fates of the four great monarchies of the world. And as man is intruſted with a will of his own, whereas all material things move only as they are moved, and have no ſelf-determining power, on whoſe account they can reſiſt the will of God; and as alſo of angels, at leaſt ſome orders of them, are of a higher quality (if I may ſo ſpeak) than huhuman ſouls; ſo it is very probable, that in the government of angels, whether good or bad, that are intellectual voluntary agents, that is required and employed far grater diſplays of God's wiſdom, power, and goodneſs, in the guidance of adiaphorous matter; and the method of God's conduct in the government of theſe, is a far nobler object for men's contemplation, than the laws, according to which the parts of matter hit againſt, and juſtle one another, and the effects or reſults of ſuch motions.

And accordingly we find in ſcripture, that, whereas about the production of the material world, and the ſetting of the frame of nature, God employed only a few commanding words, which ſpeedily had their full effects; to govern the race of mankind, even in order to their own happineſs, he employed not only laws and commands, but revelations, miracles, promiſes threats, exhortations, mercies, judgments, and divers other methods and means; and yet oftentimes, when he might well ſay, as he did once by his prophet, “What could I have “done more to my vineyard, that I have not “done it?’ he had juſt cauſe to expoſtulate as he did in the ſame place, “Wherefore, when “I looked, that it ſhould bring forth grapes, “brought it forth wild grapes?” and to complain of men, as by that very prophet he did even of *Iſrael*, “I have ſpread out my hands Iſa. lxv. 2. “all the day to a rebellious people.” But not to wander too far in this digreſſion; what we have ſaid of men, may render it probable, that the grand attributes of God are more ſignally exerciſed, and made more conſpicuous in the making and governing of each of the three intellectual communities, than in the framing and upholding the community of mere bodily things. And ſince all immaterial ſubſtances are for that reaſon naturally immortal, and the univerſal matter is believed ſo too, poſſibly thoſe revolutions, that will happen after the day of judgment, wherein though probably *not* the matter, yet that ſtate and conſtitution of it, on whoſe account it is this world will be deſtroyed, and make way for quite new frames and ſets of things corporeal, and the beings, that compoſe each of theſe intellectual communities, will, in thoſe numberleſs ages they ſhall laſt, travel through I know not how many ſucceſſive changes and adventures; perhaps, I ſay, theſe things will no leſs diſplay, and bring glory to the divine attributes, than the contrivance of the world, and the oeconomy of man's ſalvation, though theſe be (and that worthily) the objects of the naturaliſts and the divines contemplation. And there are ſome paſſages in the prophetical part of the ſcripture, and eſpecially in the book of the Apocalypſe, which, as they ſeem to intimate, that as God will perform great and noble things, which mechanical philoſophy never reached to, and which the generality of divines ſeem not to have thought of; ſo divers of thoſe great things may be, in ſome meaſure, diſcovered by an attentive ſearcher into the ſcriptures, and that ſo much to the advantage of the devout indagator, that St. *John*, near the beginning of his *Revelations*, pronounces them happy, that read the matters contained in this prophecy, and [*] *obſerve* the things written therein. Rev. i. 3. Which implies, that by heedful comparing together the indications couched in thoſe prophetick writings, with events and occurrences in the affairs of the world, and the church, we may diſcover much of the admirable oeconomy of providence in the governing of both: and I am prone to think, the early diſcoveries of ſuch great and important things to be, in God's account, no mean vouchſafements, not only becauſe the title of happy is here given to him, that attains them, but becauſe of the two perſons, to whom the great diſcoveries of this kind were made, I mean, the prophet *Daniel* and St. *John*; the firſt is by the angel ſaid to be, on that account, a perſon highly favoured; and the other is, in the goſpel, repreſented as our Saviour's beloved diſciple. And you will the more eaſily think the foreknowledge of the divine diſpenſations gatherable from ſcripture to be highly valuable, if you conſider, that, according to St. *Paul*, thoſe very angels, that are called principalities and powers in heavenly places, learnt by the church ſome abſtruſe points of the, manifold wiſdom of God. Πολυποικιλος σοφία τοῦ Θεοῦ Epheſ. iii. 10. But I muſt no longer indulge ſpeculations, that would carry my curioſity beyond the bounds of time itſelf, and therefore beyond thoſe, that ought to be placed to this occaſional excurſion.

And yet, as on the one ſide, I ſhall not allow myſelf the preſumption of framing conjectures about thoſe remote diſpenſations, which will not, moſt of them, have a beginning before this world ſhall have an end; ſo on the other ſide I would not diſcourage you, or any pious enquirer, from endeavouring to advance in the knowledge of thoſe attributes of God, that may ſucceſsfully be ſtudied, without prying into the ſecrets of the future.

AND here, Sir, let me freely confeſs to you, that I am apt to think, that if men were not wanting to God's glory, and their own ſatisfaction, there would be far more diſcoveries made, than are yet attained to, of the divine attributes. When we conſider the moſt ſimple, or uncompounded eſſence of God, we may eaſily be perſuaded, that what belongs to any of his attributes (ſome of which thinking men generally admire) muſt be an object of enquiry exceeding noble, and worthy of our knowledge. And yet the abſtruſeneſs of this knowledge is not in all particulars ſo invincible, but that I ſtrongly hope, a philoſophical eye, illuſtrated by the revelations extant in the ſcripture, may pierce a great deal farther than has yet been done, into thoſe myſterious ſubjects, which are too often (perhaps out of a miſtaken reverence) ſo poorly handled by divines and ſchoolmen, that not only what they have taught, is not worthy of God (for that is a neceſſary, and therefore excuſable deficiency) but too frequently it is not worthy of men, I mean, of rational creatures, that take upon them to treat of ſuch high points, and inſtruct others about them. And I queſtion not but your friend will the leſs ſcruple at this, if he call to mind thoſe new and handſome notions about ſome of the attributes of God, that his maſter *Carteſius*, though but moderately verſed in the ſcriptures, has preſented us with. Nor do I doubt, but that a much greater progreſs might be made in the diſcovery of ſubjects, where, though we can never know all, we may ſtill know farther, if ſpeculative genius's would propoſe to themſelves particular doubts and enquiries, about particular attributes, and frame and examine hypotheſes, eſtabliſh theorems, draw corollaries; and (in ſhort) apply to this ſtudy the ſame ſagacity, aſſiduity, and attention of mind, which they often employ about enquiries of a very much inferior nature; inſomuch, as *Des Cartes* (how profound a geometrician ſoever he were) confeſſes in one of his epiſtles, that he employed no leſs than ſix weeks to find the ſolution of a ploblem or queſtion of *Pappus*. And *Pythagoras* was ſo addicted to, and concerned for geometrical ſpeculations, that when he had found that famous propoſition, which makes the 47th in *Euclid*'s Firſt Book, he is recorded to have offered a hecatomb, to expreſs his joy and gratitude for the diſcovery: which yet was but of one property of one ſort of right-lined triangles. And certainly, if Chriſtian philoſophers did rightly eſtimate, how noble and fertile ſubjects the divine attributes are, they would find in them wherewithal to exerciſe their beſt parts, as well as to recompence the employment of them. But becauſe, what I would diſſuade, does not, perhaps, proceed only from lazineſs, but from a miſtake; as if there were little to be known of ſo incomprehenſible an object as God, ſave, that in general, all his attributes are like himſelf, infinite, and conſequently not to be fully known by human underſtandings, becauſe they are finite; I ſhall add, that though it be true, that by reaſon of God's infinity, we cannot comprehend him, that is, have a full and adequate knowledge of him; yet, we may not only know very many things concerning him, but, which is more, may make an endleſs progreſs in that knowledge. As no doubt, *Pythagoras* (newly mentioned) knew very well what a triangle was, and was acquainted with divers of its properties and affections, before he diſcovered that famous one. And though, ſince him, *Euclid*, *Archimedes*, and other geometricians have demonſtrated, I know not how many other affections of the ſame figure, yet they have not to this day exhauſted the ſubject: and poſſibly I (who pretend not to be a mathematician) may now and then, in managing certain æquations I had occaſion for, have lighted upon ſome theorems about triangles, that occurred not to any of them. The divine attributes are ſuch fruitful themes, and ſo worthy of our admiration, that the whole fabrick of the univerſe, and all the phænomena exhibited in it, are but imperfect expreſſions of God's wiſdom, and ſome few of his other attributes. Iſa. vi. 2, 3. Luke ii. 13, 14. Rev. v. 11, 12. And I do not much marvel, that the angels themſelves are repreſented in ſcripture, as employed in adoring God, and admiring his perfections. For even they being but finite, can frame but inadequate conceptions of him; and conſequently muſt endeavour, by many of them, to make amends for the incompleatneſs of every one of them; which yet they can never but imperfectly do. And yet God's infinity can but very improperly be made a diſcouragement of our enquiries into his nature and attributes. For (not now to examine, whether infinity, though expreſſed by a negative word, be not a poſitive thing in God) we may, notwithſtanding his infinity, diſcover as much of him as our nature is capable of knowing: and what harm is it to him, that is drinking in a river, that he cannot drink up all the water, if he have liberty fully to quench his thirſt, and take in as much liquor as his ſtomach can contain? Infinity therefore ſhould not hinder us from a generous ambition to learn as much as we can of an object, whoſe being infinite does but make our knowledge of it the more noble and deſirable, which indeed it is, in ſuch a degree, that we need not wonder, that the angels are repreſented as never weary of their employment of contemplating and praiſing God. For, as I lately intimated, that they can have but inadequate ideas of thoſe boundleſs perfections, and by no number of thoſe ideas can arrive to make amends for the incompleatneſs of them; ſo it need not ſeem ſtrange, that in freſh diſcoveries of new parts (if I may ſo call them) of the ſame object, it being ſuch a one, they ſhould find nobler and happier entertainments, than any where elſe variety could afford them.

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[*] To render the original word (*obſerve*, or) *watch*, rather than *keep*, ſeems more congruous to the ſenſe of the text, and is a criticiſm ſuggeſted to me by an eminent mathematician, as well as divine, who took notice, that the word τηρεῖ is uſed by the Greeks, as a term of art to expreſs the aſtronomical obſervation of eclipſes, planetary conjunctions, oppoſitions, and other celeſtial phænomena.

SECTION II.

HAVING thus taken notice of ſome particulars of thoſe many, which may be employed to ſhew, how noble the objects are, that theology propoſes to be contemplated; I now proceed to ſome conſiderations, that may make us ſenſible, how great an obligation there lies on us, to addict ourſelves to the ſtudy of them.

YET, of the particulars, whereon this obligation may be grounded, I ſhall now name but two, they being indeed comprehenſive ones, obedience, and gratitude.

AND firſt, let me repreſent, that it needs not, I ſuppoſe, be ſolicitouſly proved, that it is the will and command of God, that men ſhould learn thoſe truths, that he has been pleaſed to teach, whether concerning his nature or attributes, or the way, wherein he will be ſerved and worſhipped by man. For if we had not injunctions of ſcripture to that purpoſe, yet your friend is too rational a man to believe, that God would ſo ſolemnly cauſe his truths to be publiſhed to mankind, both by preaching and writing, without intention to oblige thoſe (at leaſt) that have the capacity and opportunity to enquire into ſome of them; and if it appear to be his will, that a perſon ſo qualified ſhould ſearch after the moſt important truths, that he hath revealed, it cannot but be their duty to do ſo. For though the nature of the thing itſelf did not lay any obligation on us, yet the authority of him, that commands it, would; ſince, being the ſupreme and abſolute Lord of all his creatures, he has as well a full right to make what laws he thinks fit, and enjoin what ſervice he thinks fit, as a power to puniſh thoſe, that either violate the one, or deny the other; and accordingly it is very obſervable, that before *Adam* fell, and had forfeited his happy ſtate, by his own tranſgreſſion, he not only had a law impoſed upon him, Gen. ii. 16, 17. but ſuch a law, as, being about a matter itſelf indifferent, (for ſo it was to eat, or not to eat, of the tree of life, as well as of any other,) derived its whole power of obliging from the mere will and pleaſure of the law-giver. Whence we may learn, that man is ſubject to the laws of God, not as he is obnoxious to him, but as he is a rational creature, and that the thing, that is not a duty in its own nature, may become an indiſpenſible one, barely by its being commanded. And indeed, if our firſt parent, in the ſtate of innocency and happineſs, wherein he taſted of God's bounty, without as yet ſtanding in need of his mercy, was moſt ſtrictly obliged, out of mere obedience, to conform to a law, the matter of which was indifferent in itſelf; ſure we, in our lapſed condition, muſt be under a high obligation to obey the declared will of God, whereby we are enjoined to ſtudy his truths, and perform that, which has ſo much of intrinſick goodneſs in it, that it would be a duty, though it were not commanded; and has ſuch recompences propoſed to it, that it is not more a duty, than it will be an advantage.

But it is not only obedience, and intereſt, that ſhould engage us to the ſtudy of divine things, but gratitude; and that exacted by ſo many important motives, that he, who ſaid, *ingratum ſi dixeris, omnia dixeris*, could not think ingratitude ſo much worſe than ordinary vices, as a contempt of the duty I am preſſing would be worſe than an ordinary ingratitude.

It were not difficult, on this occaſion, to manifeſt, that we are extremely great debtors unto God, both as he is the author, and the preſerver of our very beings; and as he (immediately, or mediately,) fills up the meaſure of thoſe continual benefits, with all the prerogatives, and other favours we do receive from him, as men; and the higher bleſſings, which (if we are not wanting to ourſelves,) we may receive from him, as Chriſtians.

But to ſhew, in how many particulars, and to how high a degree, God is our benefactor, were to launch out into two immenſe a ſubject; which it were the leſs proper for me to do, becauſe I have, in other papers, diſcourſed of thoſe matters already. Seraph Love. I will therefore ſingle out a motive of gratitude, which will be peculiarly pertinent to our preſent purpoſe. For, whereas your friend does ſo highly value himſelf upon the ſtudy of natural philoſophy, and deſpiſes not only divines, but ſtateſmen, and even the learnedeſt men in other parts of philoſophy and knowledge, becauſe they are not verſed in phyſicks; he owes to God that very ſkill, among many other vouchſafements. Pſal. xxxii. 9. For it is God, who *made man unlike the horſe and the mule, who have no underſtanding*, and endowed him with that noble power of reaſon, by the exerciſe of which, he attains to whatever knowledge he has of natural things above the beaſts that periſh. For that may juſtly be applied to our other acquiſitions, which *Moſes*, by God's appointment, told the Iſraelites concerning the acquiſts of riches; where he bids the people beware, that when their herds, and their flocks, and other treaſures were multiplied, their heart be not lifted up, and prompt them to ſay, Deut. viii. 10, 11, 12, "My power, and the might of my "hand, hath gotten me this wealth." But, (ſubjoins that excellent perſon, as well as matchleſs law-giver,) 13, 14, Thou ſhalt remember thy Lord thy God, for it is he, that giveth thee power to get wealth. 18. But to make men rational creatures, is not all God has done towards the making them philoſophers. For, to the knowledge of particular things, objects are as well requiſite as faculties; and if we admit the probable opinion of divines, who teach us, that the angels were created before the material world, Job xxxviii. 5, 6, 7. as being meant by thoſe *ſons of God*, and *morning ſtars*, that, with glad ſongs and acclamations, celebrated the foundations of the earth; we muſt allow, that there were many creatures endowed with, at leaſt, as much reaſon as your friend, who yet were unacquainted with the myſteries of nature, ſince ſhe herſelf had not yet received a being. Wherefore, God having as well made the world, as given man the faculties, whereby he is enabled to contemplate it; naturaliſts are as much obliged to God for their knowledge, as we are for our intelligence to thoſe, that write us ſecrets in cyphers, and teach us the ſkill of decyphering things ſo written; or to thoſe, who write what would fill a page in the compaſs of a ſingle penny, and preſent us to boot a microſcope to read it. And as the naturaliſt hath peculiar inducements to gratitude, for the endowment of knowledge; ſo ingenuity lays this peculiar obligation on him to expreſs his gratitude, in the way I have been recommending, that it is one of the acceptableſt ways it can be expreſſed in; eſpecially ſince, by this way, philoſophers may not only exerciſe their own gratitude towards God, but procure him that of others. How pleaſing men's hearty praiſes are to God, may appear, among other things, by what is ſaid and done by that royal poet, whom God was pleaſed to declare a man after his own heart; for he introduces God pronouncing, "Whoſo offereth Pſal. l. 23. "praiſe, glorifieth me;" where the word our interpreters render offereth, in the *Hebrew*, ſignifies to ſacrifice; with which agrees, that elſewhere thoſe, that pay God their praiſes, are ſaid to ſacrifice to him "the calves of their lips." Hoſ. xiv. 2. And that excellent perſon, to whom God vouchſafed ſo particular a teſtimony, was ſo aſſiduous in this exerciſe, that the book which we, following the *Greek*, call *Pſalms*, is, in the original, from the things it moſt abounds with, called *Sepher Tehillim*, i. e. the book of praiſes. And to let you ſee, that many of his praiſes were ſuch, as the naturaliſt may beſt give, he exclaims, in one place, "How manifold are thy Pſal. civ. 24. "works, O Lord? how wiſely haſt thou made "them," (as *Junius* and *Tremellius* render it, and the *Hebrew* will bear;) and elſewhere, "The Pſal. xix. 1. "heavens declare the glory of God, and the firmament ſheweth his handy-work, *&c.*" Again, in another place, "I will praiſe thee, becauſe I Pſal. cxxxix. 14. "am fearfully and wonderfully made. Marvellous are thy works, and that my ſoul knoweth "right well." And not content with many of the like expreſſions, he does ſeveral times, in a devout tranſport, and poetical ſtrain, invite the heavens, and the ſtars, and the earth, and the ſeas, and all the other inanimate creatures, to join with him in the celebration of their common maker. Which, though it ſeem to be merely a poetical ſcheme, yet, in ſome ſort, it might become a naturaliſt, who, by making out the power, wiſdom, and goodneſs of the Creator; and by reflecting thence on thoſe particulars, wherein thoſe attributes ſhine, may, by ſuch a devout conſideration of the creatures, make them, in a ſenſe, join with him in glorifying their author.

In any other caſe, I dare ſay, your friend is not ſo ill-natured, but that he would think it an unkind piece of ingratitude, if ſome great and excellent prince, having freely and tranſcendently obliged him, he ſhould not concern himſelf to know what manner of man his benefactor is; and ſhould not be ſolicitous to inform himſelf of thoſe particulars, relating to the perſon and affairs of that obliging monarch, which were not only in themſelves worthy of any man's curioſity, but about which the prince had ſolemnly declared, he was very deſirous to have men inquiſitive. And ſurely it is very diſingenious, to undervalue, or neglect the knowledge of God himſelf, for a knowledge, which we cannot attain without him, and by which he deſigned to bring us to that ſtudy we neglect for it: which is not only, not to uſe him as a benefactor, but as if he meant to puniſh him, (if I may ſo ſpeak,) for having obliged us, ſince we ſo abuſe ſome of his favours, as to make them inducements to our unthankful diſregard of his intentions in the reſt. And this ingratitude is the more culpable, becauſe the laws of ingenuity, and of juſtice itſelf, charge us to glorify the maker of all things viſible, not only upon our own account, but upon that of all his other works. For, by God's endowing of none but man here below, with a reaſonable ſoul, not only he is the ſole viſible being, that can return thanks and praiſes in the world, and thereby is obliged to do ſo, both for himſelf, and for the reſt of the creation; but it is for man's advantage, that God has left no other viſible beings in the world, by which he can be ſtudied and celebrated. For reaſon is ſuch a ray of divinity, that, if God had vouchſafed it to other parts of the univerſe beſides man, the abſolute empire of man over the reſt of the world muſt have been ſhared, or abridged. So that he, to whom it was equally eaſy to make creatures ſuperior to man, (as the ſcripture tells us of legions, and myriads of angels,) as to make them inferior to him, dealt ſo obligingly with mankind, as rather to truſt (if I may ſo ſpeak) our ingenuity, whether he ſhall reap any celebrations from the creatures we converſe with, than leſſen our empire over them, or our prerogatives above them.

BUT I fear, that, notwithſtanding all the excellency of revealed truths, and conſequently of that only authentic repoſitory of them, the ſcripture, you, as well as I, have met with ſome (for I hope there are not many) virtuoſi, that think to excuſe the neglect of the ſtudy of it, by alledging, that, to them, who are laymen, not eccleſiaſticks, there is required to ſalvation the explicit knowledge but of very few points, which are ſo plainly ſummed up in the apoſtles creed, and are ſo often and conſpicuouſly ſet down in the ſcripture, that one needs not much ſearch, or ſtudy, to find them there.

IN anſwer to this allegation, I readily grant, 1 Tim. ii. 4. that through the great goodneſs of God, who is willing to have all men ſaved, and come to the knowledge of the truth, that is neceſſary to be ſo, there are much fewer articles abſolutely neceſſary to be by all men diſtinctly believed, than may be met with in divers long confeſſions of faith, ſome of which have, I fear, leſs promoted knowledge than impaired charity. But then it may be alſo conſidered, 1. That it is not ſo eaſy for a rational man, that will trouble himſelf to enquire no farther than the apoſtles creed, to ſatisfy himſelf upon good grounds, that all the fundamental articles of Chriſtianity are contained in it. 2. That the creed propoſes only the *credenda*, not the *agenda* of religion; Joh. xiij. 7. whereas the ſcriptures were deſigned, Heb. v. 9. not only to teach us what truths we are to believe, but by what rules we are to live; the obedience to the laws of Chriſtianity being as neceſſary to ſalvation, as the belief of its myſteries. 3. That beſides the things, which are abſolutely neceſſary, there are ſeveral, that are highly uſeful, to make us more clearly underſtand, and more rationally and firmly believe, and more ſteadily practice, the points, that are neceſſary. 4. And ſince, whether or no thoſe words of our Saviour to the Jews, ἐρευνᾶτε τὰς γραφὰς *, be to be rendered in the Joh. v. 39. imperative or indicative mode; St. *Paul* would have the word of Chriſt to dwell richly in us, (by which, whether he mean the holy ſcriptures Coloſſ. iii. 16. then extant, or the doctrine of Chriſt, is not here material;) thereby teaching us, that ſearching into the matters of religion may become neceſſary, as a duty, though it were not otherwiſe neceſſary, as a means of attaining ſalvation. And indeed it is far more pardonable to want or miſs the knowledge of truths, than to deſpiſe or neglect it. And the goodneſs of God to illiterate or miſtaken perſons is to be ſuppoſed meant in pity to our frailties, not to encourage our lazineſs; nor is it neceſſary, that he, that pardons thoſe ſeekers of his truths, that miſs them, ſhould excuſe thoſe deſpiſers, that will not ſeek them.

BUT whether or no by this deſigned neglect of theology the perſons, I deal with, do ſufficiently conſult their own ſafety, I doubt they will not much recommend their ingenuity. For to have received from God a greater meaſure of intellectual abilities than the generality of Chriſtians, and yet willingly to come ſhort of very many of them, in the knowledge of the myſteries, and other truths of Chriſtianity, which he often invites us, if not expreſly commands, to ſearch after, is a courſe, that will not reliſh of over-much gratitude. Is it a piece of that, and of ingenuity, to receive one's underſtanding and one's hopes of eternal felicity from the goodneſs of God, without being ſollicitous of what may be known of his nature and purpoſes, by ſo excellent a way as his own revelation of them? to diſpute anxiouſly about the properties of an atom, and be careleſs about the enquiry into the attributes of the great God, who Prov. xxvi. 10. formed all things; to inveſtigate the ſpontaneous generation of ſuch vile creatures as inſects, than the myſterious generation of the adorable Son of God; and, in a word, to be more concerned to know every thing, that makes a corporeal part of the world, than the divine and incorporeal author of the whole?

AND then, is it not, think you, a great piece of reſpect, that theſe men pay to thoſe truths, which God thought fit to ſend ſometimes prophets and apoſtles, ſometimes angels, and ſometimes his only Son himſelf, to reveal, that ſuch truths are ſo little valued by them, that rather than take the pains to ſtudy them, they will implicitly, and at adventures believe, what that ſociety of Chriſtians, they chance to be born and bred in, have, truly or falſely, delivered, concerning them? And does it argue a due regard to points of religion, that thoſe, who would not believe a propoſition in ſtaticks, perhaps about a more point, the centre of gravity, or in geometry, about the properties of ſome nameleſs curve line, or ſome ſuch other things, (which to ignore, is uſually not a blemiſh, and about which to be miſtaken, is more uſually without danger) ſhould yet take up the articles of faith, concerning matters of great and everlaſting conſequence, upon the authority of men, fallible as themſelves, when ſatisfaction may be had without them from the infallible word of Acts xvii. 11. God? in this very unlike thoſe Bereans, whom the Evangeliſt honours with the title of Noble, that when the doctrines of the goſpel were propoſed to them, "they ſearched the ſcriptures "daily, whether thoſe things were ſo."

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* Search, or, you ſearch the Scriptures.

AGAIN, if a man ſhould refuſe to learn to read any more, than juſt as much as may ſerve his turn, by intituling him to the benefit of the clergy, to ſave him from hanging, would theſe men think ſo ſmall a meaſure of literature, as he had acquired on ſuch an account, could prove that man to be a lover of learning; and yet a neglecter of the ſtudy of all not abſolutely neceſſary-divine truths, during one's life, becauſe the belief of the articles of the creed may make a ſhift to keep him from being doomed to hell for ignorance after his death, will not by (what in a learned man muſt be) ſo pitiful a degree of knowledge be much better intitled to that ingenuous love of God and his truths, that becomes a rational creature and a Chriſtian.

THE ancient prophets, though honoured by God with direct illuminations, were yet 1 Pet. i. 10, 11. very ſolicitous to find out and learn the very circumſtances of the evangelical diſpenſations, which yet they did not know. And ſome of the goſpel myſteries are of ſo noble and excel- 1 Pet. i. 12. lent a nature, that "the angels themſelves de"ſire to look into them." And though all the evangelical truths are not preciſely neceſſary to be known, it may be both a duty not to deſpiſe the ſtudy of them, and a happineſs to employ ourſelves about it. It was the earneſt prayer of a great king, and no leſs a pro- Pſal. cxix. 18. phet, that his eyes might be opened to behold (not the obvious and neceſſary truths, but) the wondrous things of God's law. He is pronounced happy in the beginning of the Rev. i. Apocalypſe, that reads and obſerves the things contained in that dark and obſcure part of ſcripture. And it is not only thoſe truths, that make articles of the creed, but divers other doctrines of the goſpel, that Chriſt himſelf judged worthy to be concluded with this epi- Mat. xi. 15. Mark iv. 9, 23. Luke viii. 8. phonema, "He that hath ears to hear, let him "hear;" on which the excellent *Grotius* makes this juſt paraphraſe, *Intellectus nobis à Deo potiſſimum datus eſt, ut eum intendamus documentis ad pietatem pertinentibus.*

SECTION III.

I COME now to our third and laſt inducement to the ſtudy of divine things, which conſiſts in, and compriſes the advantages of that ſtudy, which do as much ſurpaſs thoſe of all other contemplations, as divine things tranſcend all other objects. And indeed, the utility of this ſtudy is ſo pregnant a motive, and contains in it ſo many invitations, that your friend muſt have as little ſenſe of intereſt, as of gratitude, if he can neglect ſuch powerful and ſuch engaging invitations.

FOR, in the firſt place, theological ſtudies ought to be highly endeared to us, by the delightfulneſs of conſidering ſuch noble and worthy objects, as are therein propoſed.

THE famous anſwer given by an excellent philoſopher, who being aſked, what he was born for? replied, "To contemplate the ſun," may juſtly recommend their choice, who ſpend their time in contemplating the maker of the ſun, to whom that glorious planet it ſelf is but a ſhadow. And perhaps that philoſopher failed more in the inſtance than in the notion: for his anſwer implies, that man's end and happineſs conſiſt in the exerciſe of his nobleſt faculties on the nobleſt objects. And ſurely the ſeat of formal happineſs being the ſoul, and that happineſs conſequently conſiſting in the operations of her faculties; as the ſupreme faculty of the mind is the underſtanding, ſo the higheſt pleaſures may be expected from the due exerciſe of it upon the ſublimeſt and worthieſt objects. And therefore I wonder not, that though ſome of the ſchool-men would aſſign the will a larger ſhare in man's felicity, than they will allow the intellect; yet the generality of them are quite of another mind, and aſcribe the preheminence, in point of felicity, to the ſuperior faculty of the ſoul. But whether or no this opinion be true in all caſes, it may, at leaſt, be admitted in ours: for the chief objects of a Chriſtian philoſopher's contemplation being as well the infinite goodneſs, as the other boundleſs perfections of God, they are naturally fitted to excite in his mind an ardent love of that adorable Being, and thoſe other joyous affections and virtuous diſpoſitions, that have made ſome men think happineſs chiefly ſeated in the will. But having intimated thus much by the way, I paſs on to add, that the contentment afforded by the aſſiduous diſcovery of God and divine myſteries has ſo much of affinity with the pleaſures, that ſhall make up men's bleſſedneſs in heaven it ſelf, that they ſeem rather to differ in degree than in kind. For the happy ſtate even of angels is by our Saviour repreſented by this employment, "That they continually ſee the face of his father, who is in heaven." And the ſame infallible teacher, intending elſewhere to expreſs the celeſtial joys, that are reſerved for thoſe, who for their ſake denied themſelves ſenſual pleaſures, employs the viſion of God as an emphatical periphraſe of felicity, "Bleſſed, ſaid

Matth. v. 8. ſaid he, are the pure in heart, for they ſhall ſee God." And as *Ariſtotle* teaches, that the ſoul doth after a ſort become that, which it ſpeculates, St. *Paul* and St. *John* aſſure us, that God is a transforming object, and that in heaven "we ſhall be like him, for (or, becauſe) 1 John iii. 2. Ὅτι. "we ſhall ſee him, as he is." And though I readily admit, that this beatifick viſion of God, wherein the underſtanding is the proper inſtrument, includes divers other things, which will concur to the complete felicity of the future life; yet I think, we may be allowed to argue, that that raviſhing contemplation of divine objects will make no ſmall part of that happy eſtate, which in theſe texts take its denomination from it.

I have above intimated, that the ſcripture attributes to the angels themſelves tranſports of wonder and joy upon the contemplation of God, and the exerciſes they conſider of his wiſdom, juſtice, or ſome other of his attributes. But leſt, in referring you to the angels, you ſhould ſay, that I do in this diſcourſe lay aſide the perſon of a naturaliſt, in favour of divines; I will refer you to *Des Cartes* himſelf, whom I am ſure your friend will allow to have been a rigid philoſopher, if ever there were any. Thus then ſpeaks he in that treatiſe, where he thinks he employs a more than mathematical rigor; and where he was obliged to utter thoſe (I had almoſt ſaid paſſionate) words, I am going to cite from him, only by the impreſſions made on him by the tranſcendent excellency of the object he contemplated, Medit. tertia ſub finem. *Sed priuſquam* (ſays he) *hoc diligentius examinem, ſimulque in alias veritates, quæ inde colligi poſſunt, inquiram, placet hic aliquandiu in ipſius Dei contemplatione immorari, ejus attributa apud me expendere, & immenſi hujus luminis pulchritudinem, quantum caligantis ingenii mei acies ferre poterit, intueri, admirari, adorare. Ut enim in hac ſola Divinæ Majeſtatis contemplatione ſummam alterius vitæ felicitatem conſiſtere fide credimus; ita etiam jam ex eadem, licet multo minus perfecta, maximam, cujus in hac vita capaces ſimus, voluptatem, percipi poſſe experimur.*

But as high a ſatisfaction as the ſtudy of divine things affords by the nobleneſs of its object, the contentment is not much inferior, that accrues from the ſame ſtudy, upon the ſcore of the ſenſe of a man's having in it performed his duty. To make actions of this nature ſatisfactory to us, there is no need, that the things we are employed about, ſhould in themſelves be excellent or delightful; the inward gratulations of conſcience for having done our duties is able to gild the bittereſt pills, and like the wood, that grew by the waters of Exod. xv. 25. *Marah*, to correct and ſweeten that liquor, which before was the moſt diſtaſteful. Thoſe ancient Pagan heroes, whoſe virtues may make us bluſh, being guided but by natural reaſon, and innate principles of moral virtues, could find the moſt difficult and moſt troubleſome duties, upon the bare account of their being duties, not only tolerable, but pleaſant. And though to deny ſome luſts be, in our Saviour's eſteem, no leſs uneaſy, than for a man to pluck out his right eye, or cut off his right Matth. v. 29, 30. hand; yet even ladies have with ſatisfaction choſen, not only to deny themſelves the greateſt pleaſures of the ſenſes, but to ſacrifice the ſeat of them, the body it ſelf, to preſerve the ſatisfaction of being chaſte. Nor are they only the dictates of obedience, that we comply with in this ſtudy, but thoſe of gratitude; and that is a virtue, that has ſo powerful an aſcendant upon ingenuous minds, that thoſe, whoſe principles and aims were not elevated by religion, have, in acknowledgment to their parents and their country, courted the greateſt hardſhips, and hazards, and ſufferings, as if they were as great delights and advantages. And a grateful perſon ſpends no part of his life to his greater ſatisfaction, than that, which he ventures or employs for thoſe to whom he is obliged for it; and oftentimes finds a greater contentment even in the difficulteſt acknowledgments of a favour, than he did in receiving of it.

ANOTHER advantage, and that no mean one, that may accrue from the contemplation of theological truths, is, the improvement of the contemplator himſelf in point of piety and virtue. For, as the goſpel is ſtiled, the myſtery of godlineſs; and St. *Paul* elſewhere calls 1 Tim. iii. 16. what it teaches, the truth, which is according to godlineſs, that is, a doctrine framed and Tit. i. 1. fitted to promote the intereſt of piety and virtue in the world: ſo this character and encomium belongs (though perhaps not equally) to the more retired truths diſcovered by ſpeculation, as well as to thoſe more obvious ones, that are familiarly taught in catechiſms and confeſſions of faith. I would by no means leſſen the excellency and prerogatives of fundamentals; but, ſince the grand and nobleſt engagements to piety and virtue are a high veneration for God and his Chriſt, and an ardent love of them; I cannot but think, that thoſe particular enquiries, that tend to make greater diſcoveries of the attributes of God, of the nature, and offices, and life of our ſaviour, and of the wiſdom and goodneſs they have diſplayed in the contrivance and effecting of man's redemption, do likewiſe tend to encreaſe our admiration, and inflame our love, for the poſſeſſors of ſuch divine excellencies, and the authors of ſuch invaluable benefits. And as the brazen ſerpent, that was but a Numb. xxi. 9. type of one of the goſpel myſteries, brought recovery to thoſe, that looked up to it; ſo the myſteries themſelves, being duly conſidered, have had a very ſanative influence on many, that contemplated them. Nor is it likely, that he, that diſcerns more of the depth of God's wiſdom and goodneſs, ſhould not, *cæteris paribus*, be more diſpoſed than others to admire him, to love him, to truſt him, and ſo to reſign up himſelf to be governed by him: which frame of mind both is itſelf a great part of the worſhip of God, and doth directly tend to the production and increaſe of thoſe vertues, without the practice of which, the ſcripture plainly tells us, that we can neither obey God, nor expreſs our love to him. And from this bettering of the mind by the ſtudy of theology, will flow (to add that upon the by) another benefit, namely, that by giving us a higher value for God and his truths, it will endear heaven to us, and ſo not only aſſiſt us to come thither, but heighten our felicity there.

I know it may be ſaid, that the melioration of the mind is but a moral advantage. But give me leave to anſwer, that beſides that it is ſuch a moral advantage, as ſuppoſes an intellectual improvement, whoſe fruit it is, a moral benefit may be great enough, even in the judgment of a mere philoſopher, and an Epicurean, to deſerve as much ſtudy as natural philoſophy itſelf. And that you may not think, that I ſpeak this only, becauſe I write in this epiſtle as a friend to divines, I will tell you, that *Epicurus* himſelf, who has now a-days ſo numerous a ſect of naturaliſts to follow him, ſtudied phyſicks, and writ ſo many treatiſes about them for this end, that by knowing the natural cauſes of thunder, lightning, and other dreadful phænomena, the mind might be freed from the diſquieting apprehenſions men commonly had, that ſuch ſtrange and formidable things proceeded from ſome incenſed deity, and ſo might trouble the mind, as well as the air. This account I have been giving of *Epicurus*'s deſign, is but what ſeems plainly enough intimated by his own words, preſerved us by *Laertius*, near the end of his phyſiological epiſtle to *Herodotus*, where recommending to him the conſideration of what he had delivered about phyſical principles in general, and meteors in particular, he ſubjoins, *Si enim ab iſtis non diſceſſerimus, tum id unde oritur perturbatio, quodque metum ingerit, recta cum ratione ediſſeremus, noſque ab ipſis eximemus.* And to this in the cloſe of his meteorological epiſtle to *Pythocles*, his beſt interpreter, *Gaſſendus*, makes him ſpeak conſtantly in theſe words, *Maxime verò dede teipſum ſpeculationi principiorum, ex quibus conſtant omnia, & infinitatis naturæ, aliorumque his cohærentium. Inſuper verò criteriorum, affectuumque animi, & ſcopum illius, in quem iſta ediſſerentes collineavimus, attende, tranquillitatem intelligo ſtatumque mentes imperturbatum.* But this is not all the teſtimony I can give you from *Epicurus* himſelf to the ſame purpoſe; for among his *Ratæ Sententiæ*, preſerved us by *Laertius*, (himſelf reputed an Epicurean) I find one, that goes further; *Si nihil*, ſays he, *conturbaret nos quod ſuſpicamur, veremurque ex rebus ſublimibus, neque item quod ex ipſa morte, ne quando nimirum ad nos pertineat aliquid, ac noſſe præterea poſſemus, qui Germani fines dolorum atque cupiditatum ſint* (οὐκ ἂν προσδεόμεθα φυσιολογίας) *nihil phyſiologiâ indigeremus.* Thus far the teſtimony of *Epicurus*, of whoſe mind though I am not at all, as to what he would intimate, “That phyſiology is either proper to free the mind from the belief of a provident deity, and the ſoul's immortality, or fit for no other conſiderable purpoſes;” yet this uſe we may well make of theſe declarations, that in *Epicurus*'s opinion a moral advantage, that relates to the government of the affections, may deſerve the pains of making enquiries into nature. And ſince it hence appears, that a mere philoſopher, who admitted no providence, may think it worth his pains, to ſearch into the abſtruſeſt parts of phyſicks, and the difficulteſt phænomena of nature, only to eaſe himſelf of one troubleſome affection, fear; it need not be thought unphiloſophical, to proſecute a ſtudy, that will not only reſtrain one undue paſſion, but advance all virtues, and free us from all ſervile fears of the Deity; and tend to give us a ſtrong and well-grounded hope in him; and make us look upon God's greateſt power, not with terror, but with joy.

Diogenis Laertii lib. 10.

THERE is yet another advantage belonging to the ſtudy of divine truths, which is too great to be here pretermitted. For whereas there is ſcarce any thing more incident to us whilſt we inhabit our (*batté chómer*) cottages of clay, and dwell in this vale of tears, than afflictions; it ought not a little to endear to us the newly mentioned ſtudy, that it may be eaſily made to afford us very powerful conſolations in that otherwiſe uneaſy ſtate.

Job. iv. 19.

I know it may be ſaid, that the ſpeculations, about which the naturaliſt is buſied, are as well pleaſing diverſions, as noble employments of the mind. And I deny not, that they are often ſo, when the mind is not hindered from applying it ſelf attentively to them; ſo that afflictions ſlight and ſhort may well be weathered out by theſe philoſophical avocations; but the greater and ſharper ſort of afflictions, and the approaches of death, require more powerful remedies, than theſe diverſions can afford us. For in ſuch caſes, the mind is wont to be too much diſcompoſed, to apply the attention requiſite to the finding a pleaſure in phyſical ſpeculations; and in ſickneſſes, the ſoul is oftentimes as indiſpoſed to reliſh the pleaſures of merely human ſtudies, as the languiſhing body is to reliſh thoſe meats, which at other times were delightful: and there are but few, that can take any great pleaſure to ſtudy the world, when they apprehend themſelves to be upon the point of being driven out of it, and in danger of loſing all their ſhare in the objects of their contemplation. It will not much qualify our ſenſe of the burning heat of a fever, or the painful gripes of the cholick, to know, that the three angles of a triangle are equal to two right ones; or, that heat is not a real quality (as the ſchools would have it,) but a modification of the motion of the inſenſible parts of matter; and pain not a diſtinct, inherent quality in the things, that produce it, but an affection of the ſentiment. The naturaliſt's ſpeculations afford him no conſolations, that are extraordinary in, or peculiar to the ſtate of affliction; and the avocations, they preſent him with, do rather amuſe the mind from an attention to leſſer evils, than bring it any advantages to remove or compenſate them, and ſo work rather in the nature of opiates, than of true cordials.

BUT now, if ſuch a perſon as Dr. *N.* falls into adverſity, the caſe is much otherwiſe; for we muſt conſider, that when the ſtudy of divine things is ſuch as it ought to be, though that in itſelf, or in the nature of the employment, be an act or exerciſe of reaſon; yet being

ing applied to, out of obedience, and gratitude, and love to God, it is upon the account of its motives, and its aim, an act of religion; and as it proceeds from obedience, and thankfulneſs, and love to God, ſo it is moſt acceptable to him; and upon the account of his own appointment, as well as goodneſs, is a moſt proper and effectual means of obtaining his favour; and then I preſume, it will eaſily be granted, that he, who is ſo happy as to enjoy that, can ſcarce be made miſerable by affliction. For not now to enter upon the common place of the benefits of afflictions to them, that love God, and to them, that are loved by him, it Pſal. ciii. 14. may ſuffice, that he, who (as the ſcripture ſpeaks) knows our frame, and has promiſed thoſe, that are his, that they ſhall not be over- 1 Cor. xx. 13. burdened, is diſpoſed and wont to give his afflicted ſervants, both extraordinary comforts in afflictions, and comforts appropriated to that ſtate. For though natural philoſophy be like its brighteſt object, the ſtars, which, however the aſtronomer can with pleaſure contemplate them, are unable, being mere natural agents, to afford him a kinder influence than uſual, in caſe he be caſt upon his bed of languiſhing, or into priſon; yet the almighty and compaſſionate maker of the ſtars, being not only a voluntary, but the moſt free agent, can ſuit and proportion his reliefs to our neceſſities, and alleviate our heavieſt afflictions by ſuch ſupporting conſolations, that not only they can never ſurmount our patience, but are oftentimes unable ſo much as to hinder our joy; and Job xviii. 14. when death, that king of terrors, preſents itſelf, whereas the mere naturaliſt ſadly expects to be deprived of the pleaſure of his knowledge, by loſing thoſe ſenſes and that world, which are the inſtruments and the objects of it; and perhaps, diſcovering beyond the grave nothing but either a ſtate of eternal deſtruction, or of eternal miſery, fears either to be confined for ever to the ſepulchre, or expoſed to torments, that will make even ſuch a condition deſirable; the pious ſtudent of divine truths is not only freed from the wracking apprehenſions of having his ſoul reduced to a ſtate of annihilation, or caſt into hell, but enjoys a comfortable expectation of finding far greater ſatisfaction than ever, in the ſtudy he now rejoices to have purſued; ſince the change, that is ſo juſtly formidable to others, will but bring him much nearer to the divine objects of his devout curioſity, and ſtrangely elevate and enlarge his faculties to apprehend them.

And this leads me to the mention of the laſt advantage belonging to the ſtudy I would perſuade you to; and indeed, the higheſt advantage, that can recommend any ſtudy, or invite men to any undertaking; for this is no leſs than the everlaſting fruition of the divine objects of our ſtudies hereafter, and the comfortable expectation of it here. For the employing of one's time and parts, to admire the nature and providence of God, and contemplate the divine myſteries of religion, as it is one of the chief of thoſe homages and ſervices, whereby we venerate and obey God; ſo it is one of thoſe, to which he hath been pleaſed to apportion no leſs a recompence, than (that which can have no greater) the Dan ix. 21, 22. enjoyment of himſelf. The ſaints and angels Luke i. 11, 26. in heaven have divers of them been employed Acts x. 4, 5, 6. to convey the truths of theology, and are ſolicitous to look into thoſe ſacred myſteries; 1 Pet. i. 12. and God hath been pleaſed to appoint, that thoſe men who ſtudy the ſame leſſons, that they do here, ſhall ſtudy them in their company hereafter. And doubtleſs, though heaven abound with unexpreſſible joys, yet it will be none of the leaſt, that ſhall make up the happineſs, even of that place, that the knowledge of divine things, that was here ſo zealouſly purſued, ſhall there be compleatly attained. For thoſe things, that do here moſt excite our deſires, and quicken the curioſity and induſtry of our ſearches, will not only there continue, but be improved to a far greater meaſure of attractiveneſs and influence. For all thoſe intereſts, and paſſions, and luſts, that here below either hinder us from clearly diſcerning, or keep us from ſufficiently valuing, or divert us from attentively enough conſidering, the beauty and harmony of divine truths, will there be either aboliſhed, or transfigured: and as the object will be unveiled; ſo our eye will be enlightened, that is, as God will there diſcloſe thoſe worthy objects of the angels curioſity, ſo he will enlarge our faculties, to enable us to gaze, without being dazled, upon thoſe ſublime and radiant truths, whoſe harmony, as well as ſplendor, we ſhall be then qualified to diſcover, and conſequently with tranſports to admire. And this enlargement and elevation of our faculties will, proportionably to its own meaſure, increaſe our ſatisfaction at the diſcoveries it will enable us to make. For theology is like a heaven, which wants not more ſtars than appear in it, but we want eyes, quick-ſighted and piercing enough to reach them. And as the milky way, and other whiter parts of the firmament, have been full of immortal lights from the beginning, and our new teleſcopes have not placed, but found them, there; ſo, when our Saviour, after his glorious reſurrection, inſtructed his apoſtles to teach the goſpel, it is not ſaid, that he altered any thing in the Luke xxiv. 45. ſcriptures of *Moſes* and the prophets, but only opened and enlarged their intellects, that Pſal. cxix. 18. they might underſtand the ſcriptures: and the royal prophet makes it his prayer, "That "God would be pleaſed to open his eyes, that "he might ſee wonderful things out of the "law;" being (as was above intimated) ſo well ſatisfied, that the word of God wanted not admirable things, that he is only ſolicitous for the improvement of his own eyes, that they might be qualified to diſcern them.

I had almoſt forgotten one particular about the advantages of theological ſtudies, that is too conſiderable to be left unmentioned: for as great as I have repreſented the benefits accruing from the knowledge of divine truths; yet to endear them to us, it may be ſafely added, that, to procure us theſe benefits, the actual attainment of that knowledge is not always abſolutely neceſſary, but a hearty endeavour after it may ſuffice to entitle us to them. The patient chemiſt, that conſumes himſelf and his eſtate in ſeeking after the philoſopher's ſtone, if he miſs of his idolized elixir, had as good, nay better, have never ſought it, and remains as poor in effect, as he was rich in expectation. The huſbandman, that employs his ſeed and time, to obtain from the ground a plentiful harveſt, if, after all, an unkind ſeaſon happen, muſt ſee his toil made fruitleſs;

> ———*longique perit labor irritus anni.*

Too many patients, that have punctually done and ſuffered for recovery, all, that phyſicians could preſcribe, meet at laſt with death, inſtead of health. You know what entertainment has been given by ſkilful geometricians to the laborious endeavours, even of ſuch famous writers as *Scaliger*, *Longomontanus*, and other Tetragoniſts; and that their ſucceſſor Mr. *Hobbes*, after all the ways he has taken, and thoſe he has propoſed, to ſquare the circle, and double the cube, by miſſing of his end, has, after his various attempts, come off, not only with diſappointment, but with diſgrace. And (to give an inſtance even in things celeſtial) how much pains has been taken to find out longitudes, and make aſtrological predictions with ſome certainty, which for want of coming up to what they aimed at, have been uſeleſs, if not prejudicial to the attempters?

Acts xvii. 24, 25. BUT God (to ſpeak with St. *Paul* on another occaſion) "that made the world, and all "things therein, and is lord of heaven and "earth," ſeeks not our ſervices, as though he "needed any thing, ſeeing he giveth life, and "breath, and all things:" his ſelf-ſufficiency and bounty are ſuch, that he ſeeks in our obedience the occaſions of rewarding it, and preſcribes us ſervices, because the practiſe of them is not only ſuitable to our rational nature, but ſuch as will prevail with his juſtice, to let his goodneſs make our perſons happy. Agreeably Jam. ii. 21. to this doctrine we find in the ſcripture, that *Abraham* is ſaid to have been "juſtified by "faith, when he offered his ſon *Iſaac* upon the "altar," (though he did not actually ſacrifice him) becauſe he endeavoured to do ſo; although God graciouſly accepting the will for the deed, accepted alſo of the blood of a ram inſtead of *Iſaac*'s. And thus we know, that it was not *David*, but *Solomon*, that built the temple of *Hieruſalem*, and yet God ſays to 2 Chron. vi. 8, 9. the former of thoſe kings (as we are told by the latter) "Foraſmuch as it was in thine "heart to build an houſe for my name, thou "didſt well in that it was in thine heart, not"withſtanding thou ſhalt not build the houſe, "&c." And if we look to the other circum2 Sam. vii. ſtances of this ſtory, as they are delivered in the ſecond book of *Samuel*, we ſhall find, that upon *David*'s declaration of a deſign to build God an houſe, God himſelf vouchſafes to verſe 5. honour him, as he once did *Moſes*, with the peculiar title of his ſervant; and commands the verſe 11. prophet to ſay to him, "Alſo the Lord tells "thee, that he will make thee an houſe;" to which is added one of the graciouſeſt meſſages, that God ever ſent to any particular man. By which we may learn, that God approves and accepts even thoſe endeavours of his ſervants, if they be real and ſincere, that never come to be actually accompliſhed: good deſigns and endeavours are our part, but the events of thoſe, as of all other things, are in the all-diſpoſing hand of God, who, if we be not wanting to what lies in us, will not ſuffer us to be loſers by the defeating diſpoſitions of his providence; but crown our endeavours, either with ſucceſs, or with ſome other recompence, that will keep us from being loſers, by miſſing of that. And indeed, if we conſider the great elogies, that the ſcripture, as well frequently as juſtly, gives God's goodneſs (which it repreſents as over, or as above all his works) and Hab. i. 13. that his purer eyes puniſh, as well as ſee, the murder and adultery of the heart, when thoſe intentional ſins are hindered from advancing into actual ones; we can ſcarce doubt, but he, whoſe juſtice puniſhes ſinful aims, will allow his infinite goodneſs to recompenſe pious attempts: and therefore our Saviour pronounces Matth. v. 6. them bleſſed, that hunger and thirſt after righteouſneſs, aſſuring them, that they ſhall be ſatisfied, and thereby ſufficiently intimating to us, that an earneſt deſire, after a ſpiritual grace (and ſuch is the knowledge of divine things) may intitle a man to the complete poſſeſſion of it, if not in this life, yet in the next, where we ſhall not any more walk by faith, but by 2 Cor. v. 7. ſight, and obtain as well a knowledge as other endowments, befitting that glorious ſtate, wherein the purchaſer of it for us, aſſures us, Luke xx. 36. that we ſhall be [ἰσάγγελοι] equal, or like to the angels.

THE conſiderations, Sir, I have hitherto laid before you, to recommend the ſtudy of divine truths, have, I hope, perſuaded you, that it is on many accounts, both noble and eligible in itſelf; and therefore I ſhall here conclude the firſt part of this diſcourſe. And in regard, that the under-valuation *Phyſiophilus* expreſſes for that excellent employment, ſeems to flow (chiefly at leaſt) from his fondneſs and partiality for natural philoſophy; it will next concern us to compare the ſtudy of theology with that of phyſicks, and ſhew, that the advantages, which your friend alledges in favour of the latter, are partly much leſſened by diſadvantageous circumſtances, and partly much outweighed by the tranſcendent excellencies of theological contemplations: the ſtudy whereof will thereby appear to be not only eligible in itſelf, but preferable to its rival. And I muſt give you warning to expect to find the ſecond part, which the making this compariſon challenges to itſelf, a good deal more prolix than the firſt; not only becauſe it often requires more trouble, and more words to detect and diſprove an error, than to make out a truth; but alſo becauſe, that divers things tending to the credit of divinity, and which conſequently might have been brought into the firſt part of this diſcourſe, were thought more fit to be interwoven with other things, in the anſwers made to the objections examined in the ſecond.

THE
EXCELLENCY
OF
THEOLOGY:
OR,
The PRE-EMINENCE of the STUDY OF DIVINITY, above that of NATURAL PHILOSOPHY.

THE SECOND PART.

I SHALL, without preamble, begin this diſcourſe, by conſidering the delightfulneſs of phyſicks, as the main thing, that inveigles your friend, and divers other virtuoſi, from reliſhing, as they ought, and otherwiſe would, the pleaſantneſs of theological diſcoveries. And to deal ingenuouſly with you, I ſhall not ſcruple to acknowledge, that though the addreſs I have made to nature has laſted ſeveral years, and has been toilſome enough, and not unexpenſive; yet I have been pleaſed enough with the favours, ſuch as they are, that ſhe has from time to time accorded me, not to complain of having been unpleaſantly employed. But though I readily allow the attainments of naturaliſts to be able to give philoſophical ſouls ſincerer pleaſures, than thoſe, that the more undiſcerning part of mankind is ſo fond of; yet I muſt not therefore allow them to ſurpaſs, or even equal the contentment, that may accrue to a ſoul qualified by religion, to reliſh the beſt things moſt from ſome kind of theological contemplations.

THIS, I preſume, will ſufficiently appear, if I ſhew you, that the ſtudy of phyſiology is not unattended with conſiderable inconveniencies, and that the pleaſantneſs of it may be, by a perſon ſtudious of divinity, enjoyed with endearing circumſtances.

BUT before I name any of the particular reaſons, that I am to repreſent, I fear it may be requiſite to interpoſe a few words, to obviate a miſtake, which, if not prevented, may have an ill aſpect, not only upon the firſt ſection, but upon a great part of the following diſcourſe. For I know, that it may be ſaid, that whereas I alledge divers things, to leſſen the lately mentioned delightfulneſs of the ſtudy of phyſick, and to depreciate ſome other advantages, by which the following ſections would recommend it, ſome of the ſame things may be objected againſt the delightfulneſs of the ſtudy of divinity. But this objection will not, I preſume, much move you, if you conſider the argument and ſcope of the two parts of this letter. For in the former I have ſhewn by poſitive proofs, that the ſtudy of theology is attended with divers advantages, which belong to it, either only as ſome of them do, or principally as others. And now in the ſecond part I come to conſider, whether what is alledged in behalf of the ſtudy of philoſophy, deſerve to counter-balance thoſe prerogatives or advantages; and therefore it neither need be, nor is my deſign, to compare, for inſtance, the delightfulneſs of the two ſtudies, theology and phyſicks, but by ſhewing the inconveniencies, that allay the latter, to weaken the argument, that is drawn from that delightfulneſs, to conclude it preferable to the ſtudy of theology. So that my work, in this and the following ſections, is, not ſo much to inſtitute compariſons, as to obviate or anſwer allegations. For ſince I have in the paſt diſcourſe grounded the excellency of the ſtudy of divinity, chiefly upon thoſe great advantages, that are peculiar to it; my reaſonings would not be fruſtrated, though it ſhould appear, that in point of delightfulneſs, certainty, &c. that ſtudy ſhould, in many caſes, be liable to the ſame objections with the ſtudy of nature, ſince it is not mainly for theſe qualities, but, as I was ſaying, for other and peculiar excellencies, that I recommended divinity. And therefore, ſuppoſing the delightfulneſs, &c. of that and of phyſicks to be allayed by the ſame, or equal inconveniencies or imperfections; that ſuppoſition would not hinder the ſcales to be ſwayed in favour of divinity, upon the ſcore of thoſe advantages, that are unqueſtioned, and peculiarly belong to it. I know not, whether I need add, that notwithſtanding this, you are not to expect, that I ſhould give philoſophy the wounds of an enemy. For my deſign being not to diſcourage you, nor any ingenious man, from courting her at all, nor from courting her much, but from courting her too much, and deſpiſing divinity for her, I employ againſt her not a ſword to wound her, but a balance, to ſhew, that her excellencies, though ſolid and weighty, are leſs ſo, than the preponderating ones of theology. And this temper and purpoſe of mine renders my taſk difficult enough to have, perhaps, ſome right to your pardon, as well as ſome need of it, if I do not every way ſteer ſo exactly, as equally to avoid injuring the cauſe I am to plead for, and diſparaging a ſtudy, which I would ſo little depreciate, that I allow it a great part of my inclinations, and not a little ſhare of my time. And having ſaid this, to keep the deſign of this diſcourſe from being miſunderſtood, I hope we may now proceed to the particulars, whoſe ſcope we have been declaring.

Returning then to what I was about to ſay before this long, but needful, advertiſement interrupted me, I ſhall reſume my diſcourſe of the delightfulneſs of the ſtudy of phyſicks, about which I was going in the firſt place to tell you, that I know you and your friend will freely grant me, that the knowledge of the empty and barren phyſiology, that is taught in the ſchools, as it exacts not much pains to be acquired, ſo it affords but little ſatisfaction when attained. And as I know you will give me leave to ſay this; ſo, being warranted by no ſlight experience of my own, I ſhall take leave to ſay alſo, that the ſtudy of that experimental philoſophy, which is that, whereof your friend is ſo much enamoured, is, if it be duly proſecuted, a very troubleſome and laborious employment. For, (to mention at preſent but this) that great variety of objects the naturaliſt is not only by his curioſity, but by their ſecret dependances upon one another, engaged to conſider, and ſeveral ways to handle, will put him upon needing, and conſequently upon applying himſelf to ſuch a variety of mechanick people, (as diſtillers, druggiſts, ſmiths, turners, &c.) that a great part of his time, and perhaps all his patience, ſhall be ſpent in waiting upon tradeſmen, and repairing the loſſes he ſuſtains by their diſappointments, which is a drudgery greater than any, who has not tried it, will imagine, and which yet being as inevitable as unwelcome, does very much counter-balance and allay the delightfulneſs of the ſtudy we are treating of. In which ſo great a part of a man's care and time muſt be laid out in providing the apparatus's neceſſary for the trying of experiments.

But this is not all. For when you have brought an experiment to an iſſue, though the event may often prove ſuch as you will be pleaſed with, yet it will ſeldom prove ſuch as you can acquieſce in. For it fares not with an inquiſitive mind in ſtudying the book of nature, as in reading of *Æſop's* fables, or ſome other collection of apologues of differing ſorts, and independent one upon another; where, when you have read over as many at one time as you think fit, you may leave off when you pleaſe, and go away with the pleaſure of underſtanding thoſe you have peruſed, without being ſolicited by any troubleſome itch of curioſity to look after the reſt, as thoſe, which are needful to the better underſtanding of thoſe you have already gone over, or that will be explicated by them, and ſcarce without them. But in the book of nature, as in a well-contrived romance, the parts have ſuch a connection and relation to one another, and the things we would diſcover are ſo darkly or incompleatly knowable by thoſe, that precede them, that the mind is never ſatisfied till it comes to the end of the book; till when all that is diſcovered in the progreſs, is unable to keep the mind from being moleſted with impatience, to find that yet concealed, which will not be known, till one does at leaſt make a further progreſs. And yet the full diſcovery of nature's myſteries is ſo unlikely to fall to any man's ſhare in this life, that the caſe of the purſuers of them is at beſt like theirs, that light upon ſome excellent romance, of which they ſhall never ſee the latter parts. For indeed (to ſpeak now without a ſimile) there is ſuch a relation betwixt natural bodies, and they may in ſo many ways (and divers of them unobſerved) work upon, or ſuffer from one another, that he, who makes a new experiment, or diſcovers a new phænomenon, muſt not preſently think, that he has diſcovered a new truth, or detected an old error. For, (at leaſt if he be a conſidering man) he will oftentimes find reaſon to doubt, whether the experiment or obſervation have been ſo ſkilfully and warily made in all circumſtances, as to afford him ſuch an account of the matter of fact, as a ſevere naturaliſt would deſire. And then, ſuppoſing the hiſtorical part no way defective, there are far more caſes than are taken notice of, wherein ſo many differing agents may produce the exhibited phænomenon, or have a great influence upon the experiment or obſervation, that he muſt be leſs jealous than becomes a philoſopher, to whom experiments do not oftentimes as well ſuggeſt new doubts, as preſent new phænomena.

And even thoſe trials, that end in real diſcoveries, do, by reaſon of the connection of phyſical truths, and the relations that natural bodies have to one another, give ſuch hopes and ſuch deſires of improving the acquiſts we have already made, to the explicating of other difficulties, or the making of further diſcoveries, that an inquiſitive naturaliſt finds his work to encreaſe daily upon his hands, and the event of his beſt toils, whether it be good or bad, does but engage him into new ones, either to free himſelf from his ſcruples, or improve his ſucceſſes. So that, though the pleaſure of making phyſical diſcoveries is, in itſelf conſidered, very great; yet this does a little impair it, that the ſame attempts, which afford that delight, do ſo frequently beget both anxious doubts, and a diſquieting curioſity. So that, if knowledge be, as ſome philoſophers have ſtiled it, the aliment of the rational ſoul, I fear I may too truly ſay, that the naturaliſt is uſually fain to live upon ſallads and ſauces, which, though they yield ſome nouriſhment, excite more appetite than they ſatisfy, and give us indeed the pleaſure of eating with a good ſtomach, but then reduce us to an unwelcome neceſſity of always riſing hungry from the table.

Of divers things, that leſſen the delightfulneſs of phyſiological ſtudies, I do ſo amply diſcourſe in other papers, that I might well remit you thither; but indeed it is not neceſſary, that I ſhould inſiſt on this argument any further. It is true, that ſuch a reference might be very proper, if the myſteries of theology and phyſick were like thoſe of theology and necromancy, or ſome other part of unlawful magick, whereof the former could not be well reliſhed without an abhorrence of the latter. But as the two great books, of nature and of ſcripture, have the ſame author; ſo the ſtudy of the latter does not at all hinder an inquiſitive man's delight in the ſtudy of the former. The doctor I am pleading for, may as much reliſh a phyſical diſcovery, as *Phyſiophilus*; nay, by being addicted to theology and religion, he is ſo far from being uncapable of the contentments accruing from the ſtudy of nature, that beſide thoſe things, that recommend it to others, there are ſeveral things, that peculiarly endear it to him.

For 1. he has the contentment to look upon the wonders of nature, not only as the productions of an admirably wiſe author of things, but of ſuch an one, as he intirely honours and loves, and to whom he is related. He, that reads an excellent book, or ſees ſome rare engine, will be otherwiſe affected with the ſight or the peruſal, if he knows it to have been made by a friend, or a parent, than if he conſiders it but as made by a ſtranger, whom he has no particular reaſon to be concerned for. And if *Rehoboam* did not as well degenerate from the ſentiments of mankind, as from his family, he could not but look upon that magnificent temple of *Solomon* with another eye, than did the throngs of ſtrangers, that came only to gaze at it, as an admirable piece of architecture, whilſt he conſidered, that it was his father, that built it. And if, as we ſee, the ſame heroick actions, which we read in hiſtory, of ſome great monarch, that ſtrangers barely and unconcernedly admire, the natives of his country do not only venerate, but affectionately intereſt themſelves therein, becauſe they are his country-men, and their anceſtors were his ſubjects: how much may we ſuppoſe the ſame actions would affect them, if they had the honour to be that prince's children? We may well therefore preſume, that it is not without a ſingular ſatisfaction, that the contemplator, we are ſpeaking of, does in all the wonders of nature diſcover, how wiſe, and potent, and bountiful, that author of nature is, in whom he has a great intereſt, and that ſo great an one, as both to be admitted into the number of his friends, and adopted into the number of his ſons, and is thereby in ſome meaſure concerned in all the admirations and praiſes, that are paid, either by himſelf or others, to thoſe adorable attributes, that God has diſplayed in that great maſter-piece of power and wiſdom, the world. And when he makes greater diſcoveries in theſe expreſſes and adumbrations of the divine perfections, the delightfulneſs of his contemplation is proportionably increaſed upon ſuch an account, as that, which endears to the paſſionate lover of ſome charming beauty an excellent, above an ordinary, picture of her; becauſe that the ſame things, that make him, as it does other gazers, look upon it as a finer piece, make him look upon it as the more like his miſtreſs, and thereby entertain him with the ſublimer ideas of the beloved original; to whoſe tranſcendent excellencies he ſuppoſes, that the nobleſt repreſentations muſt be the moſt reſembling.

And there is a farther reaſon, why our contemplator ſhould find a great deal of contentment in theſe diſcoveries. For we have in our nature ſo much of imperfection, and with all ſo much of inclination to ſelf-love, that we do too confidently proportion our ideas of what God can do for us, to what we have already the knowledge, or the poſſeſſion of. And though, when we make it our buſineſs, we are able with much ado ſomewhat to enlarge our apprehenſions, and raiſe our expectations beyond their wonted pitch; yet ſtill they will be but ſcantly promoted and heightened, if thoſe things themſelves be but mean and ordinary, which we think we have done enough, if we make them ſurpaſs. A country villager, born and bred in a homely cottage, cannot have any ſuitable apprehenſions of the pleaſures and magnificence of a great monarch's court. And if he ſhould be bid to ſcrue up his imagination to frame ideas of them, they would be borrowed from the beſt tiled houſe he had ſeen in the market-towns, where he had ſold his turnips or corn, and the wedding-feaſt of ſome neighbouring farmer's daughter. And though a child in the mother's womb had the perfect uſe of reaſon, yet could it not in that dark cell have any ideas of the ſun or moon, or beauties or banquets, or algebra, or chemiſtry, and many other things, which his elder brothers, that breathe freſh air, and freely behold the light, and are in a more mature eſtate, are capable of knowing and enjoying. Now among thinking men, whoſe thoughts run much upon that future ſtate, which they muſt ſhortly enter into, but ſhall never paſs out of; there will frequently and naturally ariſe a diſtruſt, which, though ſeldom owned, proves oftentimes diſquieting enough. For ſuch men are apt to queſtion, how the future condition, which the goſpel promiſes, can afford them ſo much happineſs as it pretends to; ſince they ſhall in heaven but contemplate the works of God, and praiſe him, and converſe with him; all which they think may, though not immediately, be done by men here below, without being happy. But he, that by teleſcopes and microſcopes, dexterous diſſections, and well employed furnaces, &c. diſcovers the wondrous power and ſkill of him, that contrived ſo vaſt and immenſe a maſs of matter into ſo curious a piece of workmanſhip as this world, will pleaſingly be convinced of the boundleſs power and goodneſs of the great architect. And when he ſees, how admirably every animal is furniſhed with parts requiſite to his reſpective nature; and that there is particular care taken, that the ſame animal, as for example, man, ſhould have differing proviſions made for him, according to his differing ſtates within the womb, and out of it, (a human egg, and an embrio, being being much otherwiſe nouriſhed and fitted for action, than is a (compleat) man;) he, I ſay, who conſiders this, and obſerves the ſtupendous providence, and excellent contrivances, that the curious priers into nature (and none but they) can diſcover, will be as well enabled as invited to reaſon thus within himſelf; that ſure God, who has with ſuch admirable artifice framed ſilk-worms, butterflies, and other meaner inſects, and with ſuch wonderful providence taken care, that the nobler animals ſhould as little want any of all the things requiſite to the compleating of their reſpective natures; and who, when he pleaſes, can furniſh ſome things with qualifications quite differing from thoſe, which the knowledge of his other works could have made us imagine, (as is evident in the load-ſtone, and in quick-ſilver among minerals, and the ſenſitive plant among vegetables, the camelion among animals, &c.) this God, I ſay, muſt needs be fully able to furniſh thoſe he delights to honour, with objects ſuitable to their improved faculties, and with all, that is requiſite to the happineſs he intends them in their glorified ſtate; and is able to bring this to paſs by ſuch amazing contrivances, as perhaps will be quite differing from any, that the things we have yet ſeen ſuggeſt to us any ideas of. And ſure he, that has in ſo immenſe, ſo curious, and ſo magnificent a fabrick, made ſuch proviſion for men, who are either deſperately wicked, or but very imperfectly good, and in a ſtate, where they are not to enjoy happineſs, but by obedience and ſufferings to fit themſelves for it, may ſafely be truſted with finding them in heaven employments and delights becoming the felicity he deſigns them there; as we ſee, that here below he provides as well for the ſoaring eagle, as for the creeping caterpillar, (and is able to keep the ocean as fully ſupplied with rivers, as lakes or ponds are with ſprings and brooks.) And as a ſtate of celeſtial happineſs is ſo great a bleſſing, that thoſe things, that afford us either greater aſſurances, or greater foretaſtes of it, are of the number of the greateſt contentments and advantages, that ſhort of it we can enjoy; ſo it is hard for any divine to receive ſo much of this kind of ſatisfaction, as he, who by ſkilfully looking into the wonders of nature, has his apprehenſions of God's power and manifold wiſdom (as an apoſtle calls it) elevated and enlarged. As when the queen of *Sheba* had particularly ſurveyed the aſtoniſhing prudence, that *Solomon* diſplayed in the ordering of his magnificent court, ſhe tranſportedly concluded thoſe ſervants of his to be happy enough to deſerve a monarch's envy, that were allowed the honour and priviledge of a conſtant and immediate attendance on him.

Epheſ. iii. 10.

SECTION II.

I Doubt not but you have too good an opinion of your friend, not to think, that you may alledge in his favour, that the chief thing, which makes him prefer phyſiology to all other kind of knowledge, is, that it enables thoſe, who are proficients in, it to do a great deal of good, both by improving of trades, and by promoting of phyſick it ſelf. And I am too mindful of what I writ to *Pyrophilus*, to deny, either that it can aſſiſt a man to advance phyſick and trades, or that, by ſo doing, he may highly advantage mankind. And this I (who would not leſſen your friend's eſteem for phyſicks, but only his partiality) willingly acknowledge to be ſo allowable an endearment of experimental philoſophy, that I do not know any thing, that to men of a humane, as well as ingenious diſpoſition, ought more to recommend the ſtudy of nature, except the opportunity it affords men to be juſt and grateful to the author both of nature and of man. I do not then deny, that the true naturaliſt may very much benefit mankind; but I affirm, that, if men be not wanting to themſelves, the divine may benefit them much more. It were not perchance either unſeaſonable, or impertinent, to tell you on this occaſion, that he, who effectually teaches men to ſubdue their luſts and paſſions, does as much as the phyſician contribute to the preſervation of their bodies, by exempting them from thoſe vices, whoſe no leſs uſual than deſtructive effects are wars, and duels, and rapines, and deſolations, and the pox, and ſurfeits, and all the train of other diſeaſes, that attend gluttony and drunkenneſs, idleneſs and luſt; which are not enemies to man's life and health barely upon a phyſical account, but upon a moral one, as they provoke God to puniſh them with temporal as well as ſpiritual judgments; ſuch as plagues, wars, famines, and other publick calamities, that ſweep away a great part of mankind; beſides thoſe perſonal afflictions of bodily ſickneſs, and diſquiets of conſcience, that do both ſhorten men's lives, and imbitter them. Whereas piety having (as the ſcripture aſſures us) promiſes both of this life, and of that, which is to come, thoſe teachers, that make men virtuous and religious, by making them temperate, and chaſte, and inoffenſive, and calm, and contented, do not only procure them great and excellent diſpoſitions to thoſe bleſſings, both of the right hand and of the left, which God's goodneſs makes him forward to beſtow on thoſe, who by grace and virtue are made fit to receive them; but do help them to thoſe qualifications, that by preſerving the mind in a calm and cheerful temper, as well as by affording the body all, that temperance can confer, do both lengthen their lives, and ſweeten them. Theſe things, I ſay, it were not impertinent to inſiſt on; but I will rather chuſe to repreſent to you, that the benefits, which men may receive from the didivine, ſurpaſs thoſe, which they receive from the naturaliſt, both in the nobleneſs of the advantages, and in the duration of them.

Be it granted then, that the naturaliſt may much improve both phyſick and trades; yet ſince theſe themſelves were deviſed for the ſervice of the body, (the one to preſerve or reſtore his health, and the other to furniſh it with accommodations or delights;) the boaſted uſe of natural philoſophy, by its advancing trades and phyſick, will ſtill be to ſerve the body, which is but the lodging and inſtrument of the ſoul, and which, I preſume, your friend, and which I am ſure your ſelf will be far from thinking the nobleſt part of man. I know it may be ſaid, nor do I deny it, that divers mechanical arts are highly beneficial, not only to the inventors, but to thoſe places, and perhaps thoſe ſtates, where ſuch improvements are found out and cheriſhed. But though I moſt willingly grant, that this conſideration ought to recommend experimental philoſophy, as well to ſtates as to private perſons; yet many of theſe improvements do rather transfer than encreaſe mankind's goods, and prejudice one ſort of men as much as they advantage another, (as in the caſe of the eaſtern ſpices, of whoſe trade the Portugals and Dutch by their later navigations, did, by appropriating it to themſelves, deprive the Venetians) or elſe do but increaſe that, which, though very beneficial to the producers, is not really ſo to mankind in general: of which we have an example in the invention of extracting gold and ſilver out of the oar, with mercury. For though it have vaſtly enriched the Spaniards in the *Weſt Indies*, yet it is not of any ſolid advantage to the world; no more than the diſcovery of the Peruvian and other American mines; by which, (eſpecially reckoning the multitudes of unhappy men, that are made miſerable, and deſtroyed in working them;) mankind is not put into a better condition than it was before. And if the philoſopher's ſtone it ſelf, (ſuppoſing there be ſuch a thing) were not an incomparable medicine, but were only capable of tranſmuting other metals into gold, I ſhould perhaps doubt, whether the diſcoverer of it would much advantage mankind; there being already gold and ſilver enough to maintain trade and commerce among men; and for all other purpoſes, I know not, why a plenty of iron, and braſs, and quickſilver, which are far more uſeful metals, ſhould not be more deſirable. But not to urge this; we may conſider, that theſe advancements of enriching trades do ſtill bring advantages but to the outward man, and thoſe many arts and inventions, that aim at the heightening the pleaſures of the ſenſes*, belong but to the body; and even in point of gratifying that, are not ſo requiſite and important, as many ſuppoſe; education, cuſtom, &c. having a greater intereſt, than moſt imagine, in the reliſh men have even of ſenſitive pleaſures. And as for phyſick, not to mind you, that it has been loudly, (how juſtly, I here examine not,) complained of, that the new philoſophy has made it far greater promiſes than have yet been performed; I ſhall only take notice, that ſince all, that phyſick is wont to pretend to, is, to preſerve health, or reſtore it; there are multitudes in the world, that have no need of the aſſiſtance the naturaliſt would give the phyſician; and a healthy man, as ſuch, is already in a better condition, than the philoſopher can hope to place him in, and is no more advantaged by the naturaliſt's contribution to phyſick, than a ſound man, that ſleeps in a whole ſkin, is by all the fine tools of a ſurgeon's caſe of inſtruments, and the various compoſitions of his cheſt.

AND as the benefits, that may be derived from theology, much ſurpaſs thoſe, that accrue from phyſicks, in the nobleneſs of the ſubject they relate to; ſo have they a great advantage in point of duration. For all the ſervices, that medicines, and engines, and improvements can do a man, as they relate but to this life, ſo they determine with it. Phyſick indeed, and chymiſtry do, the one more faintly, and the other more boldly, pretend ſometimes, not only to the cure of diſeaſes, but the prolongation of life; but ſince none will ſuſpect, but that the maſters of thoſe parts of knowledge would employ their utmoſt ſkill to protract their own lives, thoſe, that remember, that *Solomon* and *Helmont* lived no longer, than millions, that were ſtrangers to philoſophy; and that even *Paracelſus* himſelf, for all his boaſted *Arcana*, is by *Helmont* and other chemiſts confeſſed to have died ſome years ſhort of fifty; we may very juſtly fear, that nature will not be ſo kind to her greateſt votaries, as to give them much more time than other men, for the payment of the laſt debt all men owe her. And if a few years reſpite could by a ſcrupulous and troubleſome uſe of diet and remedies be obtained; yet that, in compariſon of the eternity, that is to follow, is not at all conſiderable. But whereas within no great number of years, (a little ſooner, or a little later) all the remedies, and reliefs, and pleaſures, and accommodations, that philoſophical improvements can afford a man, will not keep him from the grave, (which within very few days will make the body of the greateſt virtuoſo as hideous and as loathſome a carcaſe, as that of any ordinary man;) the benefits, that may accrue to us by divinity, as they relate chiefly, though not only, to the other world; ſo they will follow us out of this, and prove then incomparably greater than ever, when they alone ſhall be capable of being enjoyed. So that philoſophy, in the capacity we here conſider it, does but as it were provide us ſome little conveniences for our paſſage, like ſome accommodations for a cabbin, which outlaſts not the voyage; but religion provides us a vaſt and durable eſtate, or, as the ſcripture ſtiles it, an unſhaken kingdom, when we are arrived at our journey's end. And therefore the benefits accruing from religion, may well be concluded preferable to their competitors, ſince they not only reach to the mind of man, but reach beyond the end of time itſelf; whereas all the variety of inventions, that philoſophy ſo much boaſts of, as whilſt they were in ſeaſon they were deviſed for the ſervice of the body, ſo they make us buſy, and pride ourſelves about things, that within a ſhort time will not (ſo much as upon its ſcore) at all concern us.

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* See examples of this, in my notes about Senſation and Senſible Qualities.

SECTION III.

I EXPECT you ſhould here urge on your friend's behalf, that the ſtudy of phyſicks has one prerogative, (above that of divinity,) which, as it is otherwiſe a great excellency, ſo does much add to the delightfulneſs of it. I mean, the certainty, and clearneſs, and thence reſulting ſatisfactorineſs of our knowledge of phyſical, in compariſon of any we can have of theological matters, whoſe being dark and uncertain, the nature of the things themſelves, and the numerous controverſies of differing ſects about them, ſufficiently manifeſt.

BUT upon this ſubject, divers things are to be conſidered.

FOR firſt, as to the fundamental and neceſſary articles of religion, I do not admit the allegation, but take thoſe articles to be both evident, and capable of a moral demonſtration. And if there be any articles of religion, for which a rational and cogent proof cannot be brought, I ſhall for that very reaſon conclude, that ſuch articles are not abſolutely neceſſary to be believed; ſince it ſeems no way reaſonable to imagine, that God having been pleaſed to ſend not only his prophets and his apoſtles, but his only ſon into the world, to promulgate to mankind the Chriſtian religion, and both to cauſe it to be conſigned to writing, that it may be known, and to alter the courſe of nature by numerous miracles, that it might be believed; it ſeems not reaſonable, I ſay, to imagine, that he ſhould not propoſe thoſe truths, which he in ſo wonderful and ſo ſolemn a manner recommended, with at leaſt ſo much clearneſs, as that ſtudious and well-diſpoſed readers may certainly underſtand ſuch, as are neceſſary for them believe.

2. THOUGH I will not here engage myſelf in a diſquiſition of the ſeveral kinds, or, if you pleaſe, degrees, of demonſtration (which yet is a ſubject, that I judge far more conſiderable than cultivated,) yet I muſt tell you, that as a moral certainty (ſuch as we may attain about the fundamentals of religion) is enough in many caſes for a wiſe man, and even a philoſopher to acquieſce in; ſo that phyſical certainty, which is pretended for the truths demonſtrated by naturaliſts, is, even where it is rightfully claimed, but an inferior kind or degree of certainty, as moral certainty alſo is. For even phyſical demonſtrations can beget but a phyſical certainty, (that is, a certainty upon ſuppoſition, that the principles of phyſick be true,) not a metaphyſical certainty, (wherein it is abſolutely impoſſible, that the thing believed ſhould be other than true.) For inſtance, all the phyſical demonſtrations of the antients about the cauſes of particular phænomena of bodies ſuppoſe, that *ex nihilo nihil fit*; and this may readily be admitted in a phyſical ſenſe, becauſe, according to the courſe of nature, no body can be produced out of nothing, but ſpeaking univerſally it may be falſe, as Chriſtians generally, and even the Carteſian naturaliſts, aſſerting the creation of the world, muſt believe, that, *de facto*, it is. And ſo whereas *Epicurus* does, I remember, prove, that a body once dead cannot be made alive again, by reaſon of the diſſipation and diſperſion of the atoms, it was, when alive, compoſed of; though all men will allow this aſſertion to be phyſically demonſtrable, yet the contrary may be true, if God's omnipotence intervenes, as all the philoſophers, that acknowledge the authority of the New Teſtament, where *Lazarus* and others are recorded to have been raiſed from the dead, muſt believe, that it actually did appear, and even all unprejudiced reaſoners muſt allow it to be poſſible, there being no contradiction implied in the nature of the thing. But now to affirm, that ſuch things, as are indeed contradictories, cannot be both true, or that *factum infectum reddi non poteſt*, are metaphyſical truths, which cannot poſſibly be other than true, and conſequently beget a metaphyſical and abſolute certainty. And your maſter *Carteſius* was ſo ſenſible of a dependance of phyſical demonſtrations upon metaphyſical truths, that he would not allow any certainty not only to them, but even to geometrical demonſtrations, until he had evinced, that there is a God, and that he cannot deceive men, that make uſe of their faculties aright.

TO which I may add, that even in many things, that are looked upon as phyſical demonſtrations, there is really but a moral certainty. For when, for inſtance, *Des Cartes* and other modern philoſophers, take upon them to demonſtrate, that there are divers comets, that are not meteors, becauſe they have a parallax leſſer than that of the moon, and are of ſuch a bigneſs, and ſome of them move in ſuch a line, &c. it is plain, that divers of theſe learned men had never the opportunity to obſerve a comet in their lives, but take theſe circumſtances upon the credit of thoſe aſtronomers, that had ſuch opportunities. And though the inferences, as ſuch, may have a demonſtrable certainty; yet the premiſſes they are drawn from having but an hiſtorical one, the preſumed phyſico-mathematical demonſtration can produce in a wary mind but a moral certainty, and not the greateſt neither of that kind, that is poſſible to be attained; as he will not ſcruple to acknowledge, that knows by experience, how much more difficult it is, than moſt men imagine, to make obſervations about ſuch nice ſubjects, with the exactneſs, that is requiſite for the building of an undoubted theory upon them. And there are I know not how many things in phyſicks, that men preſume they believe upon phyſical and cogent arguments, wherein they really have but a moral aſſurance; which is a truth heeded by ſo few, that I have been invited to take the more particular notice of them in other papers, written purpoſely to ſhow the doubtfulneſs and incompleatneſs of natural philoſophy; of which diſcourſe, ſince you may command a ſight, I ſhall not ſcruple to refer you thither for the reaſons of my affirming here, that the moſt even of the modern virtuoſi are wont to fancy more of clearneſs and certainty in their phyſical theories, than a critical examiner will find. Only, that you may not look upon this as a put off, rather than a reference, I will here touch upon a couple of ſubjects, which men are wont to believe to be, and which indeed ought to be, the moſt thoroughly underſtood; I mean the nature of body in general, and the nature of ſenſation.

AND for the firſt of theſe, ſince we can turn our ſelves no way, but we are every where environed, and inceſſantly touched by corporeal ſubſtances, one would think, that ſo familiar an object, that does ſo aſſiduouſly, and ſo many ways affect our ſenſes, and for the knowledge of which we need not enquire into the diſtinct nature of particular bodies, nor the properties of any one of them, ſhould be very perfectly known unto us. And yet the notion of body in general, or what it is, that makes a thing to be a corporeal ſubſtance, and diſcriminates it from all other things, has been very hotly diſputed of, even among the modern philoſophers, *& adhuc ſub judice lis eſt.* And though your favourite *Des-Cartes*, in making the nature of a body to conſiſt in extenſion every way, has a notion of it, which it is more eaſy to find fault with, than to ſubſtitute a better; yet I fear, it will appear to be attended, not only with this inconvenience, that God cannot, within the compaſs of this world, wherein if any body vaniſh into nothing, the place or ſpace left behind it, muſt have the three dimenſions, and ſo be a true body, annihilate the leaſt particle of matter, at leaſt without, at the ſame inſtant and place, creating as much (which agrees very ill with that neceſſary and continual dependance, which he aſſerts matter itſelf to have on God for its very being;) but with ſuch other inconveniences, that ſome friends of yours, otherwiſe very inclinable to the Carteſian philoſophy, know not how to acquieſce in it: and yet I need not tell you, how fundamental a notion the deviſer of it aſſerts it to be. Neither do I ſee, how this notion of a corporeal ſubſtance will any more than any of the formerly received definitions of it, extricate us out of the difficulties of that no leſs perplexed, than famous controverſy, *de compoſitione continui.* And though ſome ingenious men, who perhaps perceive better than others, how intricate it is, have of late endeavoured to ſhew, that men need not be ſolicitous to determine this controverſy, it not being rightly propoſed by the ſchool-men, that have ſtarted it; and though I perhaps think, that natural philoſophy may be daily advanced without the deciſion of it, becauſe there is a multitude of conſiderable things to be diſcovered and performed in nature, without ſo much as dreaming of this controverſy; yet ſtill, as I would propoſe the queſtion, the difficulties, till removed, will ſpread a thick night over the notion of body in general. For either a corporeal and extended ſubſtance is (either really or mentally) diviſible into parts endowed with extenſion, and each of theſe parts is diviſible alſo into other corporeal parts, leſſer and leſſer, *in infinitum*; or elſe this ſubdiviſion muſt ſtop ſomewhere, (for there is no mean between the two members of the diſtinction;) and in either caſe the opinion pitched upon will be liable to thoſe inconveniences, not to ſay abſurdities, that are rationally urged againſt it by the maintainers of the oppoſite; the objections on both ſides being ſo ſtrong, that ſome of the more candid, even of the modern metaphyſicians, after having tired themſelves and their readers with arguing *Pro* and *Con*, have confeſſed the objections on both ſides to be inſoluble.

BUT though we do not clearly underſtand the nature of body in general; yet ſure we cannot but be perfectly acquainted with what paſſes within ourſelves in reference to the particular bodies we daily ſee, and hear, and ſmell, and taſte, and touch. But alas, though we know but little, ſave by the informations of our ſenſes; yet we know very little of the manner, by which our ſenſes informs us. And to avoid prolixity, I will at preſent ſuppoſe with you, that the ingenious *Des-Cartes* and his followers have given the faireſt account of ſenſation, that is yet extant. Now, according to him, a man's body being but a well organized ſtatue, that, which is truly called ſenſation, is not performed by the organ, but by the mind, which perceives the motion produced in the organ; (for which reaſon he will not allow brutes to have ſenſe properly ſo called;) ſo that if you aſk a Carteſian, how it comes to paſs, that the ſoul of man, which he juſtly aſſerts to be an immaterial ſubſtance, comes to be wrought upon, and that in ſuch various manners, by thoſe external bodies, that are the objects of our ſenſes, he will tell you, that by their impreſſions on the ſenſories, they variouſly move the fibres or threads of the nerves, wherewith thoſe parts are endowed, and by which the motion is propagated to that little kernel in the brain, called by many writers the Conarion, where theſe differing motions being perceived by the there reſiding ſoul, become ſenſations, becauſe of the intimate union, and, as it were, permiſtion (as *Carteſius* himſelf expreſſes it) of the ſoul with the body.

BUT now, Sir, give me leave to take notice, that this union of an incorporeal, with a corporeal ſubſtance, (and that without a medium) is a thing ſo unexampled in nature, and ſo difficult to comprehend, that I ſomewhat queſtion, whether the profound ſecrets of theology, not to ſay the adorable myſtery itſelf of the incarnation, be more abſtruſe than this. For how can I conceive, that a ſubſtance purely immaterial, ſhould be united without a phyſical medium, (for in this caſe there can be none,) with the body, which cannot poſſibly lay hold on it, and which it can pervade, and fly away from at pleaſure, as *Des Cartes* muſt confeſs the ſoul actually does in death. And it is almoſt as difficult to conceive, how any part of the body, without excepting the animal ſpirits, or the Conarion, (for theſe are as truly corporeal, as other parts of the human ſtatue,) can make impreſſions upon a ſubſtance perfectly incorporeal, and which is not immediately affected by the motions of any other parts, beſides the *Genus Nervoſum.* Nor is it a ſmall difficulty to a mere naturaliſt (who, as ſuch, does not in phyſical matters take notice of revelations about angels,) to conceive, how a finite ſpirit can either move, or, which is much the ſame thing, regulate and determine the motion of a body. But that, which I would on this occaſion invite you to conſider, is, that ſuppoſing the ſoul does in the brain perceive the differing motions communicated to the outward ſenſes; yet this, however it may give ſome account of ſenſation in general, will not at all ſhew us a ſatisfactory reaſon of particular and diſtinct ſenſations. For if I demand, why, for inſtance, when I look upon a bell, that is ringing, ſuch a motion or impreſſion in the Conarion produces in the mind that peculiar ſort of perception, ſeeing, and not hearing; and another motion, though coming from the ſame bell, at the ſame time, produces that quite differing ſort of perception, that we call ſound, but not viſion; what can be anſwered, but that it was the good pleaſure of the author of human nature to have it ſo? And if the queſtion be aſked about the differing objects of any one particular ſenſe; as, why the great plenty of unperturbed light, that is reflected from ſnow, milk, &c. does produce a ſenſation of whiteneſs, rather than redneſs or yellowneſs? or why the ſmell of caſtor, or aſſa fœtida, produces in moſt perſons that, which they call a ſtink, rather than a perfume? (eſpecially ſince we know ſome hyſterical women, that think it not only a wholſome, but a pleaſing ſmell.) And if alſo you further aſk, why melody and ſweet things do generally delight us? and diſcords and bitter things do generally diſpleaſe us? Nay, why a little more than enough of ſome objects, that produce pleaſure, will produce pain? (as may be exemplified in a cold hand, as it happens to be held out at a juſt, or at too near a diſtance from the fire:) if, I ſay, theſe, and a thouſand other queſtions of the like kind, be aſked, the anſwer will be but the general one, that is already given, that ſuch is the nature of man. For to ſay, that moderate motions are agreeable to the nature of the ſenſory they are excited in, but violent and diſorderly ones (as jarring ſounds, and ſcorching heat) do put it into too violent a motion for its texture; will by no means ſatisfy. For beſides that this anſwer give no account of the variety of ſenſations of the ſame kind, as of differing colours, taſtes, &c. but reaches only to pleaſure and pain; even as to theſe, it will reach but a very little way; unleſs the givers of it can ſhow, how an immaterial ſubſtance ſhould be more harmed by the briſker motion of a body, than by the more languid.

AND as you and your friend think, you may juſtly ſmile at the Ariſtotelians, for imagining, that they have given a tolerable account of the qualities of bodies, when they have told us, that they ſpring from certain ſubſtantial forms, though when they are aſked particular queſtions about theſe incomprehenſible forms, they do in effect but tell us in general, that they have ſuch and ſuch faculties, or effects, becauſe nature, or the author of nature, endowed them therewith; ſo, I hope, you will give me leave to think, that it may keep us from boaſting of the clearneſs and certainty of our knowledge, about the operations of ſenſible objects, whilſt, as the Ariſtotelians cannot particularly ſhow, how their qualities are produced, ſo we cannot particularly explicate, how they are perceived; the principal thing, that we can ſay, being in ſubſtance this, that our ſenſations depend upon ſuch an union or permiſtion of the ſoul and body, as we can give no example of in all nature, nor no more diſtinct account of, than that it pleaſed God ſo to couple them together. But I beg your pardon for having detained you ſo long upon one ſubject, though perhaps it will not prove time miſ-ſpent, if it have made you take notice, that in ſpight of the clearneſs and certainty, for which your friend ſo much prefers phyſicks before theology, we are yet to ſeek, (I ſay yet, becauſe I know not what time may hereafter diſcover) both for the definition of a corporeal ſubſtance, and a ſatisfactory account of the manner of ſenſation: though without the true notion of a body we cannot underſtand, that object of phyſicks in general, and without knowing the nature of ſenſation, we cannot know that, from whence we derive almoſt all that we know of any body in particular.

IF after all this your friend ſhall ſay, that *Des-Cartes*'s account of body, and other things in phyſicks, being the beſt, that men can give, if they be not ſatisfactory, it muſt be imputed to human nature not to the Carteſian doctrine, I ſhall not ſtay to diſpute, how far the allegation is true; eſpecially ſince, though it be admitted, it will not prejudice my diſcourſe. For, whatſoever the cauſe of the imperfection of our knowledge about phyſical matters be, that there is an imperfection in that knowledge is manifeſt; and that ought to be enough to keep us from being puffed up by ſuch an imperfect knowledge, and from undervaluing upon its account the ſtudy of thoſe myſteries of divinity, which, by reaſon of the nobleneſs and remoteneſs of the objects, may much better than the nature of corporeal things, (which we ſee, and feel, and continually converſe with,) have their obſcurity attributed to the weakneſs of our human underſtandings. And if it be a neceſſary imperfection of human nature, that, whilſt we remain in this mortal condition, the ſoul being confined to the dark priſon of the body, is capable (as even *Ariſtotle* ſomewhere confeſſes) but of a dim knowledge; ſo much the greater value we ought to have for Chriſtian religion, ſince, by its means (and by no other without it) we may attain a condition, wherein, as our nature will otherwiſe be highly bleſſed and advanced; ſo our faculties will be elevated and enlarged, and probably made thereby capable of attaining degrees and kinds of knowledge, to which we are here but ſtrangers. In favour of which I will not urge the received opinion of divines, that before the fall (which yet is a leſs noble condition than is reſerved for us in heaven,) *Adam*'s knowledge was ſuch, that he was able at firſt ſight of them, to give each of the beaſts a name expreſſive of its nature; becauſe that, in ſpight of ſome ſkill (which my curioſity for divinity, not philoſophy, gave me) in the holy tongue, I could never find, that the Hebrew names of animals, mentioned in the beginning of *Geneſis*, argued a (much) clearer inſight into their natures, than did the names of the ſame or ſome other animals in Greek, or other languages: wherefore, as I ſaid, I will not urge *Adam*'s knowledge in paradiſe for that of the ſaints in heaven, though the notice he took of *Eve* at his firſt ſeeing of her, (if it were not conveyed to him by ſecret revelation) may be far more probably urged, than his naming of the beaſts: but I will rather mind you, that the proto-martyr's ſight Acts vii. 56. was ſtrengthened ſo, as to "ſee the heavens "opened, and Jeſus ſtanding at the right hand "of God;" and when the prophet had prayed, 2 Kings vi. 17. that his ſervant's eyes might be opened, he immediately ſaw the mountain, where they were, all covered with chariots and horſemen, which, though mentioned to be of fire, were altogether inviſible to him before. To which, as a higher argument, I ſhall only add a couple of paſſages of ſcripture, which ſeem to allow us even vaſt expectations as to the knowledge our glorified nature may be advanced to. The one is that, which St. *Paul* ſays to the *Corinthians*, 1 Cor. xiii. 12. "for now we ſee through a darkly, "but then face to face: now I know in part, "but then ſhall I know even as alſo I am "known." And the other, where Chriſt's fa- 1 Joh. iii. 2. vourite-diſciple tells believers, "Beloved, now "we are the ſons of God, and it doth not yet "appear what we ſhall be; but we know, "that when he ſhall appear, we ſhall be like "him for we ſhall ſee him as he is."

WHAT has hitherto been diſcourſed, contains the firſt conſideration, that I told you might be propoſed about the certainty aſcribed to the knowledge we are ſaid to have of natural things; but this is not all I have to repreſent to you on this ſubject. For I conſider further, that it is not only by the certainty we have of them, that the knowledge of things is endeared to us, but alſo by the worthineſs of the object, the number of thoſe, that are unacquainted with it, the remoteneſs of it from common apprehenſions, the difficulty of acquiring it without peculiar advantages, the uſefulneſs of it when attained, and other particulars, which it is not here neceſſary to enumerate. I preſume, you doubt not but your friend does very much prefer the knowledge he has of the myſteries of nature (at many of which we have as yet but ingenious conjectures) to the knowledge of one, that underſtands the elements of arithmetick, though he be demonſtratively ſure of the truth of moſt of his rules and operations. And queſtionleſs *Copernicus* received a much higher ſatisfaction in his notion about the ſtability of the ſun, and the motion of the earth, though it were not ſo clear, but that *Tycho*, *Ricciolus*, and other eminent aſtronomers have rejected it, than in the knowledge of divers of the theorems about the ſphere, that have been demonſtrated by *Euclid*, *Theodoſius*, and other geometricans. Our diſcovering, that ſome comets are not, as the ſchools would have them, ſublunary meteors, but celeſtial bodies, and the conjectural theory, which is all, that hitherto we have been able to attain of them, do much better pleaſe both your friend, and you, and me, than the more certain knowledge we have of the time of the riſing and ſetting of the fixed ſtars. And the eſtimates we can make, by the help of parallaxes, of the heights of thoſe comets, and of ſome of the planets, though they are uncertain enough, (as may appear by the vaſtly different diſtances, that are aſſigned to thoſe bodies by eminent aſtronomers;) yet theſe uncertain meaſures of ſuch elevated and celeſtial lights do far more pleaſe us, than that we can by the help of a geometrical quadrant, or ſome ſuch inſtrument, take with far greater certainty the height of a tower or a ſteeple. And ſo a mathematician, when he probably conjectures at the compaſs of the terreſtrial globe, and divides, though but unaccurately, its ſurface, firſt, into proportions of ſea and land, and then into regions of ſuch extents and bounds, and, in a word, ſkilfully plays the coſmographer; thinks himſelf much more nobly and pleaſantly employed, than when, being reduced to play the ſurveyor, he does, with far more certainty, meaſure how many acres a field contains, and ſet out, with what hedges and ditches it is bounded. Now, that the knowledge of God, and of thoſe myſteries of theology, that are ignored by far the greateſt part of mankind, has more ſublime and excellent objects, and is unattained to by much the greateſt part even of learned men, and nevertheleſs is of unvaluable importance, and of no leſs advantage towards the purifying and improving of us here, and the making us perfect and happy hereafter, the paſt diſcourſe has very much miſcarried, if it have not evinced. Wherefore, as to be admitted into the privy-council of ſome great monarch, and thereby be enabled to give a probable gueſs at thoſe thoughts and deſigns of his, that govern kingdoms, and make the fates of nations, is judged preferable to that clearer knowledge, that a notary can have of the dying thoughts and intentions of an ordinary perſon, whoſe will he makes: and as the knowledge of a ſkilful phyſician, whoſe art is yet conjectural, is preferable to that of a cutler, that makes his diſſecting knives, though this man can more certainly perform what he deſigns in his own profeſſion, than the phyſician can in his: and (in fine) as the ſkill of a jeweller, that is converſant about diamonds, rubies, ſapphires, and ſome other ſorts of ſmall ſtones, which being, for the moſt part, brought us out of the *Indies*, we muſt take many things about them upon report, is, becauſe of the nobleneſs of the object, preferred to that of a maſon, that deals in whole quarries of common ſtones, and may be ſure upon his own experience of divers things concerning them, which as to jewels we are allowed to know but upon tradition: ſo a more dim and imperfect knowledge of God, and the myſteries of religion, may be more deſireable, and upon that account more delightful, than a clearer knowledge of thoſe inferior truths, that phyſicks are wont to teach.

I muſt now mention one particular more, which may well be added to thoſe, that peculiarly endear phyſicks to the divine, that is ſtudious of them. For as he contemplates the works of nature not barely for themſelves, but to be the better qualified and excited to admire and praiſe the author of nature; ſo his contemplations are delightful to him, not barely as they afford a pleaſing exerciſe to his reaſon, but as they procure him a more welcome approbation from his conſcience, theſe diſtinct ſatisfactions being not at all inconſiſtent. And queſtionleſs, though *Eſau* did at length miſs of his aim, yet, while he was hunting veniſon for the good old patriarch, that deſired it of him, beſides the pleaſure he was uſed to take in purſuing the dear he chaſed, he took a great one, in conſidering, that now he hunted to pleaſe his father, and in order to obtain of him an ineſtimable bleſſing. So, when *David* employed his ſkilful hand and voice, in praiſing god with vocal and inſtrumental muſick, he received in one act a double ſatisfaction, by exerciſing his ſkill and his devotion; and was no leſs pleaſed with thoſe melodious ſounds, as they were hymns, than as they were ſongs. And this example prompts me to add, that as the devout ſtudent of nature we were ſpeaking of, does intentionally refer the knowledge he ſeeks of the creatures to the glory of the creator; ſo in his diſcoveries, that, which moſt contents him, is, that the wonders he obſerves in nature, heighten that admiration he would fain raiſe to a leſs diſproportion to the wiſdom of God; and furniſh him with a nobler holocauſt for thoſe ſacrifices of praiſe, he is juſtly ambitious to offer up to the deity. And as there is no doubt to be made, but that, when *David* invented (as the ſcripture intimates, that he did) new inſtruments of muſick, there was nothing in that invention, that pleaſed him ſo much, as that they could aſſiſt him to praiſe God the more melodiouſly; ſo the pious ſtudent of nature finds nothing more welcome in the diſcoveries he makes of her wonders, than the riſes and helps they may afford him, the more worthily to celebrate and glorify the divine attributes adumbrated in the creatures. And as a huntſman, or a fowler, if he meets with ſome ſtrange bird or beaſt, or other natural rarity, thinks himſelf much the more fortunate, if it happen to be near the court, where he may have the king to preſent it to, than if he were to keep it but for himſelf, or ſome of his companions; ſo our devout naturaliſt has his diſcoveries of nature's wonders endeared to him, by having the deity to preſent them to, in the veneration they excite in the finder, and which they enable him to engage others to join in.

Gen. xxxvii.

Amos vi. 5.

SECTION IV.

BUT I confeſs, Sir, I much fear, that that, which makes your friend have ſuch detracting thoughts of theology, is a certain ſecret pride, grounded upon a conceit, that the attainments of natural philoſophers are of ſo noble a kind, and argue ſo tranſcendent an excellency of parts in the attainer, that he may juſtly undervalue all other learning, without excepting theology itſelf.

YOU will not, I ſuppoſe, expect, that a perſon, who has written ſo much in the praiſe of phyſicks, and laboured ſo much for a little ſkill in it, ſhould now here endeavour to depreciate that ſo uſeful part of philoſophy. But I do not conceive, that it will be at all injurious to it, to prefer the knowledge of ſupernatural to that of mere natural things, and to think, that the truths, which God indiſcriminately expoſes to the whole race of mankind, and to the bad, as well as to the good, are inferior to thoſe myſterious ones, whoſe diſcloſure he reckons among his peculiar favours, and whoſe contemplation employs the curioſity, and, in ſome points, exacts the wonder of the very angels. That I may therefore repreſs a little the overweaning opinion your friend has of his phyſical attainments, give me leave to repreſent a few particulars conducive to that purpoſe.

AND firſt, as for the nobleneſs of the truths taught by theology and phyſicks, thoſe of the former ſort have manifeſtly the advantage, being not only converſant about far nobler objects, but diſcovering things, that human reaſon of itſelf can by no means reach unto; as has been ſufficiently declared in the foregoing part of this letter.

NEXT, we may conſider, that, whatever may be ſaid to excuſe pride (if there were any) in *Moſchus* the Phœnician, who is affirmed to have firſt invented the atomical hypotheſis, and in *Democritus* and *Leucippus*, (for *Epicurus* ſcarce deſerves to be named with them) that highly advanced that philoſophy; and in Monſieur *Des Cartes*, who either improved, or at leaſt much innovated the corpuſcularian hypotheſis: whatever, I ſay, may be alledged on the behalf of theſe men's pride; I ſee no great reaſon, why it ſhould be allowed in ſuch as your friend; who, though ingenious men, are neither inventors, nor eminent promoters of the philoſophy they would be admired for, but content themſelves to learn what others have taught, or, at leaſt, to make ſome little further application of the principles, that others have eſtabliſhed, and the diſcoveries they have made. And whereas your friend is not a little proud of being able to confute ſeveral errors of *Ariſtotle*, and the ancients, it were not amiſs if he conſidered, that many of the chief truths, that overthrow thoſe errors, were the productions of time and chance, and not of his daring ratiocinations: for there needs no great wit to diſprove thoſe, that maintain the uninhabitableneſs of the torrid zone, or deny the antipodes, ſince navigators have found many parts of the former well peopled, and ſailing round the earth, have found men living in countries diametrically oppoſite to ours. Nor will it warrant a man's pride, that he believes not the moon to be the only planet, that ſhines with a borrowed light, or the galaxy to be a meteor; ſince that now the teleſcope ſhews us, that *Venus* has her full and wain like the moon, and that the milky way is made up of a vaſt multitude of little ſtars, inconſpicuous to the naked eye. And indeed of thoſe other diſcoveries, that overthrow the aſtronomy of the ancients, and much of their philoſophy about the celeſtial bodies, few or none have any cauſe to boaſt, but the excellent *Galileus*, who pretends to have been the inventor of the teleſcope: for that inſtrument once diſcovered, to be able to reject the ſeptenary number of the planets, by the detection of the four Satellites of *Jupiter*, or talk of the mountains and valleys in the moon, requires not much more excellency in your friend, than it would to deſcry in a ſhip, where the naked eye could diſcern but the body of the veſſel (to deſcry, I ſay) by the help of a proſpective glaſs, the maſts, and ſails, and deck, and perceive a boat towed at her ſtern: though indeed, *Galileo* himſelf had no great cauſe to boaſt of the invention, though we are much obliged to him for the improvement of the teleſcope, ſince no leſs a maſter of dioptricks than *Des Cartes* does acknowledge with other writers, that perſpective-glaſſes were not firſt found out by mathematicians or philoſophers, but caſually by one *Metius*, a Dutch ſpectacle-maker. On which occaſion I ſhall mind you, that to hide pride from man, divers others of the chief diſcoveries, that have been made in phyſicks, have been the productions, not of philoſophy, but chance, by which gun-powder, glaſs, and, for aught we know, the verticity of the load-ſtone, (to which we owe both the *Indies*) came to be found in theſe later ages; as (more recently) the milky veſſels of the myſentery, the new receptacle of the chyle, and that other ſort of veſſels, which moſt men call the lymphæ-ducts, were lighted on but by chance, according to the ingenuous confeſſion of the diſcoverers themſelves.

We may further conſider, that thoſe very things, which are juſtly alledged in the praiſe of the corpuſcularian philoſophy it ſelf, ought to leſſen the pride of thoſe, that but make uſe of it. For that hypotheſis, ſuppoſing the whole univerſe (the ſoul of man excepted) to be but a great Automaton, or ſelf-moving engine, wherein all things are performed by the bare motion (or reſt) the ſize, the ſhape, and the ſituation, or texture of the parts of the univerſal matter it conſiſts of; all the phænomena reſult from thoſe few principles, ſingle or combined, (as the ſeveral tunes or chimes, that are rung on five bells,) and theſe fertile principles being already eſtabliſhed by the inventors and promoters of the particularian hypotheſis; all that ſuch perſons, as your friend, are wont farther to do, is but to inveſtigate, or gueſs by what kind of motions the three or four other principles are varied. So that the world being but, as it were, a great piece of clock-work, the naturaliſt, as ſuch, is but a mechanician; however the parts of the engine, he conſiders, be ſome of them much larger, and ſome of them much minuter, than thoſe of clocks or watches. And for an ordinary naturaliſt to deſpiſe thoſe, that ſtudy the myſteries of religion, as much inferior to phyſical truths, is no leſs unreaſonable, than it were for a watch-maker, becauſe he underſtands his own trade, to deſpiſe privy-counſellors, who are acquainted with the ſecrets of monarchs, and myſteries of ſtate; or than it were for a ſhip-carpenter, becauſe he underſtands more of the fabrick of the veſſel, to deſpiſe the admiral, that is acquainted with the ſecret deſigns of the prince, and employed about his moſt important affairs.

That great reſtorer of phyſicks, the illuſtrious *Verulam*, who has traced out a moſt uſeful way to make diſcoveries in the intellectual globe, as he calls it, confeſſes, that his work was (to ſpeak in his own terms) *partus temporis potius quàm ingenii*. And though I am not of his opinion, where he ſays in another place, that his way of philoſophizing does *exæquare ingenia*; yet I am apt to think, that the fertile principles of the mechanical philoſophy being once ſettled, the methods of enquiring and experimenting being found out, and the phyſico-mechanical inſtruments of working on nature's and art's productions being happily invented, the making of ſeveral leſſer improvements, eſpecially by rectifying of ſome almoſt obvious of ſupine errors of the ſchools, by the aſſiſtance of ſuch facilitating helps, may fall to the lot of perſons not endowed with any extraordinary ſagacity, or acuteneſs of parts. And though the inveſtigation, and clear eſtabliſhment of the true principles of philoſophy, and the deviſing the inſtruments of knowledge, be things, that may be allowed to be the proper work of ſublimer wits; yet, if a man be furniſhed with ſuch aſſiſtances, it is not every diſcourſe, that he makes, or thing, which he does by the help of them, that is difficult enough to raiſe him to that illuſtrious rank. And indeed, divers of the vulgar errors, as well as of ſcholars as other men, being mainly grounded upon the mere and often miſtaken authority of *Ariſtotle*, and perhaps ſome frivolous reaſons of his ſcholaſtic interpreters of ſuch precarious and ungrounded things, that to ruin them, does oftentimes require more of boldneſs than ſkill; it may perhaps be ſaid of your friend, in relation to his philoſophical ſucceſſes againſt ſuch vulgar errors, as I am ſpeaking of, what a Roman ſaid of *Alexander*'s triumph over the effeminate Aſiaticks, *Quod nihil aliud quàm bene auſus ſit vana contemnere*. And in ſome caſes it happens, that, when once a grand truth, or a happy way of experimenting has been found, divers phænomena of nature, that had been left unexplained, or were left mis-explained by the ſchools, did, in my opinion, require a far leſs ſtraining exerciſe of the mind to unriddle and explain them, than muſt have been requiſite to diſpel the darkneſs, that attended divers theological truths, that are now cleared up, and perhaps than I have myſelf, now and then, employed in ſome of thoſe attempts, to illuſtrate theological matters, that you may have met in ſome papers, that I have preſumed to write on ſuch ſubjects. And indeed the improvements, that ſuch virtuoſi, as your friend, are wont to make of the fertile theorems and hints, that have been preſented them by the founders, or prime benefactors of true natural philoſophy, are ſo poor and ſlender, and do ſo much oftener proceed from induſtry and chance, than they argue a tranſcendent ſagacity, or a ſublimity of reaſon, that, though ſuch perſons may have cauſe enough to be delighted with what they have done, yet they have none to be proud of it; and their performances may deſerve our thanks, and perhaps ſome of our praiſe, but reach not ſo high, as to merit our admiration; which is to be reſerved for thoſe, that have been either framers, or grand promoters, of true and comprehenſive hypotheſis, or (elſe) the authors of other noble and uſeful diſcoveries, many ways applicable.

It will not perhaps be improper to add on this occaſion, that, as our knowledge is not very deep, not reaching with any certainty to the bottom of things, nor penetrating to their intimate or innermoſt natures; ſo its extent is not very large, not being able to give us, with any clearneſs and particularity, an account of the celeſtial and deeply ſubterraneal parts of the world, of which all the others make but a very ſmall (not to ſay contemptible) portion.

For, as to the very globe, that we inhabit, not to mention, how many plants, animals, and minerals, we are as yet wholly ignorant of, and how many others we are but ſlenderly acquainted with; I conſider, that the objects, about which our experiments and enquiries are converſant, do all belong to the ſuperficial parts of the terreſtrial globe, of which the earth, known to us, ſeems to be but as it were the cruſt or ſcurf. But what the internal part of this globe is made up of, is no leſs diſputable, than of what ſubſtance the remoteſt ſtars we can deſcry, conſiſt: for even among the modern philoſophers ſome think, the internal portion of the earth to be pure and elementary earth, which, ſay they, muſt be found there, or no where. Others imagine it to be fiery, and the receptacle, either of natural or helliſh flames. Others will have the body of the terreſtrial globe to be a great and ſolid magnet. And the Carteſians on the other ſide, (though they all admit ſtore of ſubterraneal loadſtones) teach, that the ſame globe was once a fixed ſtar, and that, though it have ſince degenerated into a planet, yet the internal part of it is ſtill of the ſame nature, that it was before; the change it has received proceeding only from having had its outward parts quite covered over with thick ſpots (like thoſe to be often obſerved about the ſun,) by whoſe condenſation the firm earth we inhabit was formed. And the miſchief is, that each of theſe jarring opinions is almoſt as difficult to be demonſtratively proved falſe as true. For, whereas to the centre of the earth there is, according to the modeſteſt account of our late coſmographers, above three thouſand and five hundred miles; my enquiries among navigators and miners have not yet ſatisfied me, that men's curioſity has actually reached above one mile or two at moſt downwards, (and that not in above three or four places) either into the earth or into the ſea. So that as yet our experience has ſcarce grated any thing deep upon the huſk, if I may ſo ſpeak, without at all reaching the kernel of the terraqueous globe.

And alas! what is this globe of ours, of which itſelf we know ſo little, in compariſon of thoſe vaſt and luminous globes, that we call the fixed ſtars, of which we know much leſs? For, though former aſtronomers have been pleaſed to give us, with a ſeeming accurateneſs, their diſtances and bigneſſes, as if they had had certain ways of meaſuring them; yet later and better mathematicians will, I know, allow me to doubt of what thoſe have delivered. For ſince it is confeſſed, that we can obſerve no parallax in the fixed ſtars, nor perhaps in the higheſt planets, men muſt be yet to ſeek for a method to meaſure the diſtance of thoſe bodies. And not only the Copernicans make it to be I know not how many hundred thouſands of miles greater than the Ptolomeans, and very much greater than even *Tycho*; but *Ricciolus* himſelf, though a great Anti-Copernican, makes the diſtance of the fixed ſtars vaſtly greater, than not only *Tycho*, but, if I miſ-remember not, than ſome of the Copernicans themſelves. Nor do I wonder at theſe ſo great diſcrepances, (though ſome amount, perhaps, to ſome millions of miles) when I conſider, that aſtronomers do not meaſure the diſtance of the fixed ſtars by their inſtruments, but accommodate it to their particular hypotheſis. And by this uncertainty of the remoteneſs of the fixed ſtars you will eaſily gather, that we are not very ſure of their bulk, no not ſo much as in reference to one another; ſince it remains doubtful, whether the differing ſizes, they appear to us to be of, proceed from a real inequality of bulk, or only from an inequality of diſtance, or partly from one of thoſe cauſes, and partly from the other.

But it is not my deſign to take notice of thoſe things, which the famous diſputes among the modern aſtronomers manifeſt to be dubious. For I conſider, that there are divers things relating to the ſtars, which are ſo remote from our knowledge, that the cauſes of them are not ſo much as diſputed of, or enquired into, ſuch as may be among others, why the number of the ſtars is neither greater nor leſſer than it is? why ſo many of thoſe celeſtial lights are ſo placed, as not to be viſible to our naked eyes, nor even when they are helped by ordinary teleſcopes? (which extraordinary good ones have aſſured me of.) Why among the familiarly viſible ſtars, there are ſo many in ſome parts of the ſky, and ſo few in others? why their ſizes are ſo differing, and yet not more differing? why they are not more orderly placed, ſo as to make up conſtellations of regular or handſome figures (of which the triangle is, perhaps, the ſingle example) but ſeem to be ſcattered in the ſky as it were by chance, and have as confuſed configurations, as the drops, that fall upon one's hat in a ſhower of rain? To which divers other queſtions might be added, as about the ſtars, ſo about the interſtellar part of heaven, which ſeveral of the modern Epicureans would have to be empty, ſave where the beams of light (and perhaps ſome other celeſtial effluvia) paſs through it; and the Carteſians, on the contrary, think to be full of an ætherial matter, which ſome, that are otherwiſe favourers of their philoſophy, confeſs they are reduced to take up but as an hypotheſis. So that our knowledge is much ſhort of what many think, not only if it be conſidered intenſively, but extenſively, (as a School-man would expreſs it.) For there being ſo great a diſproportion between the heavens and the earth, that ſome moderns think the earth to be little better than a point in compariſon even of the orb of the ſun; and the Carteſians, with other Copernicans, think the great orb itſelf, (which is equal to what the Ptolomeans called the ſun's orb) to be but a point in reſpect of the firmament; and all our aſtronomers agree, that, at leaſt, the earth is but a phyſical point in compariſon of the ſtarry heaven: Of how little extent muſt our knowledge be, which leaves us ignorant of ſo many things, touching the vaſt bodies, that are above us, and penetrates ſo little a way even into the earth, that is beneath us, that it ſeems confined to but a ſmall ſhare of the ſuperficial part of a phyſical point! of which conſideration the natural reſult will be, that, though what we call our knowledge, may be allowed to paſs for a high gratification to our minds, it ought not to puff them up; and what we know of the ſyſtem, and the nature of things corporeal, is not ſo perfect and ſatisfactory, as to juſtify our deſpiſing the diſcoveries of ſpiritual things.

One of the former parts of this letter may furniſh me with one thing more, to evince thy excellencies and prerogatives of the knowledge of the myſteries of religion; and that one thing is ſuch, that I hope I ſhall need to add nothing more, becauſe it is not poſſible to add any thing higher; and that is, that the pre-eminence above other knowledge adjudged to that of divine truths by a judge above all exception, and above all compariſon, namely, by God himſelf.

This having been but lately ſhown, I ſhall not now repeat it, but rather apply what hath been there evinced, by repreſenting, that if he, who determines in favour of divine truths, were ſuch an one, as was leſs acquainted, than our over-weening naturaliſts, with the ſecrets of their idolized phyſicks; or if he were, though an intelligent, yet (like an angel) a bare contemplator of what we call the works of nature, without having any intereſt in their productions, your friend's not acquieſcing in his eſtimate of things might have, though not a fair excuſe, yet a ſtronger temptation.

But when he, by whoſe direction we prefer the higher truths revealed in the ſcripture, before thoſe, which reaſon alone teaches us, concerning thoſe comparitively mean ſubjects, things corporeal, is the ſame God, that not only underſtands the whole univerſe, and all its parts, far more perfectly, than a watch-maker can underſtand one of his own watches, (in which he can give an account only of the contrivance, and not of the cauſe of the ſpring, nor the nature of the gold, ſteel, and other bodies his watch conſiſts of,) but did make both this great Automaton, the world, and man in it: we have no colour to imagine, that he ſhould either be ignorant of, or injuriouſly diſparage his own workmanſhip, or impoſe upon his favourite creature, man, in directing him what ſort of knowledge he ought moſt to covet and prize. So that ſince it is he, who framed the world, and all thoſe things in it we moſt admire, that would have us prefer the knowledge he has vouchſafed us in his word, before that, which he has allowed us of his works; ſure it is very unreaſonable and unkind, to make the excellencies of the workmanſhip a diſparagement to the author, and the effects of his wiſdom a motive againſt acquieſcing in the deciſions of his judgment; as if, becauſe he is to be admired for his viſible productions, he were not to be believed, when he tells us, that there are diſcoveries, that contain truths more valuable than thoſe, which relate but to the objects, that he has expoſed to all men's eyes.

SECTION V.

I DOUBT, I ſhould be guilty of a moſt important omiſſion, if I ſhould here forget to conſider one thing, which I fear has a main ſtroke in the partiality your friend expreſſeth in his preference of phyſicks to theology; and that is, that he ſuppoſes he ſhall, by the former, acquire a fame, both more certain and more durable, than can be hoped for from the latter.

And I acknowledge, not only with readineſs, but with ſomewhat of gratulation of the felicity of this age, that there is ſcarce any ſort of knowledge more in requeſt, than that which natural philoſophy pretends to teach; and that among the awakened and inquiſitive part of mankind, as much reputation and eſteem may be gained by an inſight into the ſecrets of nature, as by being entruſted with thoſe of princes, or dignified with the ſplendideſt marks of their favour.

But though I readily confeſs thus much, and though perhaps I may be thought to have had, I know not by what fate, as great a ſhare of that perfumed ſmoak, applauſe, as (at leaſt) ſome of thoſe, which among the writers, that are now alive, your friend ſeems moſt to envy for it; yet I ſhall not ſcruple to tell you, partly from obſervation of what has happened to others, and partly too upon ſome little experience of my own, that neither is it ſo eaſy, as your friend ſeems to believe it, to get by the ſtudy of nature a ſure and laſting reputation, neither ought the expectation of it, in reaſon, make men undervalue the ſtudy of divinity. Nor would it here avail to object (by way of prevention) that the difficulties and impediments of acquiring and ſecuring reputation lie as well in the way of divines as philoſophers, ſince this objection has been already conſidered at the beginning of this ſecond part of our preſent tract. Beſides that the progreſs of our diſcourſe will ſhew, that the naturaliſt, aſpiring to fame, is liable to ſome inconveniences, which are either not at all, or not near equally incident to the divine. Wherefore, without ſtaying to take any further notice of this preventive allegation, I ſhall proceed to make good the firſt part of the aſſertion, that preceded it; which that I may the more fully do, give me leave (after having premiſed, that a man muſt either be a writer, or forbear to print what he knows;) to propoſe to you the following conſiderations.

AND firſt, if your *Phyſeophilus* ſhould think to ſecure a great reputation, by forbearing to couch any of his thoughts or experiments in writing, he may thereby find himſelf not a little miſtaken. For if once he have gained a repute (upon what account ſoever) of knowing ſome things, that may be uſeful to others, or of which ſtudious men are wont to be very deſirous, he will not avoid the viſits and queſtions of the curious. Or, if he ſhould affect a ſolitude, and be content to hide himſelf, that he may hide the things he knows; yet he will not eſcape the ſolicitations, that will be made him by letters. And if theſe ways of tempting him to diſcloſe himſelf, prevail not at all with him to do ſo, he will provoke the perſons, that have employed them; who finding themſelves diſobliged, by being defeated of their deſires, if not alſo their expectations, will for the moſt part endeavour to revenge themſelves on him, by giving him the character of an uncourteous and ill-natured perſon; and will endeavour, perhaps, ſucceſsfully enough, to decry his parts, by ſuggeſting, that his affected concealments proceed but from a conſcientiouſneſs, that the things he is preſumed to poſſeſs, are but ſuch, as, if they ſhould begin to be known, would ceaſe to be valued.

YOU will ſay (perchance,) that ſo much reſervedneſs is a fault: nor ſhall I diſpute it with you, whether it be or not; but if he be open and communicative in diſcourſe to thoſe ſtrangers, that come to pump him, ſuch is the diſingenuous temper of too too many, that he will be in great danger of having his notions or experiments arrogated by thoſe, to whom he imparts them, or at leaſt, by others, to whom thoſe may (though perchance deſignleſsly) happen to diſcourſe of them. And then, if either *Phyſeophilus*, or any of his friends, that know him to be author of what is thus uſurped, ſhould mention him as ſuch, the uſurpers and their friends would preſently become his enemies; and, to ſecure their own reputation, will be ſolicitous to leſſen and blemiſh his. And if you ſhould now tell me, that your friend might here take a middle way, as that, which in moſt moſt caſes is thought to be the beſt, by diſcourſing at ſuch a rate of his diſcoveries, as may ſomewhat gratify thoſe, that have a curioſity to learn them, and yet not ſpeak ſo clearly, as diveſt himſelf of his propriety in them; I ſhould reply, that neither is this expedient a ſure one, nor free from inconveniencies. For moſt men are ſo ſelf-opinionated, that they will eaſily believe themſelves maſters of things, if they do but half underſtand them. And however, though the perſons, to whom the diſcourſe was immediately made, ſhould not have too great an opinion of themſelves, no more than too great a ſagacity; yet they may eaſily, by repeating what they heard and obſerved, give ſome more piercing wit a hint ſufficient to enable him to make out the whole notion, or the diſcovery, which he will then without ſcruple, and without almoſt any poſſibility of being diſproved, aſſume for his own. But if it happen, (as it often will in extemporaneous diſcourſe) that a philoſopher be not rightly underſtood, either becauſe he has not the leiſure, no more than a deſign, to explain himſelf fully, or becauſe the perſons he converſes with, bring not a competent capacity and attention; he then runs a greater danger than before. For the vanity moſt men take in being known to have converſed with eminent philoſophers, makes them very forward to repeat what they heard ſuch a famous wit ſay; and oftentimes being ſecure of not being contradicted, ignorantly to miſ-recite it, or wittingly to wreſt it in favour of the opinion they would countenance by it. So that, whereas by the formerly mentioned frankneſs of diſcourſe, he is only in danger to have the truths he diſcovered arrogated by others, this reſervedneſs expoſes him to have opinions and errors, that he never dreamed of, fathered on him. And when a man's opinions, or diſcoveries, come once to be publikly diſcourſed of, without being propoſed by himſelf, or ſome friend well inſtructed by him, he knows not what errors or extravagancies may be imputed to him (and that without a moral poſſibility left to moſt men to diſcern them,) by the miſtake of the weak, or the diſingenuity of the partial, or the artifices of the malicious. And even the greatneſs of a man's reputation does ſometimes give ſuch countenance to vain reports and ſurmiſes, as by degrees to ſhake, if not ruin it. As we ſee, that Frier *Bacon*, and *Trithemius*, and *Paracelſus*, who, for their times, were knowing, as well as famous men, had ſuch feats aſcribed to them, as by appearing fabulous to moſt of the judicious, have tempted many to think, that all the great things, that were ſaid of them, were ſo too.

THESE are ſome of the inconveniencies, that a naturaliſt may be liable to, if he forbear the communicating of his thoughts and diſcoveries himſelf: but if *Phyſeophilus* ſhould, to ſhun theſe, aſpire to fame by the uſual way of writing books, he may indeed avoid theſe, but perhaps, not without running into other inconveniencies and hazards, very little inferior to them.

FIRST then, we may conſider, that whether a man writes in a ſyſtematical way, as they have done, who have publiſhed entire bodies of natural philoſophy, or methodical treatiſes of ſome conſiderable part of it; or whether he write in a more looſe and unconfined way, of any particular ſubject, that belongs to phyſicks; which ſoever, I ſay, of theſe two ways of writing books he ſhall make choice of, he will find it liable to inconvenience enough.

FOR if he write ſyſtematically, firſt, he will be obliged, that he may leave nothing neceſſary undelivered, to ſay divers things, that have been ſaid (perhaps many times) by others already, which cannot but be unpleaſant, not only to the reader, but (if he be ingenious) to the writer. Next, there are ſo many things in nature, whereof we know little or nothing, and ſo many more, of which we do not know enough, that our ſyſtematical writer, though we ſhould grant him to be very learned, muſt needs, either leave divers things, that belong to his theme, untreated of, or diſcourſe of them ſlightly, and oftentimes (in likelihood) erroneouſly. So that in this kind of books there is always much ſaid, that the reader did know, and commonly not a little, that the writer does not know. And to this, I muſt add, in the third place, that natural philoſophy, being ſo vaſt and pregnant a ſubject, that (eſpecially in ſo inquiſitive an age as this) almoſt every day diſcovers ſome new thing or other about it, it is ſcarce poſſible for a method, that is adapted but to what is already known, to continue long the moſt proper; as the ſame clothes will not long fit a child, whoſe age will make him quickly out-grow them. And therefore ſucceeding writers will have a fair pretence to compile new ſyſtems, that may be more adequate to philoſophy, improved ſince the publication of the former. And though there were little of new to be added, and it were more eaſy to alter, than to mend the method of our ſuppoſed author; yet novelty itſelf is a thing ſo pleaſing and inviting to the generality of men, that it often recommends things, that have nothing elſe to recommend them; and we may apply to a great many other things, what, I remember, a famous courtier of my acquaintance uſed to ſay of miſtreſſes, that another was preferable to a better, (the better being but the ſame.)

But now, if, declining the ſyſtematical way, one ſhould chooſe the other of writing looſe tracts and diſcourſes, he may indeed avoid ſome of the lately mentioned inconveniencies, but will ſcarce avoid the being plundered by ſyſtematical writers: for theſe will be apt to cull out thoſe things, that they like beſt, and inſert them in their methodical books, (perhaps much curtailed, or otherwiſe injured in the repeating,) and will place them, not as their own author did, where they may beſt confirm or adorn his diſcourſe, and be illuſtrated or upheld by it; but where it may beſt ſerve the turn of the compiler: and theſe methodical books promiſe ſo much more compendious a way, than others, to the attainment of the ſciences they treat of, that though really for the moſt part they prove greater helps to the memory, than the underſtanding; yet moſt readers being, for want of judgment, or of patience, of another mind, they are willing to take it for granted, that in former writers, if there have been any thing conſiderable, it has been all carefully extracted, as well as orderly digeſted by the later compilers: and though I take this to be a very erroneous and prejudicial conceit, yet it obtains ſo much, that as goldſmiths, that only give ſhape and luſtre to gold, are far more eſteemed, and in a better condition, than miners, who find the ore in the bowels of the earth, and with great pains and induſtry dig it up, and refine it into metal; ſo thoſe, that with great ſtudy and toil, ſucceſsfully penetrate into the hidden receſſes of nature, and diſcover latent truths, are uſually leſs regarded, or taken notice of, by the generality of men, than thoſe, who by plauſible methods, and a neat ſtile, reduce the truths, that others have found out, into ſyſtems of a taking order, and a convenient bulk.

I conſider in the ſecond place, that as the method of the books one writes, ſo the bulk of them may prove prejudicial to the naturaliſt, that aſpires to fame: for if he write large books, it is odds but that he will write in them many things unaccurate, if not impertinent, or that he will be obliged to repeat many things, that others have ſaid before; and if he write but ſmall tracts, as is the cuſtom of the judicioueſt authors, who have no mind to publiſh but what is new and conſiderable, as their excellency will make them to be the ſooner diſperſed, ſo the ſmallneſs of the bulk will endanger them to be quickly loſt, as experience ſhews us of divers excellent little tracts, which, though publiſhed not many years ago, are already out of print, (as they ſpeak) and not to be met with, ſave by chance, in ſtationers ſhops. So that theſe writings (which deſerve a better fate) come, after a while, either to be loſt (which is the caſe of divers,) or to have their memory preſerved only in the larger volume of ſome compiler, whoſe induſtry is only preferable to his judgment; it being obſervable, that (by I know not what unlucky fate) very few (for I do not ſay none) that addict themſelves to make collections out of others, have the judgment to cull out the choiceſt things in them; and the ſmall tracts, we are ſpeaking of, being preſerved but in ſuch a quoter or abridger, will run a very great danger of being conveyed to poſterity but under ſuch a repreſentation, as it pleaſes the compiler.

And this (that I may proceed to my third conſideration) may make the naturaliſt's fame very uncertain, not only becauſe of the want of judgment, that (as I newly ſaid) is too often obſervable in compilers, whereby they frequently leave far better things than they take, but for the want of ſkill to underſtand the author they cite and epitomize, or candor to do him right. For ſometimes men's phyſical opinions, and ſeveral paſſages of their writings, are ſo miſrepreſented by miſtake or deſign, eſpecially if thoſe, that recite their opinions be not of them, that men are made to teach or deliver things quite differing from their ſenſe, and perhaps quite contrary to it; of which I myſelf have had ſome unwelcome experience, a learned writer pretending, I know not how often, that I aſſerted an opinion, about which I did expreſsly ἐπέχειν. And another noted writer having (not out of deſign, but unacquaintedneſs with mechanicks, and the ſubject I writ of,) given me commendations for having, by a new experiment, proved a thing, the quite contrary whereof I intended thereby to evince, and am not alone miſtaken, if I did not do it. Other naturallſts I have met with, whoſe writings compilers have traduced out of hatred to their perſons, or their religion; as if truth could in nothing be a friend to one, that is the traducer's traducer's enemy; or as if a man, that falls into an error in religion, could not light upon a good notion in philoſophy, in ſpite of all the truths we owe to *Ariſtotle*, *Epicurus*, and the other heathen philoſophers. Nay, ſome there are, that will ſet themſelves to decry a man's writings, not becauſe they are directly his enemies, but becauſe he is eſteemed by theirs; as you may remember an inſtance in a ſervant of yours, who had divers things written againſt him upon this very account. Nor is it only by the citations of profeſſed adverſaries, or opponents, that a worthy writer's reputation may be prejudiced, ſince it is not unfrequently ſo by thoſe, that mention him with an encomium, and ſeem diſpoſed to honour him. For I have obſerved it to be the trick of certain writers, to name an author with much compliment, only for ſome one or few of the leaſt conſiderable things they borrow of him; by which artifice they endeavour to conceal their being plagiaries of more and better; which yet is more excuſable than the practice of ſome, who proceed to that pitch of diſingenuity, that they will rail at an author, to whom indeed they owe too much, that they may not be thought to be beholden to him.

BUT (4.) I muſt add, that beſides theſe dangers, that a naturaliſt's reputation with poſterity may run through the ignorance or perverſeneſs of men, it is liable to divers other hazards, from the very nature both of men, of opinions, and of things.

FOR, as men's geniuſſes and inclinations are naturally various in reference to ſtudies, one man paſſionately affecting one ſort of them, and another being fond of quite differing ones; ſo thoſe inclinations are oftentimes variouſly and generally determined by external and accidental cauſes. As when ſome great monarch happens to be a great patron, or a deſpiſer, and perhaps adverſary of this or that kind of learning; and when ſome one man has gained much applauſe for this or that kind of ſtudy; imitation or emulation oftentimes makes many others addict themſelves to it. Thus though *Rome*, under the conſuls, was inconſiderable for learning, yet the reputation of *Cicero*, and favour of *Auguſtus*, brought learning into requeſt there; where the ſmall countenance it met with amongſt moſt of the ſucceeding emperors, kept it far inferior to what it had been among the Greeks about *Alexander*'s age. And the age of the ſame *Auguſtus* was ennobled with ſtore of poets, not only by the countenance, which he and *Mæcenas* afforded them, but probably alſo by the examples they gave to, and the emulation they excited in one another. And after the decay of the Roman empire, in the fourth century, natural philoſophy and the mathematicks being very little valued, and leſs underſtood, by reaſon that men's ſtudies were by the genius of thoſe ages applied to other ſubjects, every hundred years ſcarce produced one improver, (not to ſay one eminent cultivator) either of mathematicks or of phyſicks: by which you may ſee, how little certainty there is, that, becauſe a man is ſkilled in natural philoſophy, and that ſcience is now in requeſt, his reputation ſhall be as great as now, when perhaps the ſcience itſelf will be grown out of repute.

BUT beſides the contingencies, that may happen to a naturaliſt's fame upon this account, that the ſcience he cultivates is, as well as others, ſubject to wanes and eclipſes in the general eſteem of men; there is another uncertainty ariſing from the viciſſitudes, that are to be met with in the eſtimates men make of differing hypotheſes, ſects, and ways of philoſophizing about the ſame ſcience, and particularly about natural philoſophy. For during thoſe learned times, when phyſicks firſt and moſt flouriſhed among the Grecians, *Democritus*, *Leucippus*, *Epicurus*, *Anaxagoras*, *Plato*, and almoſt all the naturaliſts, that preceded *Ariſtotle*, were Corpuſcularians, endeavouring, though not all by the ſame way, to give an account of the phænomena of nature, and even of qualities themſelves, by the bigneſs, ſhape, motion, &c. of corpuſcles, or the minuteſt active parts of matter: whereas *Ariſtotle*, having attempted to deduce the phænomena from the four firſt qualities, the four elements, and ſome few other barren hypotheſes, aſcribing what could not be explicated by them, (and conſequently far the greateſt part of nature's phænomena) to ſubſtantial forms and occult qualities; (principles, that are readily named, but ſcarce ſo much as pretended to be underſtood,) and having upon theſe ſlight and narrow principles reduced phyſicks into a kind of ſyſtem, which the judicious modeſty of the Corpuſcularians had made them backward to do; the reputation, that his great pupil *Alexander*, as well as his learning gave him; the eaſineſs of the way he propoſed to the attainment of natural philoſophy; the good luck his writings had to ſurvive thoſe of *Democritus*, and almoſt all the reſt of the Corpuſcularians, when *Charles* the Great began to eſtabliſh learning in *Europe*: theſe, I ſay, and ſome other lucky accidents, that concurred, did for about ſeven or eight hundred years together, make the Corpuſcularian philoſophy not only be juſtled, but even exploded out of the ſchools by the Peripatetick; which in our times is, by very many, upon the revival of the Corpuſcularian philoſophy, rejected, and, by more than a few, derided as precarious, unintelligible, and uſeleſs. And to give an inſtance in a particular thing, (which, though formerly named, deſerves to be again mentioned to our preſent purpoſe,) *Ariſtotle* himſelf ſomewhere confeſſes, (not to ſay brags) that the Greek philoſophers, his predeceſſors, did unanimouſly teach, that the world was (I ſay not created, but) made, and yet he, almoſt by his ſingle authority, and the ſubtile arguments (as ſome have been pleaſed to think them,) that he employed, (though divers of them were borrowed of *Ocellus Lucanus*,) was able for many ages to introduce into the ſchools of philoſophers that irreligious and ill-grounded opinion of the eternity of the world, which afterwards the Chriſtian doctrine made men begin to queſtion, and which now, both that and right reaſon have perſuaded moſt men to reject.

And this invites me to conſider farther, that the preſent ſucceſs of the opinions, that your *Phyſiophilus* befriends, ought not to make him ſo ſure, as he thinks he is, that the ſame opinions will be always in the ſame, or a greater vogue, and have the ſame advantages, in point of general eſteem, that they now have, over their corrivals. For, opinions ſeem to have their fatal ſeaſons and viciſſitudes, as well as other things; as may appear, not only by the examples of it newly given, but alſo by the hypotheſis of the earth's motion, which having been in great requeſt before *Pythagoras*, (who yet is commonly thought the inventor of it,) had its reputation much encreaſed by the ſuffrage of the famous ſect of the Pythagoreans, (whom *Ariſtotle* himſelf takes notice of as the patrons of that opinion;) and yet afterwards for near 2000 years it was laughed at, as not only falſe, but ridiculous. After all which time, this ſo long antiquated opinion being revived by *Copernicus*, has in a little time made ſo great a progreſs among the modern aſtronomers and philoſophers, that if it go on to prevail at the ſame rate, the motion of the earth will be acknowledged by all its mathematical inhabitants. But though it be often the fate of an oppreſſed truth, to have at length a reſurrection, yet it is not always its peculiar privilege; for obſolete errors are ſometimes revived, as well as diſcredited truths: ſo that the general diſrepute of an opinion in one age will not give us an abſolute ſecurity, that it will not be in as general requeſt in another, in which it may perhaps, not only revive, but reign.

Nor is it only in the credit of men's opinions about philoſophical matters, that we may obſerve an inconſtancy and viciſſitude, but in the very way and method of philoſophizing; for *Democritus*, *Plato*, *Pythagoras*, and others, who were of the more ſincere and ingenious cultivators of phyſicks among the Greeks, exerciſed themſelves chiefly either in making particular experiments and obſervations, as *Democritus* did in his manifold diſſections of animals; or elſe applied the mathematicks to the explicating of a particular phænomenon of nature, as may appear (not to mention what *Hero* teaches in his Pneumaticks,) by the accounts, *Democritus*, *Plato*, and others, give of fire and other elements, from the figure and motion of the corpuſcles they conſiſt of. And although this way of philoſophizing were ſo much in requeſt before *Ariſtotle*, that (albeit he unluckily brought in another, yet) there are manifeſt and conſiderable footſteps of it to be met with in ſome of his writings, (and particularly in his books of animals, and his mechanical queſtions;) yet the ſcholaſtick followers of *Ariſtotle* did, for many ages, neglect the way of philoſophizing of the antients, and (to the great prejudice of learning) introduced every where, inſtead of it, a quite contrary way of writing. For, not only they laid aſide the mathematicks, (of which they were for the moſt part very ignorant,) but inſtead of giving us intelligible and explicite (if not accurate) accounts of particular ſubjects, grounded upon a diſtinct and heedful conſideration of them, they contented themſelves with hotly diſputing, in general, certain unneceſſary, or at leaſt unimportant queſtions about the objects of phyſicks, about *Materia Prima*, ſubſtantial forms, privation, place, generation, corruption, and other ſuch general things, with which when they had quite tired themſelves and their readers, they uſually remained utter ſtrangers to the particular productions of that nature, about which they had ſo much wrangled, and were not able to give a man ſo much true and uſeful information about particular bodies, as even the meaneſt mechanicks, ſuch as mine-diggers, butchers, ſmiths, and even dairy-maids, could do. Which made their philoſophy appear ſo imperfect and uſeleſs, not only to the generality of men, but to the more elevated and philoſophical wits, that our great *Verulam* attempted with much ſkill and induſtry, (and not without ſome indignation) to reſtore the more modeſt and uſeful way practiſed by the antients, of enquiring into particular bodies, without haſtening to make ſyſtems, into the requeſt it formerly had; wherein the admirable induſtry of two of our *London* phyſicians, *Gilbert* and *Harvey*, has not a little aſſiſted him. And I need not tell you, that ſince him, *Des Cartes*, *Gaſſendus*, and others, having taken in the application of geometrical theorems, for the explication of phyſical problems; he, and they, and other reſtorers of natural philoſophy, have brought the experimental and mathematical way of enquiring into nature, into at leaſt as high and growing an eſteem, as ever it poſſeſſed when it was moſt in vogue among the naturaliſts, that preceded *Ariſtotle*.

To the conſiderations I have hitherto deduced, which (perhaps) might alone ſuffice for my purpoſe, I ſhall yet ſubjoin one, that I take to be of greater weight than any of them, for the manifeſting, how difficult it is to be ſure, that the phyſical opinions, which at preſent procure a champion or promoter of them veneration, ſhall be ſtill in requeſt. For beſides that inconſtant fate of applauded opinions, which may be imputed to the inconſtancy of men, there is a greater danger, that threatens the aſpirer's reputation from the very nature of things: for the moſt general principles of all, viz. the figure, bigneſs, motion, and other mechanical affections of the ſmall parts of matter, being (as your friend believes) ſufficiently and clearly eſtabliſhed already; he muſt expect to raiſe his reputation from ſubordinate hypotheſes and theories; and in theſe I ſhall not ſcruple to ſay, that it is extremely difficult, even for thoſe, that are more exerciſed than he in framing them, and in making of experiments to have ſo reaching and attentive a proſpect of all things fit to be known, as not to be liable to have their doctrine made doubtful, or diſproved by ſomething, that he did not diſcover, or that aftertimes may. This, I doubt not, but you would eaſily be prevailed with to allow, if I had leiſure and conveniency to tranſmit to you my ſceptical naturaliſt. And without having recourſe to that tract, it may poſſibly ſuffice, that we conſider, that one of the conditions of a good [*] hypotheſis is, that it fairly comport not only with all other truths, but with all other phænomena of nature, as well as thoſe it is framed to explicate. For this being granted, (which cannot be denied,) he, that eſtabliſhes a theory, which he expects ſhall be acquieſced in by all ſucceeding times, and make him famous in them, muſt not only have a care, that none of the phænomena of nature, that are already taken notice of, do contradict his hypotheſis at the preſent, but that no phænomena, that may be hereafter diſcovered, ſhall do it for the future. And I very much queſtion, whether *Phyſeophilus* do know, or, upon no greater a number and variety of experiments than moſt men build upon, can know, how incompleat the hiſtory of nature we yet have, is, and how difficult it is to build an accurate hypotheſis upon an incompleat hiſtory of the phænomena it is to be fitted to; eſpecially conſidering, that (as I was ſaying) many things may be diſcovered in after-times by induſtry or chance, which are not now ſo much as dreamed of, and which may yet overthrow doctrines ſpeciouſly enough accommodated to the obſervations, that have been hitherto made.

THOSE ancient philoſophers, that thought the torrid zone to be uninhabitable, did not eſtabliſh their opinion upon wild reaſonings; and as it continued uncontrouled for many ages, ſo perhaps it would have always done, if the diſcoveries made by modern navigations had not manifeſted it to be erroneous. The ſolidity of the celeſtial orbs was, for divers centuries above 1000 years, the general opinion of aſtronomers and philoſophers; and yet in the laſt age, and in ours, the free trajection, that has been obſerved in the motion of ſome comets, from one of the ſuppoſed orbs to another, and the intricate motions in the planet *Mars*, (obſerved by *Kepler* and others, to be ſometimes nearer, as well as ſometimes remoter from the earth than is the ſun;) theſe, I ſay, and other phenomena undiſcovered by the ancients, have made even *Tycho*, as well as moſt of the recent aſtromoners, exchange the too long received opinion of ſolid orbs for the more warrantable belief of a fluid æther. And though the celeſtial part of the world, by reaſon of its remoteneſs from us, be the moſt unlikely of any other to afford us the means of over-throwing old theories by new diſcoveries; yet even in that we may take notice of divers inſtances to our preſent purpoſe, though I ſhall here name but this one, viz. That, after the Ptolemaick number and order of the planets had paſt uncontradicted for very many ages; and even the Tychonians and Copernicans, (however they did, by their differing hypotheſes, diſſent from the Ptolemaick ſyſtem (as to the order) did (yet) acquieſce in it as to the number of the planets; by the happy diſcoveries, made by *Galilæo* of the Satellites of *Jupiter*, and by the excellent *Hugenius*, of the new planet about *Saturn*, (which I think I had the luck to be the firſt, that obſerved and ſhewed diſbelievers of it in *England*) the aſtronomers of all perſuaſions are brought to add to the old ſeptenary number of the planets, and take in five others, that their predeceſſors did not dream of. That the chyle prepared in the ſtomach paſſed through the meſaraick veins to the liver, and ſo to the heart, was for many ages the unanimous opinion, not only of phyſicians, but anatomiſts, whoſe numerous diſſections did not tempt them to queſtion it; and yet, ſince the caſual, though lucky, diſcoveries made of the milky veſſels in the thorax by the dextrous *Pecquet*, thoſe, that have had with you and me the curioſity to make the requiſite experiments, are generally convinced, that, at leaſt, a good part of the chyle goes from the ſtomach to the heart, without paſſing through the meſaraick veins, or coming at all to the liver.

IT were eaſy to multiply inſtances of this kind, but I rather chuſe to add, that it is not only about the qualities, and other attributes of things, but about their cauſes alſo, that new, and oftentimes accidental diſcoveries may deſtroy the credit of long and generally approved opinions. That quick-lime exceedingly heats the water, that is poured on to quench it, on the account of *Antiperiſtaſis*, has been very long and univerſally received by the ſchool-philoſophers, where it is the grand and uſual argument, urged to eſtabliſh *Antiperiſtaſis* [†]; and yet I preſume you have taken notice, that this proof is made wholly ineffectual in the judgment of many of the virtuoſi, by ſome contrary experiments of mine, and particularly that of exciting in quick-lime full as great an efferveſcence by the affuſion of hot water inſtead of cold. So it has been generally believed, that in the congelation of water, that liquor is condenſed into a narrower room; whereas our late experiments [‖] have ſatisfied moſt of the curious, that ice is water expanded, or, if you pleaſe, that ice takes up more room than the water did, whilſt it remained unfrozen. And whereas the notion of nature's abhorrence of a vacuum has not only, ever ſince *Ariſtotle*'s time, made a great noiſe in the ſchools, but ſeems to be confirmable by a multitude of phænomena; the experiments of *Torricellius* and ſome of [‡] ours, evidencing, that the air has a great weight and a ſtrong ſpring, have, I think, perſuaded almoſt all, that have impartially conſidered them, that, whether there be or be not ſuch a thing, as they call *fuga vacui*, yet ſuction, and the aſcenſion of water in pumps, and thoſe other phænomena, that are generally aſcribed to it, may be very well explicated without it, and are, indeed, cauſed by the weight of the atmoſphere, and the elaſtical power of the air.

AND this puts me in mind to take notice, that even practical inventions, where one would think the matter of fact to be evident, may, by

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[*] See the requiſites of a good hypotheſis.

[†] See this ſubject handled at large in an appendix to the author's *Examen* of *Antiperiſtaſis*.

[‖] In the hiſtory of cold.

[‡] Now publiſhed in the book of new phyſico-mechanical experiments.

undreamed-of diſcoveries be brought to loſe the general reputation they had for compleatneſs in their kind. For to endear the invention of ſucking pumps, and of ſyphons, it has been generally preſumed, that by means of either of theſe, water and any other liquor may, *ob fugam vacui*, be raiſed to what height one pleaſes; and accordingly ways have been propoſed by famous authors, to convey water from one ſide of an high mountain to the other: whereas, firſt, the unexpected diſappointments, that were met with by ſome pump-makers, and afterwards experiments purpoſely made, ſufficiently evince, that neither a pump nor a ſiphon will raiſe water to above 35 foot, or thereabouts, nor quick-ſilver to ſo many inches.

And as to the invention of weather-glaſſes, which has been ſo much, and juſtly applauded and uſed, as it has been generally received for the trueſt ſtandard of the heat and cold of the weather; ſo it ſeems to be liable to no ſuſpicion of deceiving us: for not only it is evident, that in winter, when the air is very cold, the water riſes much higher than in ſummer, and other ſeaſons, when it it not ſo; but if you but apply your warm hand to the bubble at the top, the water will be viſibly depreſſed by the rarified air, which upon the removal of the hand returning to its former coldneſs, the water will forthwith as manifeſtly aſcend again. [*] And yet by finding, that, as the atmoſphere has a conſiderable weight, ſo this weight is not always the ſame, but varies much, and that, as far as I can yet diſcover, uncertainly enough; I have had the luck to ſatisfy many of the curious, that theſe open thermometers are not to be ſafely relied on, ſince in them the liquor is made to riſe and fall, not only, as men have hithertherto ſuppoſed, by the cold and heat of the ambient air, but (as I have ſhewn by divers new experiments) according to the varying gravity of the atmoſphere; which variation has not only a ſenſible, but a very conſiderable influence upon the weather-glaſs. To theſe inſtances I ſhall annex only one more, from which we may learn, that notwithſtanding a very heedful ſurvey of all, that at preſent a man can take notice of, or well ſuſpect, that he ought to take into his conſideration, the caſe may be ſuch, that having deviſed an inſtrument, he may uſe it many years with good ſucceſs; and yet, unleſs he were able to live very many more, he ſhall not be ſure to outlive the danger of finding the ſame inſtrument (though to ſenſe as well conditioned as ever) fallacious: as he, that firſt applied a magnetick needle to the finding of the meridian line, might very probably conclude, that his needle pointing directly N. and S. or declining from it juſt two or three, or ſome other determinate number of degrees, he had diſcovered a certain and ready way, without the help of ſun or ſtars, or aſtronomical inſtruments, to deſcribe a meridian line, and if he lived but an ordinary number of years after his obſervation, he might probably have found his inſtrument not deceitful; which yet it may now be, the magnetick needle, not only declining in many places from the true points of N. and S. but (as later diſcoveries inform us) varying in tract of time its declination in the ſelf ſame place.

The conſiderations hither to propoſed might eaſily enough be increaſed by more of the ſame tendency, eſpecially if I thought fit to borrow from a diſcourſe (of mine) purpoſely written about the partiality and uncertainty of fame; but inſtead of adding to their number, I ſhould think myſelf obliged to excuſe my having already mentioned ſo many, and inſiſted ſo much upon them, if I did not vehemently ſuſpect, that in your *Phyſeophilus*, (as well as in many other modern naturaliſts) ſcarce any thing does more contribute to an undervaluation of the ſtudy of divinity, than, that being eagerly ambitious of a certain, as well as a poſthume fame, he is confident, that phyſiology will help to it; and therefore, the deſign of his diſcourſe made me think it expedient to ſpend ſome time to manifeſt, "that it is far leſs eaſy than he thinks, to be as ſure, that he ſhall have the praiſes of future ages, as that (though he have them) he ſhall not hear them."

The paſt conſiderations have, I preſume, convinced you, that it is no ſuch eaſy matter for a naturaliſt to acquire a great reputation, and be ſure it will prove a laſting one. Wherefore, that I may alſo confirm the ſecond part of what formerly I propoſed, I now proceed to ſhow, that, though the caſe were otherwiſe, yet he would have no reaſon to ſlight the ſtudy of divinity.

1. For, in the firſt place, nothing hinders, but that a man, who values and enquires into the myſteries of religion, may attain to an eminent degree in the knowledge of thoſe of nature. For frequently men of great parts may ſucceſsfully apply themſelves to more than one ſtudy; and few of them have their thoughts and hours ſo much engroſſed by that one ſubject or employment, but that, if they have great inclinations, as well as fitneſs for the ſtudy of nature, they will find time, not only to cultivate it, but to excel in it. You need not be told, that *Copernicus*, to whom our late philoſophers owe ſo much, was a churchman; that his champion *Lansbergius* was a miniſter, and that *Gaſſendus* himſelf was a doctor of divinity. Among the Jeſuits you know, that *Clavius*, and divers others, have as proſperouſly addicted themſelves to mathematicks as divinity. And as to phyſicks, not only *Scheiner*, *Aquilonius*, *Kircher*, *Schottus*, *Zucchius*, and others, have very laudably cultivated the optical and ſome other parts of philoſophy; but *Ricciolus* himſelf, the learned compiler of that voluminous and judicious work of the *Almageſtum novum*, wherein he has inſerted divers accurate obſervations of his own, is not only a divine, but a profeſſor of divinity. And without going out of our own country, I could, if I durſt for fear of offending the modeſty of thoſe I ſhould name, or injuring the merit of thoſe I ſhould omit; I could, I ſay, if it

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[*] See a Tract on this Subject, premiſed by the Author to his Book of Cold.

were not for this, among our Engliſh eccleſiaſticks name you divers, who though they apply themſelves ſo much to the ſtudy of the ſcripture, as to be not only ſolid divines, but excellent preachers, have yet been ſo happily converſant with nature, that, if they had lived in the learned times of the Greeks, they would have rivalled, if not eclipſed, ſome of them, *Pythagoras* and *Euclid*; others of them, *Anaxagoras* and *Epicurus*; and ſome of them, even *Archimedes* and *Democritus* themſelves.

AND certainly, provided there be curioſity and induſtry enough employed in the ſtudy of nature, it is not neceſſary, that the knowledge of nature ſhould be the ultimate end of that ſtudy; a fondneſs of the object being required only in order to the engaging the mind to ſuch a ſerious application, as a higher aim may ſufficiently invite us to; and will rather promote than diſcourage. *David* became no leſs ſkilful in muſick, than thoſe, that were addicted to it only to pleaſe themſelves in it; though we may reaſonably ſuppoſe, that ſo pious an author of pſalms and inſtruments aſpired to an excellency in that delightful ſcience, that he might apply and preſer it to the ſervice of the temple, and promote the celebration of God's praiſes with it. And as experience has manifeſted, that the heathen philoſophers, that courted moral virtue for herſelf, did not raiſe it to that pitch, to which it was advanced by the heroick practiſes of thoſe true Chriſtians, that in the higheſt exerciſe of virtue had a religious aim at the pleaſing and enjoying of God; ſo I ſee not, why natural knowledge muſt be more proſperouſly cultivated by thoſe ſelfiſh naturaliſts, that aim but at the pleaſing of themſelves in the attainment of that knowledge, than thoſe religious naturaliſts, who are invited to attention and induſtry, not only by the pleaſantneſs of the knowledge it ſelf, but by a higher and more engaging conſideration; namely, that by the diſcoveries they make in the book of nature, both themſelves and others may be excited and qualified the better to admire and praiſe the author, whoſe goodneſs does ſo well match the wiſdom they celebrate, that he declares in his word, that "thoſe, that "honour him, he will honour."

Amos vi. 5.

1 Sam. ii. 30.

AND as a man, that is not in love with a fair lady, but has only a reſpect for her, may have as true and perfect, though not as diſcompoſing an idea of her face, as the moſt paſſionate Inamorato; ſo I ſee not, why a religious and inquiſitive contemplator of nature may not be liable to give a good account of her, without preferring her ſo far to all other objects of his ſtudy, as to make her his miſtreſs, and perhaps too his idol.

II. AND now I proceed to conſider in the ſecond place, that matters of divinity may, as well as thoſe of philoſophy, afford a reputation to him, that diſcovers, or illuſtrates them. For though the fundamental articles of Chriſtian religion be, as I have formerly declared, little leſs evident than important; yet there are many other points in divinity, and paſſages in the ſcripture, which (for reaſons, that I have elſewhere mentioned) are exceeding hard to be cleared, and do not only poſe ordinary readers, and the common ſort of ſcholars, but will ſufficiently exerciſe the abilities of a great wit, and give him opportunity enough to manifeſt, that he is one. For divers of the points I ſpeak of, are much benighted upon the ſcore of the ſublimity of the things they treat of; ſuch as are the nature, attributes, and decrees of God, which cannot be eaſy to the dim underſtandings of us, that are but men: and many other particulars, that are not abſtruſe in their own nature, are yet made obſcure to us by our ignorance, (or at leaſt imperfect knowledge,) of the diſuſed languages, wherein they are delivered, and the great remoteneſs of the ages when, and the countries where, the things recorded were done or ſaid. So that oftentimes a man may need and ſhow as great learning and judgment to diſpel the darkneſs, wherein time has involved things, as that, which nature has caſt on them: and in effect we ſee, that St. *Auguſtin*, St. *Hierom*, *Origen*, and others of the fathers, have acquired no leſs a reputation, than *Empedocles*, *Anaxagoras*, or *Zeno*; and *Grotius*, *Salmaſius*, Mr. *Mede*, Dr. *Hamond*, and ſome other critical expounders of difficult texts of ſcripture, have thereby got as much credit, as *Fracaſtorius* by his book *De Sympathia & Antipathia*; *Levinus Lemnius* by his *De occultis rerum Miraculis*; or *Cardanus* (and his adverſary *Scaliger*) by what they writ *De Subtilitate*; or even *Fernelius* himſelf by his book *De Abditis rerum Cauſis*. And it will contribute to the credit, which theological diſcoveries and illuſtrations may procure a man, that the importance of the ſubjects, and the earneſtneſs, wherewith men are wont to buſy themſelves about them, ſome upon the ſcore of piety, and others upon that of intereſt, ſome to learn truths, and others to defend what they have long or publickly taught for truth, does make greater numbers of men take notice of ſuch matters, and concern themſelves far more about them, than about almoſt any other things, and eſpecially far more, than about matters purely philoſophical, which but few are wont to think themſelves fit to judge of, and concerned to trouble themſelves about. And accordingly we ſee, that the writings of *Socinus Calvin*, *Bellarmine*, *Padre Paulo*, *Arminius*, *&c.* are more famous, and more ſtudied, than thoſe of *Teleſius*, *Campanella*, *Severinus Danus*, *Magnenus*, and divers other innovators in natural philoſophy. And *Eraſtus*, though a very learned phyſician, is much leſs famous for all his elaborate diſputations againſt *Paracelſus*, than for the little tract againſt particular forms of church-government. And I preſume you have taken notice, as well as I, that there are ſcarce any five new controverſies in all phyſicks, that are known to, and hotly contended for by ſo many, as are the five articles of the Remonſtrants.

III. My ſecond conſideration being thus diſpatched, it remains, that I tell you in the third place, that ſuppoſing, but not granting, that to proſecute the ſtudy of divinity, one muſt of neceſſity neglect the acquiſt of reputation; yet this inconvenience itſelf ought not to deter us from the duty they would diſſuade. For in all deliberations, wherein any thing is propoſed to be quitted or declined, to obey or pleaſe God; methinks, we may fitly apply that of the prophet to the Jewiſh king, who being perſuaded (to expreſs his concern for God's glory) to decline the aſſiſtance of an idolatrous army of *Iſraelites*, and objecting, that by complying with the advice given him, he ſhould loſe a ſum of money, amounting to no leſs than the hire of a potent army; received from the prophet this briſk, but rational anſwer, 2 Chron. xxv. 9. “The Lord is able to give thee far more than “this.” The apoſtle *Paul*, who had been traduced, reviled, buffetted, ſcourged, impriſoned, ſhip-wrecked, and ſtoned for his zeal to propagate the truths, whoſe ſtudy I plead for; after he had once had a glimpſe of that great recompence of reward, that is reſerved for us in heaven, ſcruples not to pronounce, that he finds upon caſting up the account (for he uſes the arithmetical λογίζομαι) Rom. viii. 18. “that the ſufferings of this preſent time are “not worthy to be compared with the glory, that “is to be revealed in us.” And if all, that the perſecuted Chriſtians of his time could ſuf- Luke xxiii. 15. fer were not ſuitable (for ſo I remember the ſame Greek word to ſignify elſewhere) or proportionable to that glory; it will ſure far outweigh what we can now forego or decline for it; the loſs of an advantage, and much more the bare miſſing of it, being uſually but a negative affliction, in compariſon of the actual ſufferance of evil. Chriſt did not only tell his diſciples, that he, who ſhould give the leaſt of his followers ſo much as a cup of cold water upon the ſcore of their relation to him, ſhould not be unrewarded; but when the ſame perſons aſked him, what ſhould be done to them, who had left all to follow him, he preſently allots them thrones, as much over-valuing that all they had loſt, as an ordinary recompence may exceed a cup of cold water. And indeed God's goodneſs is ſo great, and his treaſure ſo unexhauſted, that as he is forward to recompence even the leaſt ſervices, that can be done him, ſo he is able to give the greateſt a proportionable reward. *Solomon* had an opportunity, ſuch as never any mortal had, (that we know of,) either before or ſince, of ſatisfying his deſires, whether of fame, or any other 1 Kings iii. 5. thing, that he could wiſh; “Aſk what I ſhall “give thee,” was the proffer made him by him, that could give all things worth receiving; and yet the wiſdom even of *Solomon*'s choice, approved by God himſelf, conſiſted in declining the moſt ambitious things of this life for thoſe things, that might the better qualify him to ſerve and pleaſe God. And to give you an example in a greater than *Solomon*, we may Phil. ii. 6. conſider, that he, “who being in the form of “God, thought it not robbery to be equal “with God;” and who, by leaving heaven, did, to dwell on earth, quit more than any inhabitant of the earth can gain in heaven, and denied more to become capable of being tempted, than he did when he was tempted with an offer of all the kingdoms of the world, and the glory of them: this Saviour, I ſay, is ſaid in ſcripture to have, “for the joy, that was ſet before Heb. xii. 2. “him, endured the croſs, and deſpiſed the “ſhame;” as if heaven had been a ſufficient recompence for even his renouncing honours, and embracing torments.

He, that declines the acquiſt of the applauſe of men for the contemplation of the truths of God, does but forbear to gather that, whilſt it is immature, which, by waiting God's time, he will more ſeaſonably gather when it is full ripe, and wholeſome, and ſweet. That immarceſcible crown, as St. *Peter* calls it, which the Goſpel promiſes to them, “who, by patient continuance Rom. ii. 7. “in well-doing, ſeek for glory and “honour,” will make a rich amends for the declining of a fading wreath here upon earth, where reputation is oftentimes as undeſervedly acquired, as loſt; whereas in heaven, the very having celeſtial honours argues a title to them. And ſince it is our Saviour's reaſoning, that his diſciples ought to rejoice when their reputation is purſued by calumny, as well as their lives by perſecution, “becauſe their reward is great in Matth. v. 11, 12. “heaven,” we may juſtly infer, that the grounded expectation of ſo illuſtrious a condition may bring us more content, even when it is not attended with a preſent applauſe, than this applauſe can give thoſe, who want that comfortable expectation. So that, upon the whole matter, we have no reaſon to deſpond, or to complain of the ſtudy of theology, for but making us decline an empty and tranſitory fame for a ſolid and eternal glory.

The CONCLUSION.

BY this time, Sir, I have ſaid as much as I think fit (and therefore, I hope, more than upon your ſingle account was neceſſary) to manifeſt, that *Phyſeophilus* had no juſt cauſe to undervalue the ſtudy of divinity nor our friend the doctor, for addicting himſelf to it. I hope you have not forgotten what I expreſsly enough declared at the beginning of this letter, that both your friend and you admitting the holy ſcriptures, I know myſelf thereby to be warranted to draw proofs from their authority. And if I need not remind you of this, perhaps I need not tell you by way of apology, that I am not ſo unacquainted with the laws of diſcourſing, but that, if I had been to argue with Atheiſts or Scepticks, I ſhould have forborn to make uſe of divers of the arguments I have employed, as fetched from unconceded topicks, and ſubſtituted others for ſuch, as yet, I think, it very allowable for me to urge, when I deal with a perſon, that, as your friend, does only undervalue the ſtudy of the ſcriptures, nor reject their authority. And if the prolixity I have been guilty of already, did forbid me to encreaſe it by apologies not abſolutely neceſſary, I ſhould perchance, rather think myſelf obliged to excuſe the plainneſs of the ſtile of this diſcourſe; which both upon the ſubject's ſcore, and yours, may ſeem to challenge a richer dreſs. But the matter is very ſerious, and you are a philoſopher, and when the things we treat of are highly important, I think, truths clearly made out to be the moſt perſuaſive pieces of oratory. And a diſcourſe of this nature is more likely to prove effectual on intelligent peruſers, by having the reaſons it preſents perſpicuouſly propoſed, and unprejudicedly entertained, than by their being pathetically urged, or curiouſly adorned. And I have the rather forborn expreſſions, that might ſeem more proper to move, than to convince; becauſe I foreſee, I may very ſhortly have occaſion to employ ſome of the former ſort in another letter to a friend of yours and mine, who will, I doubt, make you a ſharer in the trouble of reading it. But writing this for you, and *Phyſeophilus*, I was far more ſolicitous to give the arguments I employ a good than a bright gloſs. For even when we would excite devotion, if it be in rational men, the moſt effectual pieces of oratory are thoſe, which like burning-glaſſes inflame, by nothing but numerous and united beams of light. If this letter prove ſo happy as to give you any ſatisfaction, it will thereby bring me a great one. For prizing you as I do, I cannot but wiſh to ſee you eſteem thoſe things now, which I am confident we ſhall always have cauſe to eſteem; and then moſt, when the light of glory ſhall have made us better judges of the true worth of things. And it would extreamly trouble me to ſee you a diſeſteemer of thoſe divine things, which as long as a man undervalues, the poſſeſſion of heaven itſelf would not make him happy. And therefore, if the bleſſing of him, whoſe glory is aimed at in it, make the ſucceſs of this paper anſwerable to the wiſhes, the importance of the ſubject will make the ſervice done you by it, ſuitable to the deſires of,

SIR,

Your moſt faithful,

moſt affectionate,

and moſt humble ſervant.

ABOUT

ABOUT THE EXCELLENCY AND GROUNDS OF THE MECHANICAL HYPOTHESIS, SOME CONSIDERATIONS, Occaſionally propoſed to a FRIEND.

The PUBLISHER's ADVERTISEMENT.

THE following paper having been but occaſionally and haſtily penned, long after what the author had written (by way of dialogue) about the requiſites of a good hypotheſis, it was intended, that if it came forth at all, it ſhould do ſo as an appendix to that diſcourſe; becauſe, though one part of it does little more than name ſome of the heads treated of in the dialogue, yet, according to the exigency of the occaſion, the other part contains ſeveral things either pretermitted, or but more lightly touched on in this diſcourſe. But, although the author's deſign were to reſerve theſe thoughts, as a kind of paralipomena to his dialogue; yet, ſince he is not willing to let that, at leaſt quickly, come abroad, and theſe are fallen into my hands; I will make bold, with his good leave, to annex them to the foregoing treatiſe, not only to compleat the bulk of the book, but becauſe of ſome affinity between them, ſince both aim at manifeſting the excellency of the ſtudies they would recommend. And perhaps it will not be unwelcome to ſome of the curious to find, that our noble author in the ſame book, wherein he prefers the ſtudy of divine things to that of natural ones, does himſelf prefer the mechanical principles before all other hypotheſes about natural things; they being in their own nature ſo accommodate, to make conſidering men underſtand, rather than diſpute of, the effects of nature.

OF THE EXCELLENCY and GROUNDS OF THE CORPUSCULAR or MECHANICAL PHILOSOPHY.

THE importance of the queſtion, you propoſe, would oblige me to refer you to "the dialogue about a good "hypotheſis," and ſome other papers of that kind, where you may find my thoughts about the advantages of the mechanical hypotheſis ſomewhat amply ſet down, and diſcourſed of. But, ſince your deſires confine me to deliver in few words, not what I believe reſolvedly, but what I think may be probably ſaid for the preference or the pre-eminence of the corpuſcular philoſophy above *Ariſtotle*'s, or that of the chemiſts, you muſt be content to receive from me, without any preamble, or exact method, or ample diſcourſes, or any other thing, that may coſt many words, a ſuccinct mention of ſome of the chief advantages of the hypotheſis we incline to. And I the rather comply, on this occaſion, with your curioſity, becauſe I have often obſerved you to be alarmed and diſquieted, when you hear of any book, that pretends to uphold, or repair the decaying philoſophy of the ſchools, or ſome bold chymiſt, that arrogates to thoſe of his ſect the title of philoſophers, and pretends to build wholly upon experience, to which he would have all other naturaliſts thought ſtrangers. That therefore you may not be ſo tempted to deſpond, by the confidence or reputation of thoſe writers, that do ſome of them applaud, and others cenſure, what, I fear, they do not underſtand, (as when the Peripateticks cry up ſubſtantial forms, and the chemiſts, mechanical explications) of nature's phænomena, I will propoſe ſome conſiderations, that, I hope, will not only keep you kind to the philoſophy you have embraced, but perhaps, (by ſome conſiderations, which you have not yet met with,) make you think it probable, that the new attempts you hear of from time to time, will not overthrow the corpuſcularian philoſophy, but either be foiled by it, or found reconcilable to it.

But when I ſpeak of the corpuſcular or mechanical philoſophy, I am far from meaning with the Epicureans, that atoms, meeting together by chance in an infinite vacuum, are able of themſelves to produce the world, and all its phænomena; nor with ſome modern philoſophers, that, ſuppoſing God to have put into the whole maſs of matter ſuch an invariable quantity of motion, he needed do no more to make the world, the material parts being able by their own unguided motions, to caſt themſelves into ſuch a ſyſtem (as we call by that name:) but I plead only for ſuch a philoſophy, as reaches but to things purely corporeal, and diſtinguiſhing between the firſt original of things, and the ſubſequent courſe of nature, teaches, concerning the former, not only that God gave motion to matter, but that in the beginning he ſo guided the various motions of the parts of it, as to contrive them into the world he deſigned they ſhould compoſe, (furniſhed with the ſeminal principles and ſtructures, or models of living creatures,) and eſtabliſhed thoſe rules of motion, and that order amongſt things corporeal, which we are wont to call the laws of nature. And having told this as to the former, it may be allowed as to the latter to teach, that the univerſe being once framed by God, and the laws of motion being ſettled and all upheld by his inceſſant concourſe and general providence, the phænomena of the world thus conſtituted are phyſically produced by the mechanical affections of the parts of matter, and what they operate upon one another according to mechanical laws. And now having ſhewn what kind of corpuſcular philoſophy it is, that I ſpeak of, I proceed to the particulars, that I thought the moſt proper to recommend it.

I. The firſt thing, that I ſhall mention to this purpoſe, is the intelligibleneſs or clearneſs of mechanical principles and explications. I need not tell you, that among the Peripateticks, the diſputes are many and intricate about matter, privation, ſubſtantial forms, and their eduction, &c. And the chemiſts are ſufficiently puzzled, (as I have elſewhere ſhewn,) to give ſuch definitions and accounts of their hypoſtatical principles, as are reconcileable to one another, and even to ſome obvious phænomena. And much more dark and intricate are their doctrines about the Archeus, Aſtral Beings, Gas, Blaſs, and other odd notions, which perhaps have in part occaſioned the darkneſs and ambiguity of their expreſſions, that could not be very clear, when their conceptions were far from being ſo. And if the principles of the Ariſtotelians and Spagyriſts are thus obſcure, it is not to be expected, the explications, that are made by the help only of ſuch principles ſhould be clear. And indeed many of them are either ſo general and ſlight, or otherwiſe ſo unſatisfactory, that granting their principles, it is very hard to underſtand or admit their applications of them to particular phænomena. And even in ſome of the more ingenious and ſubtle of the peripatetick diſcourſes upon their ſuperficial and narrow theories, methinks, the authors have better plaid the part of painters than philoſophers, and have only had the ſkill, like drawers of landſkips, to make men fancy they ſee caſtles and towns, and other ſtructures, that appear ſolid and magnificent, and to reach to a large extent, when the whole piece is ſuperficial, and made up of colours and art, and compriſed within a frame perhaps ſcarce a yard long. But to come now to the corpuſcular philoſophy, men do ſo eaſily underſtand one another's meaning, when they talk of local motion, reſt, bigneſs, ſhape, order, ſituation, and contexture of material ſubſtances; and theſe principles do afford ſuch clear accounts of thoſe things, that are rightly deduced from them only, that even thoſe Peripateticks or chymiſts, that maintain other principles, acquieſce in the explications made by theſe, when they can be had, and ſeek not any further, though perhaps the effect be ſo admirable, as would make it paſs for that of a hidden form, or occult quality. Thoſe very Ariſtotelians, that believe the celeſtial bodies to be moved by intelligences, have no recourſe to any peculiar agency of theirs to account for eclipſes. And we laugh at thoſe Eaſt-Indians, that to this day go out in multitudes, with ſome inſtruments, that may relieve the diſtreſſed luminary, whoſe loſs of light they fancy to proceed from ſome fainting fit, out of which it muſt be rouzed. For no intelligent man, whether chemiſt or Peripatetic, flies to his peculiar principles, after he is informed, that the moon is eclipſed by the interpoſition of the earth betwixt her and it, and the ſun by that of the moon betwixt him and the earth. And when we ſee the image of a man caſt into the air by a concave ſpherical looking-glaſs, though moſt men are amazed at it, and ſome ſuſpect it to be no leſs than an effect of witchcraft, yet he, that is ſkilled enough in catoptricks, will, without conſulting *Ariſtotle*, or *Paracelſus*, or flying to hypoſtatical principles and ſubſtantial forms, be ſatisfied, that the phænomenon is produced by the beams of light reflected, and thereby made convergent according to optical, and conſequently mathematical laws.

BUT I muſt not now repeat what I elſewhere ſay, to ſhew, that the corpuſcular principles have been declined by philoſophers of different ſects, not becauſe they think not our explications clear, if not much more ſo, than their own; but becauſe they imagine, that the applications of them can be made but to few things, and conſequently are inſufficient.

II. IN the next place I obſerve, that there cannot be fewer principles than the two grand ones of mechanical philoſophy, matter and motion. For, matter alone, unleſs it be moved, is altogether unactive; and whilſt all the parts of the body continue in one ſtate without any motion at all, that body will not exerciſe any action, nor ſuffer any alteration itſelf, though it may perhaps modify the action of other bodies, that move againſt it.

III. NOR can we conceive any principles more primary, than matter and motion. For, either both of them were immediately created by God, or, (to add that for their ſakes, that would have matter to be unproduced,) if matter be eternal, motion muſt either be produced by ſome immaterial ſupernatural agent, or it muſt immediately flow by way of emanation from the nature of the matter it appertains to.

IV. NEITHER can there be any phyſical principles more ſimple than matter and motion; neither of them being reſoluble into any things, whereof it may be truly, or ſo much as tolerably ſaid to be compounded.

V. THE next thing I ſhall name to recommend the corpuſcular principle, is their great comprehenſiveneſs. I conſider then, that the genuine and neceſſary effect of the ſufficiently ſtrong motion of one part of matter againſt another, is, either to drive it on in its intire bulk, or elſe to break or divide it into particles of determinate motion, figure, ſize, poſture, reſt, order or texture. The two firſt of theſe, for inſtance, are each of them capable of numerous varieties. For the figure of a portion of matter may either be one of the five regular figures treated of by geometricians, or ſome determinate ſpecies of ſolid figures, as that of a cone, cylinder, &c. or irregular, though not perhaps anonymous, as the grains of ſand, hoops, feathers, branches, forks, files, &c. And as the figure, ſo the motion of one of theſe particles may be exceedingly diverſified, not only by the determination to this or that part of the world, but by ſeveral other things, as particularly by the almoſt infinitely varying degrees of celerity, by the manner of its progreſſion with, or without rotation, and other modifying circumſtances; and more yet, by the line, wherein it moves, as (beſides ſtreight) circular, elliptical, parabolical, hyperbolical, ſpiral, and I know not how many others. For as later geometricians have ſhewn, that thoſe crooked lines may be compounded of ſeveral motions, (that is, traced by a body, whoſe motion is mixed of, and reſults from, two or more ſimpler motions,) ſo how many more curves may, or rather may not be made by new compoſitions and decompoſitions of motion, is no eaſy taſk to determine.

NOW, ſince a ſingle particle of matter, by virtue of two only of the mechanical affections, that belong to it, be diverſifiable ſo many ways; how vaſt a number of variations may we ſuppoſe capable of being produced by the compoſitions and decompoſitions of myriads of ſingle inviſible corpuſcles, that may be contained and contexed in one ſmall body, and each of them be embued with more than two or three of the fertile catholick ptinciples abovementioned? Eſpecially ſince the aggregate of thoſe corpuſcles may be farther diverſified by the texture reſulting from their convention into a body, which, as ſo made up, has its own bigneſs, and ſhape, and pores, (perhaps very many and various) and has alſo many capacities of acting and ſuffering upon the ſcore of the place it holds among other bodies in a world conſtituted as ours is: ſo that, when I conſider the almoſt innumerable diverſifications, that compoſitions and decompoſitions may make of a ſmall number, not perhaps exceeding twenty of diſtinct things, I am apt to look upon thoſe, who think the mechanical principles may ſerve indeed to give an account of the phænomena of this or that particular part of natural philoſophy, as ſtaticks, hydroſtaticks, the theory of the planetary motions, &c. but can never be applied to all the phænomena of things corporeal; I am apt, I ſay, to look upon thoſe, otherwiſe learned men, as I would do upon him, that ſhould affirm, that by putting together the letters of the alphabet, one may indeed make up all the words to be found in one book, as in *Euclid*, or *Virgil*; or in one language, as Latin, or Engliſh; but that they can by no means ſuffice to ſupply words to all the books of a great library, much leſs to all the languages in the world.

AND whereas there is another ſort of philoſophers, that, obſerving the great efficacy of the bigneſs, and ſhape, and ſituation, and motion, and connexion in engines, are willing to allow, that thoſe mechanical principles may have a great ſtroke in the operations of bodies of a ſenſible bulk, and manifeſt mechaniſm, and therefore may be uſefully employed in accounting for the effects and phænomena of ſuch bodies, who yet will not admit, that theſe principles can be applied to the hidden tranſactions, that paſs among the minute particles of bodies; and therefore think it neceſſary to refer theſe to what they call nature, ſubſtantial forms, real qualities, and the like unmechanical principles and agents.

BUT this is not neceſſary; for both the mechanical affections of matter are to be found, and the laws of motion take place, not only in the great maſſes, and the middle ſized lumps, but in the ſmalleſt fragments of matter; and a leſſer portion of it being as well a body as a greater, muſt, as neceſſarily as it, have its determinate bulk and figure: and he, that looks upon ſand in a good microſcope, will eaſily perceive, that each minute grain of it has as well its own ſize and ſhape, as a rock or mountain. And when we let fall a great ſtone and a pebble from the top of a high building, we find not, but that the latter as well as the former moves conformably to the laws of acceleration in heavy bodies deſcending. And the rules of motion are obſerved, not only in cannon bullets, but in ſmall ſhot; and the one ſtrikes down a bird according to the ſame laws, that the other batters down a wall. And though nature (or rather its divine author) be wont to work with much finer materials, and employ more curious contrivances than art, (whence the ſtructure even of the rareſt watch is incomparably inferior to that of a human body;) yet an artiſt himſelf, according to the quantity of the matter he employs, the exigency of the deſign he undertakes, and the bigneſs and ſhape of the inſtruments he makes uſe of, is able to make pieces of work of the ſame nature or kind of extremely differing bulk, where yet the like, though not equal art and contrivance, and oftentimes motion too, may be obſerved: as a ſmith, who with a hammer, and other large inſtruments, can, out of maſſes of iron, forge great bars or wedges, and make thoſe ſtrong and heavy chains, that were employed to load malefactors, and even to ſecure ſtreets and gates, may, with leſſer inſtruments, make ſmaller nails and filings, almoſt as minute as duſt; and may yet, with finer tools, make links of a ſtrange ſlenderneſs and lightneſs, inſo much, that good authors tell us of a chain of divers links, that was faſtened to a flea, and could be moved by it; and if I miſremember not, I ſaw ſomething like this, beſides other inſtances, that I beheld with pleaſure, of the littleneſs, that art can give to ſuch pieces of work, as are uſually made of a conſiderable bigneſs. And therefore to ſay, that though in natural bodies, whoſe bulk is manifeſt and their ſtructure viſible, the mechanical principles may be uſefully admitted, that are not to be extended to ſuch portions of matter, whoſe parts and texture are inviſible; may perhaps look to ſome, as if a man ſhould allow, that the laws of mechaniſm may take place in a town-clock, but cannot in a pocket-watch; or, (to give you an inſtance, mixed of natural and artificial,) as if, becauſe the terraqueous globe is a vaſt magnetical body of ſeven or eight thouſand miles in diameter, one ſhould affirm, that magnetical laws are not to be expected to be of force in a ſpherical piece of loadſtone, that is not perhaps an inch long: and yet experience ſhews us, that notwithſtanding the ineſtimable diſproportion betwixt theſe two globes, the terrella, as well as the earth, hath its poles, æquator, and meridians, and in divers other magnetical properties, emulates the terreſtrial globe.

THEY, that, to ſolve the phænomena of nature, have recourſe to agents, which, though they involve no ſelf-repugnancy in their very notions, as many of the judicious think ſubſtantial forms and real qualities to do, yet are ſuch, that we conceive not, how they operate to bring effects to paſs: theſe, I ſay, when they tell us of ſuch indeterminate agents, as the ſoul of the world, the univerſal ſpirit, the plaſtic power, and the like; though they may in certain caſes tell us ſome things, yet they tell us nothing, that will ſatisfy the curioſity of an inquiſitive perſon, who ſeeks not ſo much to know, what is the general agent, that produces a phænomenon, as, by what means, and after what manner, the phænomenon is produced. The famous *Sennertus*, and ſome other learned phyſicians, tell us of diſeaſes, which proceed from incantation; but ſure it is but a ſlight account, that a ſober phyſician, that comes to viſit a patient reported to be bewitched, receives of the ſtrange ſymptoms he meets with, and would have an account of, if he be coldly anſwered, that it is a witch, or the devil, that produces them; and he will never ſit down with ſo ſhort an account, if he can by any means reduce thoſe extravagant ſymptoms to any more known and ſtated diſeaſes, as epilepſies, convulſions, hyſterical fits, &c. and, if he cannot, he will confeſs his knowledge of this diſtemper to come far ſhort of what might be expected and attained in other diſeaſes, wherein he thinks himſelf bound to ſearch into the nature of the morbific matter, and will not be ſatisfied, till he can, probably at leaſt, deduce from that, and the ſtructure of an human body, and other concurring phyſical cauſes, the phænomena of the malady. And it would be but little ſatisfaction to one, that deſires to underſtand the cauſes of what occurs to obſervation in a watch, and how it comes to point at, and ſtrike the hours, to be told, that it was ſuch a watch-maker that ſo contrived it; or to him, that would know the true cauſe of an eccho, to be anſwered, that it is a man, a vault, or a wood, that makes it.

And now at length I come to conſider that, which I obſerve the moſt to alienate other ſects from the mechanical philoſophy; namely, that they think it pretends to have principles ſo univerſal and ſo mathematical, that no other phyſical hypotheſis can comport with it, or be tolerated by it.

But this I look upon as an eaſy, indeed, but an important miſtake; becauſe by this very thing, that the mechanical principles are ſo univerſal, and therefore applicable to ſo many things, they are rather fitted to include, than neceſſitated to exclude, any other hypotheſis, that is founded in nature, as far as it is ſo. And ſuch hypotheſes, if prudently conſidered by a ſkilful and moderate perſon, who is rather diſpoſed to unite ſects than multiply them, will be found, as far as they have truth in them, to be either legitimately (though perhaps not immediately) deducible from the mechanical principles, or fairly reconcilable to them. For, ſuch hypotheſes will probably attempt to account for the phænomena of nature, either by the help of a determinate number of material ingredients, ſuch as the *tria prima* of the chemiſts, by participation whereof other bodies obtain their qualities; or elſe by introducing ſome general agents, as the Platonic ſoul of the world, or the univerſal ſpirit, aſſerted by ſome ſpagyriſts; or by both theſe ways together.

Now, to diſpatch firſt thoſe, that I named in the ſecond place; I conſider, that the chief thing, that inquiſitive naturaliſts ſhould look after in the explicating of difficult phænomena, is not ſo much what the agent is or does, as, what changes are made in the patient, to bring it to exhibit the phænomena, that are propoſed; and by what means, and after what manner, thoſe changes are effected. So that the mechanical philoſopher being ſatisfied, that one part of matter can act upon another but by virtue of local motion, or the effects and conſequences of local motion, he conſiders, that as if the propoſed agent be not intelligible and phyſical, it can never phyſically explain the phænomena; ſo, if it be intelligible and phyſical, it will be reducible to matter, and ſome or other of thoſe only catholick affections of matter, already often mentioned. And the indefinite diviſibility of matter, the wonderful efficacy of motion, and the almoſt infinite variety of coalitions and ſtructures, that may be made of minute and inſenſible corpuſcles, being duly weighed, I ſee not, why a philoſopher ſhould think it impoſſible, to make out, by their help, the mechanical poſſibility of any corporeal agent, how ſubtil, or diffuſed, or active ſoever it be, that can be ſolidly proved to be really exiſtent in nature, by what name ſoever it be called or diſguiſed. And though the Carteſians be mechanical philoſophers, yet, according to them, their *Materia Subtilis*, which the very name declares to be a corporeal ſubſtance, is, for aught I know, little (if it be at all) leſs diffuſed through the univerſe, or leſs active in it than the univerſal ſpirit of ſome ſpagyriſts, not to ſay, the *Anima Mundi* of the Platoniſts. But this upon the by; after which I proceed, and ſhall venture to add, that whatever be the phyſical agent, whether it be inanimate or living, purely corporeal, or united to an intellectual ſubſtance, the above-mentioned changes, that are wrought in the body, that is made to exhibit the phænomena, may be effected by the ſame or the like means, or after the ſame or the like manner; as for inſtance, if corn be reduced to meal, the materials and ſhape of the milſtones, and their peculiar motion and adaptation, will be much of the ſame kind; and (though they ſhould not, yet) to be ſure the grains of corn will ſuffer a various contrition and comminution in their paſſage to the form of meal; whether the corn be ground by a water-mill, or a wind-mill, or a horſe-mill, or a hand-mill; that is, by a mill, whoſe ſtones are turned by inanimate, by brute, or by rational agents. And, if an angel himſelf ſhould work a real change in the nature of a body, it is ſcarce conceivable to us men, how he could do it without the aſſiſtance of local motion; ſince, if nothing were diſplaced, or otherwiſe moved than before, (the like happening alſo to all external bodies to which it related,) it is hardly conceivable, how it ſhould be in itſelf other, than juſt what it was before.

But to come now to the other ſort of hypotheſis formerly mentioned; if the chemiſts, or others, that would deduce a compleat natural philoſophy from ſalt, ſulphur, and mercury, or any other ſet number of ingredients of things, would well conſider, what they undertake, they might eaſily diſcover, that the material parts of bodies, as ſuch, can reach but to a ſmall part of the phænomena of nature, whilſt theſe ingredients are conſidered but as quieſcent things, and therefore they would find themſelves neceſſitated to ſuppoſe them to be active; and that things purely corporeal cannot be but by means of local motion, and the effects, that may reſult from that, accompanying variouſly ſhaped, ſized, and aggregated parts of matter: ſo that the chemiſt and other materialiſts, if I may ſo call them, muſt (as indeed they are wont to do) leave the greateſt part of the phænomena of the univerſe unexplicated by the help of the ingredients (be they fewer or more than three) of bodies, without taking in the mechanical, and more comprehenſive affections of matter, eſpecially local motion. I willingly grant, that ſalt, ſulphur, and mercury, or ſome ſubſtances analogous to them, are to be obtained by the action of the fire, from a very great many diſſipable bodies here below; nor would I deny, that in explicating divers of the phænomena of ſuch bodies, it may be of uſe to a ſkilful naturaliſt to know and conſider, that this or that ingredient, as ſulphur, for inſtance, does abound in the body propoſed, whence it may be probably argued, that the qualities, that uſually accompany that principle, when predominant, may be alſo, upon its ſcore, found in the body, that ſo plentifully partakes of it. But not to mention, what I have elſewhere ſhewn, that there are many phænomena, to whoſe explication this knowledge will contribute very little or nothing at all; I ſhall only here obſerve, that, though chemical explications be ſometimes the moſt obvious and ready, yet they are not the moſt fundamental and ſatisfactory: for, the chemical ingredient itſelf, whether ſulphur or any other, muſt owe its nature and other qualities to the union of inſenſible particles in a convenient ſize, ſhape, motion or reſt, and contexture; all which are but mechanical affections of convening corpuſcles. And this may be illuſtrated by what happens in artificial fire-works. For, though in moſt of thoſe many differing ſorts that are made, either for the uſe of war, or for recreation, gunpowder be a main ingredient, and divers of the phænomena may be derived from the greater or leſſer meaſure, wherein the compoſitions partake of it; yet, beſides that there may be fire-works made without gun-powder, (as appears by thoſe made of old by the Greeks and Romans,) gunpowder itſelf owes its aptneſs to be fired and exploded to the mechanical contexture of more ſimple portions of matter, nitre, charcoal, and ſulphur; and ſulphur itſelf, though it be by many chemiſts miſtaken for an hypoſtatical principle, owes its inflammability to the convention of yet more ſimple and primary corpuſcles; ſince chemiſts confeſs, that it has an inflammable ingredient, and experience ſhews, that it very much abounds with an acid and uninflammable ſalt, and is not quite devoid of terreſtreity. I know it may be here alledged, that the productions of chemical analyſes are ſimple bodies, and upon that account irreſoluble. But, that divers ſubſtances, which chemiſts are pleaſed to call the ſalts, or ſulphurs, or mercuries of the bodies, that afforded them, are not ſimple and homogeneous, has elſewhere been ſufficiently proved; nor is their not being eaſily diſſipable, or reſoluble, a clear proof of their not being made up of more primitive portions of matter. For, compounded, and even decompounded bodies, may be as difficultly reſoluble, as moſt of thoſe, that chemiſts obtain by what they call their analyſis by the fire; witneſs common green glaſs, which is far more durable and irreſoluble than many of thoſe, that paſs for hypoſtatical ſubſtances. And we ſee, that ſome amels will be ſeveral times even vitrified in the fire, without loſing their nature, or oftentimes ſo much as their colour; and yet amel is manifeſtly, not only a compounded, but a decompounded body, conſiſting of ſalt and powder of pebbles or ſand, and calcined tin, and, if the amel be not white, uſually of ſome tinging metal or mineral. But how indeſtructible ſoever the chemical principles be ſuppoſed, divers of the operations aſcribed to them will never be well made out, without the help of local motion, (and that diverſified too;) without which, we can little better give an account of the phænomena of many bodies, by knowing what ingredients compoſe them, than we can explain the operations of a watch, by knowing of how many, and of what metals the balance, the wheels, the chain, and other parts are made; or than we can derive the operations of a wind-mill from the bare knowledge, that it is made up of wood, and ſtone, and canvas, and iron. And here let me add, that it would not at all overthrow the Corpuſcularian hypotheſis, though either by more exquiſite purifications, or by ſome other operations, than the uſual analyſis of the fire, it ſhould be made appear, that the material principles, or elements of mixed bodies, ſhould not be the *tria prima* of the vulgar chemiſts, but either ſubſtances of another nature, or elſe fewer, or more in number; as would be, if that were true, which ſome ſpagyriſts affirm, (but I could never find,) that from all ſorts of mixed bodies, five, and but five, differing ſimilar ſubſtances can be ſeparated: or, as if it were true, that the Helmontians had ſuch a reſolving menſtruum as the Alkaheſt of their maſter; by which he affirms, that he could reduce ſtones into ſalt of the ſame weight with the mineral, and bring both that ſalt, and all other kind of mixed and tangible bodies, into inſipid water. For, whatever be the number or qualities of the chemical principles, if they be really exiſtent in nature, it may very poſſibly be ſhewn, that they may be made up of inſenſible corpuſcles of determinate bulks and ſhapes; and by the various coalitions and contextures of ſuch corpuſcles, not only three or five, but many more material ingredients, may be compoſed or made to reſult. But, though the Alkaheſtical reductions newly mentioned ſhould be admitted, yet the mechanical principles might well be accommodated even to them. For the ſolidity, taſte, &c. of ſalt, may be fairly accounted for, by the ſtiffneſs, ſharpneſs, and other mechanical affections of the minute particles, whereof ſalts conſiſt; and if, by a farther action of the alkaheſt, the ſalt, or any other ſolid body, be reduced into inſipid water, this alſo may be explicated by the ſame principles, ſuppoſing a farther comminution of the parts, and ſuch an attrition, as wears off the edges and points, that enabled them to ſtrike briſkly the organ of taſte: for, as to fluidity and firmneſs, thoſe mainly depend upon two of our grand principles, motion and reſt. And I have elſewhere ſhewn, by ſeveral proofs, that the agitation of reſt, and the looſer contact, or cloſer cohæſion, of the particles, is able to make the ſame portion of matter, at one time a firm, and at another time a fluid body. So that, though the further ſagacity and induſtry of chemiſts (which I would by no means diſcourage) ſhould be able to obtain from mixed bodies homogeneous ſubſtances, differing in number, or nature, or both, from their vulgar ſalt, ſulphur, and mercury; yet the corpuſcular philoſophy is ſo general and fertile, as to be fairly reconcilable to ſuch a diſcovery; and alſo ſo uſeful, that theſe new material principles will, as well as the old *tria prima*, ſtand in need of the more catholick principles of the Corpuſcularians, eſpecially local motion. And indeed, whatever elements or ingredients men have (that I know of) pitched upon, yet if they take not in the mechanical affections of matter, their principles have been ſo deficient, that I have uſually obſerved, that the materialiſts, without at all excepting the chemiſts, do not only, as I was ſaying, leave many things unexplained, to which their narrow principles will not extend; but, even in the particulars, they preſume to give an account of, they either content themſelves to aſſign ſuch common and indefinite cauſes, as are too general to ſignify much towards an inquiſitive man's ſatiſfaction; or if they venture to give particular cauſes, they aſſign precarious or falſe ones, and liable to be eaſily diſproved by circumſtances, or inſtances, whereto their doctrine will not agree, as I have often elſewhere had occaſion to ſhew. And yet the chemiſts need not be frighted from acknowledging the prerogative of the mechanical philoſophy, ſince that may be reconcileable with the truth of their own principles, as far as theſe agree with the phænomena they are applied to. For theſe more confined hypotheſes may be ſubordinated to thoſe more general and fertile principles, and there can be no incredient aſſigned, that has a real exiſtence in nature, that may not be derived either immediately, or by a row of decompoſitions, from the univerſal matter, modified by its mechanical affections. For if, with the ſame bricks, diverſly put together and ranged, ſeveral walls, houſes, furnaces, and other ſtructures, as vaults, bridges, pyramids, &c. may be built, merely by a various contrivement of parts of the ſame kind; how much more may great variety of ingredients be produced by, or, according to the inſtitution of nature, reſult from the various coalitions and contextures of corpuſcles, that need not be ſuppoſed, like bricks, all of the ſame, or near the ſame ſize and ſhape, but may have amongſt them, both of the one and the other, as great a variety as need be wiſhed for, and indeed a greater than can eaſily be ſo much as imagined? And the primary and minute concretions, that belong to theſe ingredients, may, without oppoſition from the mechanical philoſophy, be ſuppoſed to have their particles ſo minute and ſtrongly coherent, that nature of herſelf does ſcarce ever tear them aſunder; as we ſee, that mercury and gold may be ſucceſſively made to put on a multitude of diſguiſes, and yet ſo retain their nature, as to be reducible to their priſtine forms. And you know, I lately told you, that common glaſs and good amels, though both of them but factitious bodies, and not only mixed, but decompounded concretions, have yet their component parts ſo ſtrictly united by the ſkill of illiterate tradeſmen, as to maintain their union in the vitrifying violence of the fire. Nor do we find, that common glaſs will be wrought upon by aqua fortis, or aqua regis, though the former of them will diſſolve mercury, and the latter gold.

FROM the foregoing diſcourſe it may (probably at leaſt) reſult, that if, beſides rational ſouls, there are any immaterial ſubſtances (ſuch as the heavenly intelligences, and the ſubſtantial forms of the Ariſtotelians,) that regularly are to be numbered among natural agents, their way of working being unknown to us, they can but help to conſtitute and effect things, but will very little help us to conceive how things are effected; ſo that by whatever principles natural things be conſtituted, it is by the mechanical principles, that their phænomena muſt be clearly explicated. As for inſtance, though we ſhould grant the Ariſtotelians, that the planets are made of a quinteſſential matter, and moved by angels, or immaterial intelligences; yet, to explain the ſtations, progreſſions, and retrogradations, and other phænomena of the planets, we muſt have recourſe either to eccentricks, epicycles, &c. or to motions made in elliptical or other peculiar lines; and, in a word, to theories, wherein the motion and figure, ſituation, and other mathematical or mechanical affections of bodies are mainly employed. But if the principles propoſed be corporeal things, they will be then fairly reducible, or reconcilable, to the mechanical principles; theſe being ſo general and pregnant, that among things corporeal, there is nothing real, (and I meddle not with chimerical beings, ſuch as ſome of *Paracelſus*'s,) that may not be derived from, or be brought to, a ſubordination to ſuch comprehenſive principles. And when the chemiſts ſhall ſhew, that mixed bodies owe their qualities to the predominancy of this or that of their three grand ingredients, the Corpuſcularians will ſhew, that the very qualities of this, or that ingredient, flow from its peculiar texture, and the mechanical affections of the corpuſcles it is made up of. And to affirm, that, becauſe the furnaces of chemiſts afford a great number of uncommon productions and phænomena, there are bodies or operations amongſt things purely corporeal, that cannot be derived from, or reconciled to, the comprehenſive and pregnant principles of the mechanical philoſophy, is, as if, becauſe there are a great number and variety of anthems, hymns, pavins, threnodies, courants, gavots, branles, ſarabands, jigs, and other (grave and ſprightly) tunes to be met with in the books and practiſes of muſicians, one ſhould maintain, that there are in them a great many tunes, or at leaſt, notes, that have no dependence on the ſcale of muſick; or, as if, becauſe, beſides rhombuſſes, rhomboids, trapeziums, ſquares, pentagons, chiliagons, myriagons, and innumerable other polygons, regular, and irregular, one ſhould preſume to affirm, that there are among them ſome rectilinear figures, that are not reducible to triangles, or have affections, that will overthrow what *Euclid* has taught of triangles and polygons.

TO what has been ſaid I ſhall add but one thing more; that as, according to what I formerly intimated, mechanical principles and explications are for their clearneſs preferred, even by materialiſts themſelves, to others, in the caſes where they can be had; ſo, the ſagacity and induſtry of modern naturaliſts and mathematicians having happily applied them to ſeveral of thoſe difficult phænomena, (in hydoſtaticks, the practical part of opticks, gunnery, &c.) that before were, or might be referred to occult qualities; it is probable, that when this philoſophy is deeplier ſearched into, and farther improved, it will be found applicable to the ſolution of more and more of the phænomena of nature. And on this occaſion let me obſerve, that it is not always neceſſary, though it be always deſirable, that he, that propounds an hypotheſis in aſtronomy, chemiſtry, anatomy, or other part of phyſicks, be able *à priori*, to prove his hypotheſis to be true, or demonſtratively to ſhew, that the other hypotheſes propoſed about the ſame ſubject muſt be falſe. For as, if I miſtake not, *Plato* ſaid, that the world was God's epiſtle written to mankind, and might have added, conſonantly to another ſaying of his, it was written in mathematical letters: ſo, in the phyſical explications of the parts and ſyſtem of the world, methinks, there is ſomewhat like what happens, when men conjecturally frame ſeveral keys to enable us to underſtand a letter written in cyphers. For though one man by his ſagacity have found out the right key, it will be very difficult for him, either to prove otherwiſe than by trial, that this or that word is not ſuch, as it is gueſſed to be by others, according to their keys; or to evince, *à priori*, that their's are to be rejected, and his to be preferred; yet, if due trial being made, the key he propoſes, ſhall be found ſo agreeable to the characters of the letter, as to enable one to underſtand them, and make a coherent ſenſe of them, its ſuitableneſs to what it ſhould decypher, is, without either confutations, or extraneous poſitive proofs, ſufficient to make it be accepted as the right key of that cypher. And ſo, in phyſical hypotheſes, there are ſome, that, without noiſe, or falling foul upon others, peaceably obtain diſcerning men's approbation only by their fitneſs to ſolve the phænomena, for which they were deviſed, without croſſing any known obſervation or law of nature. And therefore, if the mechanical philoſophy go on to explicate things corporeal at the rate it has of late years proceeded at, it is ſcarce to be doubted, but that, in time, unprejudiced perſons will think it ſufficiently recommended by its conſiſtency with itſelf, and its applicableneſs to ſo many phenomena of nature.

Ὁ Θεὸς ἀεὶ γεωμετρεῖ.

A RECAPITULATION.

PERCEIVING, upon a review of the foregoing paper, that the difficulty and importance of the ſubject, has ſeduced me to ſpend many more words about it, than I at firſt deſigned; it will not now be amiſs to give you this ſhort ſummary of what came into my mind, to recommend to you the mechanical philoſophy, and obviate your fears of ſeeing it ſupplanted; having firſt premiſed once for all, that preſuppoſing the creation and general providence of God, I pretend to treat but of things corporeal, and do abſtract in this paper from immaterial Beings, (which otherwiſe I very willingly admit,) and all agents and operations miraculous or ſupernatural.

I. Of the principles of things corporeal, none can be more few, without being inſufficient, or more primary, than matter and motion.

II. The natural and genuine effect of variouſly determined motion in portions of matter is, to divide it into parts of differing ſizes, and ſhapes, and to put them into different motions; and the conſequences, that flow from theſe, in a world framed as ours is, are, as to the ſeparate fragments, poſture, order, and ſituation, and, as to the conventions of many of them, peculiar compoſitions and contextures.

III. The parts of matter endowed with theſe catholick affections are, by various aſſociations, reduced to natural bodies of ſeveral kinds, according to the plenty of the matter, and the various compoſitions and decompoſitions of the principles; which all ſuppoſe the common matter they diverſify: and theſe ſeveral kinds of bodies, by virtue of their motion, reſt, and other mechanical affections, which fit them to act on, and ſuffer from one another, become endowed with ſeveral kinds of qualities, (whereof ſome are called manifeſt, and ſome occult,) and thoſe, that act upon the peculiarly framed organs of ſenſe, whoſe perceptions, by the animadverſive faculty of the ſoul, are ſenſations.

IV. These principles, matter, motion, (to which reſt is related) bigneſs, ſhape, poſture, order, texture, being ſo ſimple, clear, and comprehenſive, are applicable to all the real phænomena of nature, which ſeem not explicable by any other not conſiſtent with ours. For, if recourſe be had to an immaterial principle or agent, it may be ſuch an one, as is not intelligible; and however it will not enable us to explain the phænomena, becauſe its way of working upon things material, would probably be more difficult to be phyſically made out, than a mechanical account of the phænomena. And, notwithſtanding the immateriality of a created agent, we cannot conceive, how it ſhould produce changes in a body, without the help of mechanical principles, eſpecially local motion; and accordingly we find not, that the reaſonable ſoul in man is able to produce what changes it pleaſes in the body, but is confined to ſuch, as it may produce by determining, or guiding the motions of the ſpirits, and other parts of the body, ſubſervient to voluntary motion.

V. And if the agents, or active principles reſorted to, be not immaterial, but of a corporeal nature, they muſt either in effect be the ſame with the corporeal principles above-named; or, becauſe of the great univerſality and ſimplicity of ours, the new ones propoſed, muſt be leſs general than they, and conſequently capable of being ſubordinate, or reduced to ours, which by various compoſitions may afford matter to ſeveral hypotheſes, and by ſeveral coalitions afford minute concretions exceedingly numerous and durable, and conſequently fit to become the elementary ingredients of more compounded bodies, being in moſt trials ſimilar, and as it were the radical parts, which may, after ſeveral manners, be diverſified; as in Latin, the themes are by prepoſitions, terminations, &c. and in Hebrew, the roots by the hæemantic letters. So that the fear, that ſo much of a new phyſical hypotheſis, as is true, will overthrow; or make uſeleſs the mechanical principles, is, as if one ſhould fear, that there will be a language propoſed, that is diſcordant from, or not reducible to, the letters of the alphabet.

TRACTS.

CONTAINING

I. SUSPICIONS about ſome HIDDEN QUALITIES of the AIR; with an APPENDIX touching CELESTIAL MAGNETS, and ſome other PARTICULARS.

II. ANIMADVERSIONS upon Mr. HOBBES's PROBLEMATA DE VACUO.

III. A DISCOURSE of the CAUSE of ATTRACTION by SUCTION.

PREFACE.

AMONG other papers, that I deſigned to contribute towards the natural hiſtory of the air, I began ſome years ago to ſet down a collection of ſome new or leſs heeded obſervations and experiments relating to the cauſes and effects of changes in the air, which I referred to ſeveral heads, as to the air's heat, coldneſs, moiſture, dryneſs, diaphaneity, opacity, conſiſtence, ſeveral ſaltneſſes and other titles; the laſt of which was of the occult qualities of the air, ſuppoſing there be any ſuch. And though afterwards I was, by ſickneſs and other impediments, diverted from proceeding in that collection, and induced to lay aſide ſome of the obſervations I had provided, and employ in other treatiſes, ſuch as were proper to them; yet as to the title, that contained ſuſpicions about ſome hidden qualities of the air, the poſſibility, if not likelihood, that either the matters of fact, or the intimations delivered in them, might afford hints not uſeleſs to the ſagacious and inquiſitive, perſuaded me to let it eſcape the fate of its companions, though poſſibly, if I had more conſulted my own reputation, I ſhould leaſt of all have ſuffered this title to appear, there being none of the reſt, that was not leſs conjectural. But it being thought unfit, that any thing ſhould periſh, that related to ſo conſiderable and uncommon a ſubject, as that of this title, I was content to caſt the collected experiments into the following eſſay, for the reaſons expreſſed at the beginning and cloſe of the enſuing paper. Which, it was hoped, may be the better underſtood, and leſs liable to have its deſign miſtaken, by being uſhered in by this advertiſement about the occaſion of it.

OBSER-

OBSERVATIONS ABOUT THE GROWTH OF METALS in their ORE, Exposed to the AIR.

IT is altogether unneceſſary to my preſent purpoſe, to examine, whether metals and minerals, as if they were a kind of ſubterraneal plants, do properly grow as vegetables do. For this enquiry belongs to another place, but not to this, where the reference made in the 468th page of the following paper does not oblige me to ſpeak of the growth of metals in any other than a lax and popular ſenſe, in which a metal may be ſaid to grow, if a portion of matter being aſſigned, wherein as yet men can find either no metal, as gold or tin, or but ſuch a quantity of it; this being expoſed to the air, will after a time either afford ſome metal, where there appeared none before, or a greater proportion of metal than it had before.

Observations of this kind requiring length of time, as well as reſidence near places abounding with minerals, I have little or no opportunity to make any of them myſelf, at leaſt with the warineſs, that to me ſeems due to obſervations, that I think not eaſy to be well made. And therefore I muſt content myſelf to ſet down what I have been able to learn by converſing with mineraliſts and travellers, and to add ſome particulars, that I met with in authors of good credit.

OBSERVATIONS about the Growth of TIN.

AN ancient owner of mines, being aſked by me, whether he could, otherwiſe than upon the conjectures of vulgar tradition, prove, that minerals grow, even after the veins have been dug? anſwered affirmatively; and being deſired to let me know his proofs, he gave me theſe that follow.

He told me, that not far from his houſe there was a tin-mine, which the old diggers affirmed to have been left off, ſome ſaid eighty, ſome an hundred and twenty years ago, becauſe they had by their waſhing and vanning ſeparated all the ore from the reſt of the earth, and yet of late years they found it ſo richly impregnated with metalline particles, that it was wrought over again with very good profit, and preferred to ſome other mines, that were actually wrought, and had never been ſo robbed. And when I objected, that probably this might proceed from the lazineſs and unſkilfulneſs of workmen in thoſe times, who left in the earth the tin, that was lately ſeparated, and might then have been ſo; I was anſwered, that it was a known thing in the country, that in thoſe times the mine-men were more careful and laborious, to ſeparate the metalline part from the reſt of the ore, than now they are.

He alſo affirmed to me, that in his own time ſome tenants and neighbours of his (employed by him) having got all the ore they could out of a great quantity of ſtuff, dug out of a tin-mine, they laid the remains in great heaps, expoſed to the air, and within twenty and thirty years after, found them ſo richly impregnated, that they wrought them over again with good benefit.

And laſtly, he aſſured me, that in a work of his own, wherein he had exerciſed his ſkill and experience, (which is ſaid to be very great) to ſeparate all the particles of the tin from the terreſtrial ſubſtances, that were dug up with it out of the vein, he cauſed dams to be made to ſtop the earthy ſubſtance, which the ſtream waſhed away from the ore, giving paſſage to the water; after it had let fall this ſubſtance, which lying in heaps expoſed to the air, within ten or twelve years, and ſometimes much leſs, he examined this, or that heap, and found it to contain ſuch ſtore of metalline particles, as invited him to work it again, and do it with profit. And yet this gentleman was ſo dextrous at ſeparating the metalline from the other parts of tin-ore, that I could (not without wonder) ſee, what ſmall corpuſcles he would, to ſatisfy my curioſity, ſever from vaſt quantities (in proportion) of earthy and other mineral ſtuff.

RELATIONS agreeable to theſe I received from another very ingenious gentleman, that was converſant with tin-mines, and lived not far from more than one of them.

I was the more ſolicitous to procure an information about the growth of this metal, becauſe the bulk of that, which is uſed in *Europe*, being found in *England*, I have met with little or no mention of the growth of it in outlandiſh writers.

OBSERVATIONS about the GROWTH of LEAD.

AS for the growth of lead in the ore expoſed to the air, I remember, I enquired about it of a perſon of quality, who had a patent for divers leaden mines, that were ſuppoſed to contain ſilver, and wrought ſome of them himſelf at no ſmall charge, yet not without profit; and, as I remember, he anſwered me, that the lead-ore, that had been wrought and laid in heaps, did, in tract of time, grow impregnated with metal again, and, as experience manifeſted, became worth working a ſecond time. And indeed ſome mineraliſts deliver it as a general obſervation, that the growth and renaſcence of metals is more manifeſt in lead than in any other of them. *Feſſularum mons in Hetruria*, ſays *Boccatius Certardus*, who delivers it as a moſt approved truth, *Florentiæ Civitati imminens, lapides plumbarios habet, qui, ſi excidantur, brevi temporis ſpatio novis incrementis inſtaurantur.* J. Gerhard. *in decade quæſtionum, pag. m.* 22.

Tu ſubtilius ne quæras (ſays *Agricola*, ſpeaking of the growth of mines in general) *ſed tantummodo refer animum ad cuniculos, & conſidera, eos adeò interdum memoriâ hominum in anguſtum veniſſe, ut aliqua ſui parte nullum aut admodum difficilem præbeant tranſitum, cùm eos ſatis latè agere ſoleant foſſores, ne tranſituros impediant. In tales autem anguſtias ſunt adducti propter accretionem materiæ, ex qua lapis eſt factus.*

BUT whether this increment of lead is obſervable in all mines of that metal, I was induced to doubt by the anſwer given me by a gentleman, whoſe houſe was ſeated near ſeveral lead-mines, and who was himſelf owner of one or two, which he yet cauſes to be wrought: for this gentleman, though a chemiſt, aſſured me, that in the country, where he lives, which is divided by the ſea from that of the perſon above-mentioned, he never obſerved the lead-ore to encreaſe, either out of the veins or in them; but that in ſome places, whence ore had been dug thirty or forty, if not fifty years before, he perceived not on the ſides of the paſſages, whence the ore had been dug, that any other had grown in its place, or that the paſſages, though narrow before, were ſenſibly ſtraightened, much leſs blocked up.

AND indeed, if there were no other arguments in the caſe, the ſtraightning of the ancient paſſages in proceſs of time would not convince me. For, when I conſider, that the ſoils, that abound with metals, do uſually alſo abound with waters, which are commonly imbibed by the neighbouring earth; and when I conſider too, that water is ſomewhat expanded by being turned into ice, and that this expanſion is made, (as I have often tried) though ſlowly, yet with an exceeding great force, by which it often ſtretches or breaks the veſſels that contain it: when I conſider theſe things, I ſay, I am apt to ſupect, that ſometimes the encreaſing narrowneſs of the ſubterraneal paſſages in mines may proceed from this, that the ſoil, that invirons them, if they lie not deep, may have the water, imbibed by them, frozen in ſharp winters. By which glaciation, the moiſtened portion of the ſoil muſt forcibly endeavour to expand itſelf, and actually do ſo in the parts contiguous to the paſſage, ſince there it finds no reſiſtance: and though the expanſion made in one year or two be but ſmall, and therefore not obſerved; yet, in a ſucceſſion of many winters, it may by degrees grow to be very conſiderable. But this ſuſpicion I ſuggeſt not, that I would deny the growth of minerals, but to recommend this argument for it to further conſideration. And yet I take this to be a better proof, than what is much relied on by ſome writers of metals, who urge, that in churches, and other magnificent buildings, that are leaded over, the metalline roofs, in a long tract of years, grow far more ponderous, inſomuch, that oftentimes there is a neceſſity to remove them, and exchange them for braſs ones. For though this plauſible argument be urged by ſeveral writers, and among them by the learned *Jo. Gerhardus, pag. m.* 22; yet I fear they proceed upon a miſtake. For having had ſome occaſion to obſerve and enquire after this kind of lead, I ſoon ſuſpected, that the increment of weight, (which ſometimes may indeed be very great) was no clear proof of the real growth of the metal itſelf. For in that, which I had occaſion to conſider, the additional weight, as well as bulk, ſeemed to proceed from acetous or other ſaline corpuſcles of the timber of thoſe buildings, which by degrees exhaling and corroding, that ſide of the lead which they faſtened on, turned it with themſelves into a kind of Ceruſſe: which ſuſpicion I ſhall briefly make probably by noting, 1. That I have found by trial purpoſely made, that woods afford an acid, though not merely acid, liquor, capable of corroding lead. 2. That it is known, that lead turned into Ceruſſe increaſes notably in weight, ſome ſay, (for I had not opportunity to try it) above ſix or ſeven in the hundred. 3. That from the ſheets of lead, that have very long covered churches, and the like buildings, there is often obtained by ſcraping a good proportion of white lead, which I have known much preferred, by an eminent artiſt, to common ceruſſe, when a white pigment was to be employed. And, by the way, men's finding this ceruſſe not on that ſide of the lead, that is expoſed to the outward air, (where I ſcarce ever obſerved any) but on the inſide, that regards the timber and other wooden work, may diſabuſe thoſe, that fancied this ceruſſe to be a part of the lead calcined by the beams of the ſun, that ſtrike immediately upon the metal. And if to this it be added, that by diſtillation and otherwiſe I have found cauſe to ſuſpect, that alabaſter and white marble may emit ſpirituous parts, that will invade lead, it may be doubted, whether what *Galen* relates of the great intumeſcence of leaden bands or faſtenings, wherewith the feet of ſtatues were faſtened to their pedeſtals, be a ſure argument of the real growth of that metal in the air.

BUT I begin to digreſs, and ſeemingly to the prejudice of the particular ſcope of this paper; but yet not to that of one of the main ſcopes of all my phyſical writings, the diſquiſition and advancement of truth.

OBSERVATIONS about the GROWTH of IRON.

I Did not find in one of our chief mines of iron, that there was any notice taken of the growth of that metal; but in another place or two, ſome, that deal in iron-ore, informed me, that they believe it grows, and may be regenerated; and upon that account one of them ſet up a work, contiguous to ſome land of mine, to melt over again the remainder of ore, that had been already wrought (at a great diſtance from that place) and had for ſome ages lain in heaps expoſed to the free air; but with what ſucceſs this chargeable attempt has been made, I am not yet informed.

BUT of the growth of iron in the iſland of *Ilva* or *Elva*, in the Tyrrhene ſea, not far from the coaſt of *Tuſcany*, not only ancient authors, as *Pliny* and *Strabo*, take ſpecial notice, but modern mineraliſts of very good credit, as *Falopius* and *Cæſalpinus*, particularly atteſt the ſame thing; of whom the latter ſpeaks thus: *Vena ferri copioſiſſima eſt in Italia, ob eam nobilitata, Ilva, Tyrreni maris inſula, incredibili copiâ etiam noſtris temporibus eam gignens: nam terra, quæ eruitur, dum vena effoditur, tota procedente tempore in venam convertitur.*

Lib. III. *cap.* 6.

AND the experienced *Agricola* gives us the like account of a place in his country, *Germany*, *In Lygiis*, ſays he, *ad Sagam oppidum in pratis eruitur ferrum, foſſis ad altitudinem bipedaneam actis. Id decennio renatum denuò foditur, non aliter ac Ilvæ ferrum.*

Agric. de Vet. & Nov. Met. lib. II. *cap.* 15.

THE learned *Johan. Gerhardus*, out of a book, which he calls *Conciones Metallicæ*; I ſuppoſe he means the High Dutch Sermons of *Matheſius*, (whoſe language I underſtand not) has this notable paſſage to our preſent purpoſe: *Relatum mihi eſt à metallico foſſore, ad ferrarias, quæ non longè Ambergâ diſtant, terram inanem cum ferri minera erutam, quam vocant* den Gummer, *mixtam cum recrementis ferri, quæ appellatur* der Sinder, *congeſtam in cumulos, inſtar magni cujuſdam valli, ſolibus pluviiſque exponi, & decimo quinto anno denuò excoqui, eliquarique ferrum tantæ tenacitatis, ut ſolæ laminæ inde procudantur.*

J. Gerhard. Profeſſor Tubingenſis, Decad. Quæſt. Phyſico-chymicarum, pag. m. 18.

OBSERVATIONS about the GROWTH of SILVER.

OF the growth, as is ſuppoſed, of ſilver in the form of trees or graſs or other vegetables, I have met with ſome inſtances among mineraliſts, and I have elſewhere mentioned, that an acquaintance of mine ſhewed me a ſtone, wherein he affirmed the ſilver, I ſaw in it, to have increaſed ſince he had it. But for certain reaſons, none of theſe relations ſeem to me very proper to my preſent purpoſe; in order to which, I ſhall therefore ſet down only one inſtance, which I lately met with in a French collection of voyages, publiſhed by a perſon of great curioſity and induſtry, from whoſe civility I received the book. For there, in an account given by a gentleman of his country of a late voyage he made to *Peru*, wherein he viſited the famous ſilver-mines of *Potoſi*, I found a paſſage, which ſpeaks to this ſenſe; *Le meilleur argent, &c.* i. e. The beſt ſilver in all the *Indies*, and the pureſt, is that of the mines of *Potoſi*; the chief have been found in the mountain of *Aranzaſſe*: and (ſome lines being interpoſed) it is added, that they draw this metal even from the mineral earths, that were in times paſt thrown aſide, when the ground was open, and the grooves and ſhafts, that are in the mountains, were made; it having been obſerved, that in theſe recrements, metal had been formed afreſh ſince thoſe times, which ſufficiently ſhews the propenſity of the ſoil to the production of this metal; yet it is true, that theſe impregnated earths yield not ſo much as the ordinary ore, which is found in veins betwixt the rocks.

Voyage du Sieur au Peru, *pag.* 15.

OBSERVATIONS about the GROWTH of GOLD.

AS for the growth of gold, the enquiries I have yet made among travellers give me no great ſatiſfaction about it, and though I have ſpoken with ſeveral, that have been at the coaſt of *Guinea*, and in *Congo*, and other parts of *Afric*, where much gold is to be had; yet I could not learn by them, that they, or any acquaintance of theirs among the natives, had ſeen any mines or veins of gold, (which yet divers authors affirm to be found in more than one kingdom of *Æthiopia*, and in ſome other African Countries.) And having afterwards met with a learned traveller, that had carefully viſited the famous gold-mines of *Cremnitz* in *Hungary*, he anſwered me, that he did not learn from the miners, whether or no the ores of gold, &c. did really grow or were regenerated in tract of time, by being expoſed to the air, or upon any other account; but the grand overſeer, who was lord of part of the ſoil, told him, that he thought the whole mountain to abound with particles of gold, and therefore was wont, when the diggers had almoſt exhauſted the vein, to caſt in ſtore of earth, and dig up other neighbouring places, which, being kept there as in a conſervatory, would afterwards afford gold, as the mine did before.

AND, if a late German profeſſor of phyſick do not miſinform us, his country affords an eminent inſtance of the growth or regeneration of gold. *Nam Corbachi*, ſays he, *quæ eſt civitas Weſtphaliæ, ſub ditione comitis de Iſenborg & Waldeck, aurum excoquitur ex cumulis congeſtis, ita ut ſingulis quadrienniis iterum elaboretur cumulus unus, ſemper ſe reſtaurante naturâ, &c.*

Johan. Gerhardus in Decade Quæſtionum, pag. m. 19.

POSTSCRIPT.

SINCE the ſetting down of the foregoing obſervations, I caſually met with a curious book of travels, lately made by the very ingenious Dr. *Edward Brown*, and finding in page 100, a couple of relations, that ſeem pertinently referable, the one to a paſſage above-cited out of *Agricola*, in the notes about the growth of lead, and the other to the preſent title about the growth of gold; I thought fit to annex them in the learned author's own words, viz.

“ 1. SOME paſſages in this mine cut through the rock, and long diſuſed, have grown up again: and I obſerved the ſides of ſome, which had been formerly wide enough to carry their ore through, to approach each other, ſo as we paſſed with difficulty. This happens moſt in moiſt places; the paſſages unite not from the top to the bottom, but from one to another.

“ 2. THE common yellow earth of the country near *Cremnitz*, eſpecially of the hills towards the weſt, although not eſteemed ore, affords ſome gold: and in one place, I ſaw a great part of an hill digged away, which hath been caſt into the works, waſhed and wrought in the ſame manner as pounded ore, with conſiderable profit.

THE fore-going obſervations about the growth of gold and other metals are not all, that I might, perhaps without being blamed or it, have referred to that title. But all my papers, wherein other obſervations of this kind were ſet down, are not now at hand, and divers other inſtances, that I have met with among writers, of the growth of metals, (taking that expreſſion in the ſenſe I formerly declared) do not ſeem to me ſo pertinent in this place, becauſe the improving ores were not expoſed, nor perchance acceſſible to the air. And even as to the inſtances, that I have now mentioned, till ſeverer obſervations have been made, to determine, whether it be partly the contact or the operation of the air, or ſome internal diſpoſition, analogous to a metalline ſeed or ferment, that cauſes this metalline increment, I dare not be poſitive; though I thought the intereſt of the air in this effect might make it pardonable, to add on this occaſion to the hiſtory of nature ſome particulars, of which the cauſe conjecturally propoſed may be probable enough to countenance a ſuſpicion, till further experience have more clearly inſtructed us.

TO what has been ſaid of the growth of metals in the air, I add ſome inſtances of the growth of foſſile ſalts, and of ſome other minerals: but, beſides that theſe belong to the paper about the ſaltneſs of the air; what has been already ſaid may ſuffice for the preſent occaſion.

POSTSCRIPT.

AFTER what I writ in the 446 page of the following diſcourſe, having an opportunity to look again upon the marchaſite there mentioned to have been hermetically ſealed up after its ſurface had been freed from the grains of vitriolate ſalt, that adhered to it, I perceived, that notwithſtanding the glaſs had been ſo cloſely ſtopped, yet there plainly appeared from the outſide of the maſs ſome grains of an effloreſcence, whoſe colour between blue and green, argued it to be of a vitriolate nature. If this be ſeconded with other trials made with the like ſucceſs, it may ſuggeſt new thoughts about the growth of metals and minerals, eſpecially with reference to the air.

SUSPICIONS

ABOUT

Some Hidden QUALITIES in the AIR.

BESIDES the four firſt qualities of the air, (heat, cold, dryneſs and moiſture) that are known even to the vulgar; and thoſe more unobvious, that philoſophers and chemiſts have diſcovered, ſuch as gravity, ſpringineſs, the power of refracting the beams of light, &c. I have often ſuſpected, that there may be in the air ſome yet more latent qualities or powers differing enough from all theſe, and principally due to the ſubſtantial parts or ingredients, whereof it conſiſts. And to this conjecture I have been led, partly (though not only, or perhaps chiefly) by conſidering the conſtitution of that air we live and breathe in, which, to avoid ambiguities, I elſewhere call Atmoſpherical air. For this is not, as many imagine, a ſimple and elementary body, but a confuſed aggregate of effluviums from ſuch differing bodies, that, though they all agree in conſtituting, by their minuteneſs and various motions, one great maſs of fluid matter, yet perhaps there is ſcarce a more heterogeneous body in the world.

AND as by air I underſtand not, (as the Peripateticks are wont to do) a mere elementary body; ſo, when I ſpeak of the qualities of the air, I would not be thought to mean ſuch naked and abſtracted beings (as the ſchools often tell us of,) but ſuch as they call qualities *in concreto*, namely, corpuſcles endued with qualities, or capable of producing them in the ſubjects they invade and abound in.

In a paper about ſubterraneal ſteams.

I have elſewhere ſhewn it to be highly probable, that, beſides thoſe vapours and exhalations, which by the heat of the ſun are elevated into the air, and there afford matter to ſome meteors, as clouds, rain, parhelions and rainbows, there are, at leaſt at ſome times, and in ſome places, ſtore of effluviums emitted from the ſubterraneal parts of the terreſtrial globe; and it is no leſs probable, (from what I have there and elſewhere delivered) that in the ſubterraneal regions there are many bodies, ſome fluid and ſome conſiſtent, which, though of an operative nature, and like, upon occaſion, to emit ſteams, ſeldom or never appear upon the ſurface of the earth, ſo that many of them have not ſo much as names aſſigned them even by the mineraliſts. Now, among this multitude and variety of bodies, that lie buried out of our ſight, who can tell, but that there may be ſome, if not many, of a nature very differing from thoſe we are hitherto familiarly acquainted with; and that, as divers wonderful and peculiar operations of the loadſtone, (though a mineral many ages ago famous among philoſophers and phyſicians) were not diſcovered till of later ages, wherein its nobler virtues have been diſcloſed; ſo there may be other ſubterraneous bodies, that are endowed with conſiderable powers, which, if they were known, be found very differing from thoſe of the foſſiles we are wont to deal with?

I alſo further conſider, that (as I have elſewhere endeavoured to make it probable) the ſun and planets (to ſay nothing of the fixed ſtars) may have influences here below diſtinct from their heat and light. On which ſuppoſition it ſeems not abſurd to me to ſuſpect, that the ſubtil, but corporeal, emanations even of theſe bodies may (ſometimes at leaſt) reach to our air, and mingle with thoſe of our globe in that great receptacle or rendeſvous of celeſtial and terreſtrial effluviums, the atmoſphere. And if this ſuſpicion be not groundleſs, the very ſmall knowledge we have of the ſtructure and conſtitution of globes, ſo many thouſands or hundreds of thouſands of miles remote from us, and the great ignorance we muſt be in of the nature of the particular bodies, that may be preſumed to be contained in thoſe globes, (as minerals and other bodies are in the earth) which in many things appear of kin to thoſe that we inhabit, (as with excellent teleſcopes I have often with attention and pleaſure obſerved, particularly in the moon) this great imperfection, I ſay, of our knowledge may keep it from being unreaſonable to imagine, that ſome, if not many, of thoſe bodies and their effluxions, may be of a nature quite differing from thoſe we take notice of here about us, and conſequently may operate after a very differing and peculiar manner.

And though the chief of the heteroclite effluviums, that endow the air with hidden qualities, may probably proceed from beneath the ſurface of the earth, and from the celeſtial bodies; yet I would not deny, but that, eſpecially at ſome times, and in ſome places, the air may derive multitudes of efficacious particles from its own operations, acting as a fluid ſubſtance upon that vaſt number and variety of bodies, that are immediately expoſed to it. For, though, by reaſon of its great thinneſs, and of its being in its uſual ſtate devoid both of taſte and ſmell, it ſeems wholly unfit to be a menſtruum; yet I am not ſure but it may have a diſſolving, or at leaſt a conſuming, power on many bodies, eſpecially ſuch as are peculiarly diſpoſed to admit its operations.

For I conſider, that the air has a great advantage by the vaſt quantity of it, that may come to work in proportion to the bodies that are expoſed to it; and I have long thought, that, in divers caſes, the quantity of a menſtruum may much more conſiderably compenſate its want of ſtrength, than chemiſts are commonly aware of, (as there may be occaſion elſewhere to exemplify.) And there are liquors, which paſs for inſipid, (and are therefore thought to be altogether unfit to be ſolvents,) which, though they have their active parts too thinly diſperſed to be able preſently to make ſenſible impreſſions upon our organs of taſting, yet are not quite deſtitute of corpuſcles fit to act as a ſolvent; eſpecially if they have time enough to make with the other parts of the fluid ſuch numerous and various motions, as muſt bring, now ſome of them, and then others, to hit againſt the body expoſed to them. Which may be illuſtrated by the ruſt like to verdigreaſe, which we have obſerved in copper, that has been long expoſed to the air, whoſe ſaline particles, little by little, do, in tract of time, faſten themſelves in ſuch numbers to the ſurface of the metal as to corrode it, and produce that efflореſcence coloured like verdigreaſe which you know is a factitious body, wont to be made of the ſame metal, corroded by the ſharp corpuſcles of vinegar, or of the huſks of grapes: beſides, that by the power, which mercury has to diſſolve gold and ſilver, it appears, that it is not always neceſſary for the making a fluid fit to be a diſſolvent, that it it ſhould affect the taſte. And as to thoſe bodies, on which the aerial menſtruum can, though but ſlowly, work, the greateſt quantity of it may bring it this advantage, that, whereas even the ſtrongeſt menſtruums, if they bear no great proportion in bulk to the bodies they are to work on, are eaſily glutted, and being unable to take up any more, are fain to leave the reſt of the body undiſſolved, our aerial menſtruum bears ſo vaſt a proportion to the bodies expoſed to it, that when one portion of it has impregnated itſelf as much as it is able, there may ſtill come freſh and freſh to work further on the remaining part of the expoſed body.

Besides the ſaline and ſulphureous particles, that, at leaſt in ſome places, may (as I have elſewhere ſhewn) impregnate the air, and give it a greater affinity to chemical menſtruums more ſtrictly ſo called; I am not averſe from thinking, that the air, merely as a fluid body, that conſiſts of corpuſcles of differing ſizes and ſolidities reſtleſly and very variouſly moved, may upon the account of theſe corpuſcles be ſtill reſolving, or preying upon the particles of the bodies, that are expoſed to their action. For many of thoſe aerial corpuſcles, ſome hitting and ſome rubbing themſelves every minute againſt thoſe particles of expoſed bodies, that chance to lie in their way, may well, by thoſe numerous occurſions and affrictions, ſtrike off and carry along with them now ſome, and then others of thoſe particles; as you ſee it happens in water, which, as ſoft and fluid as it is, wears out ſuch hard and ſolid bodies as ſtones themſelves, if it often enough meet them in its paſſage, according to the known ſaying,

Gutta cavat lapidem non vi, ſed ſæpe cadendo.

And though the aerial corpuſcles be very minute, and the bodies expoſed to them oftentimes large and ſeemingly ſolid; yet this needs not make you reject our ſuppoſition, becauſe it is not upon the whole body at once, that, according to us, the aerial corpuſcles endeavour to work, but upon the ſuperficial particles, which may often be more minute than thoſe corpuſcles; as you will the more eaſily believe, if you firſt obſerve with a good microſcope, how many extant particles may be met with on the ſurface of bodies, that to the naked eye ſeem very ſmooth, and even of thoſe, that are poliſhed by art with tripoli or puttee; and then conſider, that one of theſe protuberances, being yet manifeſtly viſible, may well be preſumed to conſiſt of a multitude of leſſer particles, divers of which may very well be as minute as thoſe aerial corpuſcles, that ſucceſſively hit againſt them, and endeavour to carry them along with themſelves. And this may be illuſtrated by a familiar inſtance. For if you take a lump of loaf ſugar, or even of a much ſolider and harder body, ſal gemmæ, and caſt it into common water, though this liquor is inſipid, and the motions of its corpuſcles but very languid; yet theſe corpuſcles are capable to looſen and carry off the ſuperficial particles of ſugar or ſalt, that chance to lye in their way, and freſh corpuſcles of water ſtill ſucceeding to work upon the remaining particles of the expoſed body, that ſtands in their way, the whole lump is, by and little, diſſolved, and ceaſes to appear to the eye a thing diſtinct from the liquor.

Some things, that have occurred me, to have made me ſuſpect, that it is not impoſſible, but that ſome bodies may receive a diſpoſition to volatility, and conſequently to paſs into the air by the action either of the ſun-beams, in the form of ſun-beams, or of ſome ſubſtance, that once iſſued out of the ſun, and reached unto the air. For there may be certain bodies for the moſt part in the form of liquors, which though they paſs off from ſome peculiarly diſpoſed bodies, may during their ſtay or contact produce in them a great and ſtrange aptneſs to be volatized. In favour of which conjecture, I might here alledge both the effects, which the Paracelſians and Helmontians aſcribe to the Alkaheſt, of volatizing even fixed and ponderous bodies barely by being often abſtracted from them, and ſome other things, which I ſhall now leave unmentioned, becauſe you may find them in my notes about Volatility and Fixity.

But whatever become of this conjecture, it is conſonant to experience, that, either upon the above recited accounts, or alſo ſome others, thoſe parts of the atmoſphere, which, in a ſtricter ſenſe, may be called the air, are, at leaſt, in ſome places, ſo intermixed with particles of differing kinds, that among that great number of various ſorts of them, it is very likely that there ſhould be ſome of an uncommon and unobſerved nature. And I could countenance what has been ſaid by the waſting of odorous bodies, and eſpecially camphire, and by repreſenting, that I have obſerved ſome ſolid bodies actually cold, when their ſuperficial parts were newly taken off, to emit, though inviſibly, ſuch copious ſteams into the air, as to grow continually and manifeſtly lighter upon the balance, ſo as to ſuffer a notable decrement of weight in a minute of an hour. But the mention I make of ſuch things in another paper, diſſuades me from inſiſting on them here, where it will be ſeaſonable to reſume the diſcourſe, which the mention of the diſſolving power, that may be gueſſed to be in the air, has for ſome pages interrupted, and to tell you, that thoſe propounded, before I entered upon the digreſſion, are the two main conſiderations *à priori* (as they ſpeak) whereon I have grounded my ſurmiſe, which being propoſed but as a ſuſpicion, I preſume it will not be expected, that the argument *à poſteriori*, which I ſhall bring to countenance it, ſhould be more than conjectures, much leſs, that they ſhould be demonſtrations. And therefore I ſhall venture to lay before you ſome few phænomena, which ſeem to be at leaſt as probably referable to ſome latent quality in the air, as to any other cauſe I yet know. Upon which ſcore ſuch phænomena may be allowed to be pleaded in favour of our ſuſpicion, until ſome other certain cauſe of them ſhall be ſatisfactorily aſſigned.

HAVING premiſed thus much to keep you from looking for ſtronger proofs than I think my taſk obliges me to give; the firſt phænomenon, I ſhall propoſe, ſhall be the appearing or growth of ſome ſalts in certain bodies, which we obſerved to afford them either not at all, or at leaſt nothing near in ſuch plenty, or ſo ſoon, unleſs they be expoſed to the air. Of ſuch a phænomenon as this, that is not ſo much as mentioned by vulgar philoſophers, and very rarely, if at all, to be met with in the laboratories of chymiſts, you will not, I ſuppoſe, wonder, that I do not preſent you many examples, and ſome few I am able to name. For I remember, that ſuſpecting a ſolid marchaſite, hard as ſtone, to be fit to be made an inſtance for my purpoſe, I cauſed it to be broken, that the internal more ſhining parts might be expoſed to the air; but, though this were done in a room, where a good fire was uſually kept, ſo that the marchaſite was not only ſheltered from the rain, but kept in a dry air, yet after a while I diſcovered upon the gliſtering parts an effloreſcence of a vitriolate nature.

AND afterwards meeting with a ponderous and dark coloured mineral, and which, at the firſt breaking, diſcovered to the eye no appearance of any ſalt, nor ſo much as any ſhining marchaſitical particles, we found nevertheleſs, that a good quantity of theſe hard and heavy bodies, being kept expoſed to the air, even in a room, that preſerved them from the rain, though probably they had lain many ages entire in the hill, wherein they were found under ground; yet in not many months, by the operation of the air upon them, they were, in great part, crumbled to powder exceeding rich in copperas. Nay, I remember, that having for curioſity ſake, laid up ſome of theſe ſtones in a room, where I conſtantly kept fire, and in the drawer of a cabinet, which I did not often take out to give them freſh air, ſome, if not moſt of them, were notwithſtanding covered with a copious effloreſcence, which by its conſpicuous colour between blue and green, by its taſte, and by its fitneſs to make in a trice an inky mixture with infuſion of galls, ſufficiently manifeſted itſelf to be vitriol; whoſe growth by the help of the contact of the air is the more conſiderable, becauſe it is not a meer acid ſalt, but abounds in ſulphureous and combuſtible parts, which I have divers times been able, by methods elſewhere mentioned, actually to ſeparate or obtain from common vitriol without the addition of any combuſtible body, and ſometimes without any additament at all. It was alſo uncommon, that our blackiſh minerals required no longer time, nor no rain, to make them afford their vitriolate effloreſcences: for I remember, I kept many of thoſe marchaſites, both glittering ones and others, of which they make and ſell great quantities of vitriol at *Deptford*, without perceiving in them a change, that came any thing near to what I have recited. And I obſerved thoſe, whoſe trade it is to make vitriol, to be often obliged to let their vitriol-ſtones, as they call them, lie half a year, or even eighteen months, or two years expoſed, not only to the open air, but to the rain and ſun, to be able to obtain from them their vitriolate parts.

THAT alſo the earth or ore of allum, being robbed of its ſalt, will in tract of time recover it by being expoſed to the air, we are aſſured by the experienced *Agricola*, where, having delivered the way of making allum, he ſubjoins this advertiſement: *Terra aluminoſa, quæ in caſtellis diluta, poſtquam effluxit, ſuperfuit egeſta & coacervata quotidie, rurſus magis & magis fit aluminoſa, non aliter atque terra, ex qua halinitrum fuit confectum, ſuo ſucco plenior fit; quare denuo in caſtella conjicitur, & aquæ affuſæ ea percolantur.*

I have likewiſe obſerved, as you alſo perchance have done, that ſome kind of lime in old walls and moiſt places has gained in length of time a copious effloreſcence, very much of nitrous nature; as I was convinced by having obtained ſalt-petre from it by barely diſſolving it in common water, and evaporating the filtrated ſolution: and, that in calcined vitriol, whoſe ſaline parts have been driven away by the violence of the fire, particles of freſh ſalt may be found, after it has lain a competent time in the air, I ſhall before long have occaſion to inform you.

BUT in the mean time, (to deal ingenuouſly with you,) I ſhall confeſs to you, that though theſe and the like obſervations have ſatiſfied learned men, without having been called in queſtion, and conſequently have, at leaſt, probability enough to ground our ſuſpicion upon; yet I, that am more concerned for the diſcovery of a truth than the reputation of a paradox, propoſe the argument drawn from the foregoing obſervations, but as a probationer. For it yet ſeems to me ſomewhat doubtful, whether the ſalts, that appear in the forementioned caſes, are really produced by the operation of the air working as an agent, or alſo concurring as an ingredient ; or whether theſe ſaline ſubſtances be not the production of ſome internal thing, that is analagous to a ſeminal principle, which makes in theſe bodies a kind of maturation of ſome parts, which being once ripened, and perhaps aſſiſted by the moiſture of the air, diſcloſe themſelves in the form of ſaline concretions ; as in the feculent or tartareous parts of many wines, there will in tract of time be generated or produced ſtore of corpuſcles of a ſaline nature, that produce the acid taſte we find in tartar, eſpecially, that of rheniſh wine. It may alſo be ſuſpected, that the formerly mentioned ſalts found in marchaſites, in nitrous and aluminous earths, &c. are made by the ſaline particles of the like nature, that among multitudes of other kinds ſwim in the air, and are attracted by the congenerous particles, that yet remain in the terreſtrial bodies, that are, as it were, the wombs of ſuch minerals, (as I have elſewhere ſhewn, that the ſpirit of nitre will, with fixed nitre and ſome other alkalies, compoſe ſalt-petre ;) or elſe, that theſe aerial ſalts, if I may ſo call them, aſſiſted by the moiſture of the air, do ſoften and open, and almoſt corrode or diſſolve the more terreſtrial ſubſtance of theſe wombs, and thereby ſollicit out and ſomewhat extricate the latent ſaline particles, and, by their union with them, compoſe thoſe emerging bodies, that reſemble vitriol, allum, &c.

But not only to ſuggeſt theſe ſcruples, as if I had a mind they ſhould but trouble you, and keep you irreſolute, I ſhall propound ſomething towards the removal of them ; namely, that a convenient quantity of nitrous earth, or that other of thoſe ſubſtances, which you would examine, be kept in a cloſe veſſel to which the air has not acceſs, for at leaſt as long time as has been obſerved to be ſufficient to impregnate the like ſubſtance, or rather a portion of the ſame parcel, that was choſen to be included : for if the body, that was kept cloſe, have either gained no ſalt all, or very much leſs in proportion to its bulk than that, which was kept expoſed, we may thence eſtimate, what is to be aſcribed to the air in the production of nitre or other ſaline concretions. And, becauſe I have obſerved none of theſe bodies, that would ſo ſoon, and ſo manifeſtly even to the eye, diſcloſe a ſaline ſubſtance, as the blackiſh vitriol-ore, I lately told you I kept in a drawer of my cabinet ; I judged, that a very fit ſubject, wherewith to try, what maturation, or time, when the air was ſecluded, would perform towards the deciding of our difficulty : and accordingly having taken ſome fragments of it, which we had carefully freed from the adhering vitriolate eſſloreſcence, by whoſe plenty we are aſſured, that it was very well diſpoſed to be wrought on by the air, we put of theſe fragments of differing ſizes into two conveniently ſhaped glaſſes, which being hermetically ſealed were ordered to be carried away, and kept in fixed places ; by which means it was expected, that, even without opening the glaſſes, we ſhould be able eaſily to ſee by the changed colour of the ſuperficial parts, whether any vitriolate eſſloreſcence were produced ; but, through the negligence or miſtake of thoſe, to whom the care was recommended, the experiment was never brought to an iſſue ; and though I afterwards got more of the mineral, and made a ſecond trial of the ſame, I have not yet been informed of the event.

But, Sir, though, until the ſucceſs of ſome trial be known, I dare not too confidently pronounce about the production or regeneration of ſalts in bodies, that have been robbed of them, and aſcribe it wholly to the air ; yet, when I conſider the ſeveral and great effects of the air upon divers other bodies, I think it not raſh to conjecture, in the mean time, that the operations of the air may have a conſiderable ſhare in theſe phænomena, and ſo that there may be latent qualities in the air, in the ſenſe I declared above, where I told you, that when I ſpeak of theſe qualities, I look upon them *in concreto*, (as they phraſe it,) together with the ſubſtances or corporeal effluvia they reſide in ; and of theſe aerial qualities, taken in this ſenſe, I ſhall now proceed to mention ſome other inſtances.

The difficulty we find of keeping flame and fire alive, though but for a little time, without air, makes me ſometimes prone to ſuſpect, that there may be diſperſed through the reſt of the atmoſphere ſome odd ſubſtance, either of a ſolar, or aſtral, or ſome other exotic nature, on whoſe account the air is ſo neceſſary to the ſubſiſtence of flame ; which neceſſity I have found to be greater, and leſs dependent upon the manifeſt attributes of the air, than naturaliſts ſeem to have obſerved. For I have found by trials purpoſely made, that a ſmall flame of a lamp, though fed perhaps with a ſubtil thin oyl, would in a large capacious glaſs-receiver expire, for want of air, in a far leſs time than one would believe. And it will not much leſſen the difficulty to alledge, that either the groſs fuliginous ſmoak did in a cloſe veſſel ſtifle the flame, or, that the preſſure of the air is requiſite to impel up the aliment into the wieck : for, to obviate theſe objections, I have in a larger receiver imployed a very ſmall wieck with ſuch rectified ſpirit of wine, as would in the free air burn totally away ; and yet, when a very ſmall lamp, furniſhed (as I was ſaying) with a very ſlender wieck, was made to burn, and, filled with this liquor, was put lighted into a large receiver, that little flame, though it emitted no viſible ſmoak at all, would uſually expire within about one minute of an hour, and, not ſeldom, in a leſs time ; and this, though the wieck was not ſo much as ſinged by the flame : nor indeed is a wieck neceſſary for the experiment, ſince highly rectified ſpirit of wine will in the free air flame away well without it. And indeed it ſeems to deſerve our wonder, what that ſhould be in the air, which enabling it to keep flame alive, does yet, by being conſumed or depraved, ſo ſuddenly render the air unfit to make flame ſubſiſt. And it ſeems by the ſudden waſting or ſpoiling of this fine ſubject, whatever it be, that the bulk of it is but very ſmall in proportion to the air it impregnates with its virtue. For after the extinction of the flame, the air in the receiver was not viſibly altered, and, for aught I could perceive by the ways of judging I had then at hand, the air retained either all, or at leaſt far the greateſt part of its elaſticity, which I take to be its moſt genuine and diſtinguiſhing property.

AND this undeſtroyed ſpringineſs of the air ſeems to make the neceſſity of freſh air to the life of hot animals, (few of which, as far as I can gueſs after many trials, would be able to live two minutes of an hour, if they were totally and all at once deprived of air,) ſuggeſt a great ſuſpicion of ſome vital ſubſtance, if I may ſo call it, diffuſed through the air, whether it be a volatile nitre, or (rather) ſome yet anonimous ſubſtance, ſydereal or ſubterraneal, but not improbable of kin to that, which I lately noted to be ſo neceſſary to the maintenance of other flames.

I know not, whether you will think it pertinent to our preſent diſcourſe, that I obſerve to you, that by keeping putrifying bodies in glaſſes, which by *Hermes* his ſeal were ſecured from the contact of the external air, I have not been able to produce any inſect, or other living creature, though ſometimes I have kept animal ſubſtances, and even blood ſo included, for many months, and one or two of them for a longer time; and though all theſe ſubſtances had a manifeſt change made in their conſiſtence whilſt they remained ſealed up.

ON this occaſion I ſhall add an odd obſervation, that I met with in a little diſſertation *de admirandis Hungariæ aquis*, written by an anonymous, but ingenious nobleman of that country, where, ſpeaking of the native ſalt, that abounds in their regions, he ſays, that in the chief mine (by them called *Deſienſis*) of *Tranſylvania*, there was, a few years before he writ, a great oak, like a huge beam, dug out of the middle of the ſalt; but, though it was ſo hard, that it would not eaſily be wrought upon by iron tools, yet being expoſed to the air out of the mine, it became ſo rotten, as he expreſſes it, that in four days it was eaſy to be broken, and crumbled between one's fingers. And of that corruptive or diſſolutive power of air near thoſe mines, the ſame author mentions other inſtances.

HAVING found an antimonial preparation to procure vomits, in a caſe where I did not at all expect it, I was afterwards curious to enquire of ſome phyſicians and chymiſts, that were of my acquaintance, whether they had not taken notice, that Antimonium Diaphoreticum, which, as its name imports, is wont to work by ſweat or tranſpiration, would not become vomitive, if it were not kept from the air? To which one phyſician, that was a learned man, aſſured me, it would, as he had found by particular trials: and the like anſwer has been given me by more than one. And I find, that the experienced *Zwelfer* himſelf does ſomewhere give a caution againſt letting the air have acceſs to theſe antimonial medicines, leſt it ſhould render them, as he ſays it will, in tract of time, not only emetic, but diſpoſed to produce heart-burnings, (as they call them,) faintings, and other bad ſymptoms. And I learned by enquiry, from a very ingenious doctor of phyſic, that, having carefully prepared Antimonium Diaphoreticum, he gave many doſes whilſt it was freſh and kept ſtopped in a glaſs, (without finding, that in any patient it procured ſo much as one vomit,) but having kept a parcel of the ſelf-ſame remedy for a pretty while in a glaſs only covered looſely with a paper, the medicine, vitiated by the air, proved emetic (ſtrongly enough) to thoſe, who neither by conſtitution, or foulneſs of ſtomach, or on any other diſcernible account, were more than others, that had taken it diſpoſed to vomit. By which obſervations, and from what I formerly told you of the ſalt-petre obtainable from quick-lime, a man partial to the air would be made forward to tell you, that this looks, as if either there were in the air a ſubſtance diſpoſed to be aſſimilated by all kinds of bodies, or that the air is ſo vaſt and rich a rendezvouz of innumerable ſeminal corpuſcles, and other analogous particles, that almoſt any body long expoſed to it may there meet with particles of kin to it, and fit to repair its wrongs and loſſes, and reſtore it to its natural condition. But without taking any further notice of this odd ſurmize, I will proceed to mention two or three other phænomena of nature, that ſeem to favour the ſuſpicion, that there may be ſecret qualities in the air, in reference to ſome bodies.

THE ingenious Monſieur *de Rocheford*, in the handſome account he gives of the apple, or fruit of the tree junipa, whoſe juice is employed by the Indians to black their ſkins, that they may look the more terrible to their enemies, obſerves, that, though the ſtain, or, as he ſpeaks, the tincture of this fruit cannot be waſhed out with ſoap, yet, within nine or ten days, it will vaniſh of itſelf; which would make one ſuſpect, that there may be in the air ſome ſecret powerful ſubſtance, that makes it a menſtruum of more efficacy than ſoap itſelf to obliterate ſtains I remember, I have ſeen this fruit, but not whilſt it was ſucculent enough to have a trial made with it; which I was therefore troubled at, becauſe the author does not clearly expreſs, whether this diſappearing of the tincture happens indifferently to the bodies it chances to ſtain, or only is obſerved on the ſkins of men. For, as in the former caſe, it will afford an inſtance pertinent to our preſent purpoſe; ſo in the latter I ſhould ſuſpect, that the vaniſhing of the tincture may be due, not ſo much to the operation of the air upon it, as to the ſweat and exhalations of a human body, which abounding with volatile ſalt, may either deſtroy or carry off with them, the coloured particles they meet with in their paſſages.

I have ſometimes, not altogether without wonder, obſerved the excellency of the better ſort of Damaſco-ſteel, (for I ſpeak not of all that goes under that name,) in compariſon of ordinary ſteel. And, beſides what I have elſewhere taken notice of concerning it, there is one phænomenon, which though I am not ſure it belongs to the latent qualities of the air, yet, becauſe it may well do ſo, and I am unwilling it ſhould be loſt, I will here tell you, that having enquired of an eminent and experienced artificer, (whom I long ſince employed in ſome difficult experiments,) about the properties of Damaſco-ſteel, this honeſt and ſober man averred to me, that when he made inſtruments of it, and gave them the true temper, which is ſomewhat differing from that of other ſteel, he generally obſerved, that though, when raſors or other inſtruments made of it were newly forged, they would be ſometimes no whit better, and ſometimes leſs good, than thoſe made of other ſteel; yet when they had been kept a year or two or three in the air, though nothing elſe were done to improve them, they would be found much to ſurpaſs other inſtruments of the ſame kind, and what themſelves were before; in ſo much, that ſome of them have been laid aſide at firſt, as no way anſwering the great expectation conceived of them, which after two or three years were found to ſurpaſs it; of which alſo I am now making a trial. I have ſeveral times made a ſubſtance, that conſiſts chiefly of a metalline body, and is of a texture cloſe enough to lie for many hours undiſſolved in a corroſive menſtruum; and yet this ſubſtance, that was fixed enough to endure the being melted by the fire without loſing its colour, would, when I had purpoſely expoſed it to the air, be diſcoloured in a very ſhort time, and have its ſuperficial parts turned almoſt black.

And this brings into my mind that very pretty obſervation, that has been newly made in *Italy* by an ingenious man, who took notice, that, if after the opening of a vein, the blood be kept till it be concreted, and have excluded the ſuperficial ſerum, though the lower part be uſually of a dark and blackiſh colour, in compariſon of the ſuperficial parts, and therefore be counted far more feculent; yet, if the lump, or clot of blood be broken, and the internal, and dark coloured parts of the blood be expoſed to the air, it will after a time (for it is not ſaid how long) be wrought on by the contact of the air, that the ſuperficial part of the blood will appear as florid, as the lately mentioned upper part (ſuppoſed to be, as it were, the flower of the blood,) did ſeem before. And this obſervation I found to hold in the blood of ſome beaſts, whereon I tried it, in which I found it to ſucceed in much fewer minutes, than the Italian virtuoſo's experiment on human blood would make me expect.

On the other ſide I have often prepared a ſubſtance, whoſe effect appears quite contrary to this. For, though this factitious concrete, whilſt kept to the fire, or very carefully preſerved from the air, be of a red colour, almoſt like the common opacous bloodſtone of the ſhops; yet, if I broke it, and left the lumps, or fragments of it, a little while in the air, it would in a ſhort time (ſometimes perhaps, not amounting to a quarter of an hour) it would, I ſay, have its ſuperficial part turned of a dark colour, very little, and ſometimes ſcarce at all, ſhort of blackneſs.

A very inquiſitive perſon of my acquaintance, having occaſion to make, by diſtillation, a medicine of his own deviſing, chanced to obſerve this odd property in it, that, at that time of the year, if it were kept ſtopped, it would be coagulated almoſt like oil of anniſeeds in cold weather; yet, if the ſtopple were taken out, and ſo acceſs were for a while given to the air, it would turn to a liquor, and the veſſel being again ſtopped, it would, though more ſlowly, recoagulate. The hints, that I gueſſed, might be given by ſuch a phænomenon, making me deſirous to know ſomething of it more than barely by relation, I expreſſed rather a curioſity than diffidence about it; and the maker of it telling me, he thought, he had in a ſmall vial about a ſpoonful of this medicine left in a neighbouring chamber, I deſired his leave to conſider it myſelf, which requeſt being preſently complied with, I found it, when he brought it into the room which I ſtayed in, not liquid but conſiſtent, though of but a ſlight and ſoft contexture. And having taken out the cork, and ſet the vial in a window, which (if I well remember) was open, though the ſeaſon, which was winter, was cold, yet in a little time, that I ſtayed talking with the chemiſt, I found, that the ſo lately coagulated ſubſtance was almoſt all become fluid. And another time, when the ſeaſon was leſs cold, having occaſion to be where the vial was kept well ſtopped, and caſting my eyes on it, I perceived the included ſubſtance to be coagulated much like oil of anniſeeds. And this ſubſtance having, as the maker aſſured me, nothing at all of mineral in it, nor any chemical ſalt, it conſiſting only of two ſimple bodies, the one of a vegetable, and the other of an animal ſubſtance, diſtilled together, I ſcarce doubt but you will think with me, that theſe contrary operations of the air, which ſeems to have a power in ſome circumſtances to coagulate ſuch a body, and yet to diſſolve and make it fluid, when freſh and freſh parts are allowed acceſs to it, may deſerve to be further reflected on, in reference (among other things) to the opportune operations, the inſpired air may have on the conſiſtence and motion of the circulating blood, and to the diſcharge of the fuliginous recrements to be ſeparated from the blood in its paſſage through the lungs.

There are two other phænomena, that ſeemed favourable to our ſuſpicion, that there are anonymous ſubſtances and qualities in the air, which ought not to be altogether prætermitted on this occaſion; though, becauſe to ſpeak fully of them would require far more time than I can now ſpare, I ſhall ſpeak of them but ſuccinctly.

The latter of theſe two phænomena is the growth or appearing production of metals or minerals dug out of the earth, and expoſed to the air. And this, though it be the laſt of the two, I mention firſt, becauſe it ſeems expedient, leſt it ſhould prove too long an interruption to our diſcourſe, to poſtpone the obſervations, and annex them to the end of this paper; only intimating to you now, that the caution I formerly interpoſed about the regeneration of ſalts in nitrous, and other earths, may, for greater ſecurity, be applied, *mutatis mutandis*, to that production of metalline and mineral bodies we are ſpeaking of.

THE other of the two phænomena, I lately promiſed to mention, is afforded me by thoſe various and odd diſeaſes, that at ſome times, and in ſome places, happen to invade, and deſtroy numbers of beaſts, ſometimes of one particular kind, and ſometimes of another. Of this we have many inſtances in the books of approved authors, both phyſicians and others; and I have myſelf obſerved ſome notable examples of it. But yet I ſhould not mention it as a ground of ſuſpicion, that there may be, in ſome times and places, unknown effluvia and powers in the air, but, that I diſtinguiſh theſe from thoſe diſeaſes of animals, that proceed, as the rot in ſheep often does, from the exorbitancy of the ſeaſons, the immoderateneſs of cold, heat, or any other manifeſt quality in the air. And you will eaſily perceive, that ſome of theſe examples probably argue, that the ſubterraneal parts do ſometimes (eſpecially after earthquakes, or unuſual cleavings of the ground) ſend up into the air peculiar kinds of venomous exhalations, that produce new and mortal diſeaſes in animals of ſuch a ſpecies, and not in thoſe of another, and in this or that particular place, and not elſewhere: of which we have an eminent inſtance in that odd plague or murrain of the year 1514, which, *Fernelius* tells us, invaded none but cats. And even in animals of the ſame ſpecies, ſometimes one ſort have been incomparably more obnoxious to the plague than another; as *Dionyſius Halicarnaſſeus* mentions a plague, that attacked none but maids; whereas, the peſtilence, that raged in the time of *Gentilis* a famed phyſician, killed few women, and ſcarce any but luſty men. And ſo *Boterus* mentions a great plague, that aſſaulted almoſt only the younger ſort of perſons, few paſt thirty years of age being attacked by it: which laſt obſervation has been alſo made by ſeveral later phyſicians. To which may be added, what learned men of that faculty have noted at ſeveral times concerning plagues, that particularly invaded thoſe of this or that nation, though confuſedly mingled with other people; as *Cardan* ſpeaks of a plague at *Baſil*, with which only the Switzers, and not the Italians, French, or Germans, were infected. And *Johannes Utenhovius* takes notice of a cruel plague at *Copenhagen*, which, though it raged among the Danes, ſpared both the Engliſh, Dutch, and Germans, though they freely entered infected houſes, and were not careful to ſhun the ſick. In reciting of which inſtances I would not be underſtood, as if I imputed theſe effects merely to noxious ſubterraneal fumes; for I am far from denying, that the peculiar conſtitutions of men's bodies are likely to have a great intereſt in them: but yet it ſeems leſs probable, that the peſtilent venom diffuſed through the air ſhould owe its enormous and fatal efficacy to the exceſs of the manifeſt qualities of the air, than to the peculiar nature of the peſtilential poiſon ſent up into the air from underground, which when it is by dilution or diſſipation enervated, or by its progreſs paſt beyond the air we breathe in, or rendered ineffectual by ſubterraneal, or other corpuſcles, of a contrary quality, the plague, which it, as a con-cauſe, produced, either quite ceaſes, or degenerates into ſomewhat elſe. But I have not time to countenance this conjecture, much leſs to conſider, whether ſome of thoſe diſeaſes, that are wont to be called new, which either did begin to appear, or at leaſt to be riſe, within theſe two or three centuries, as the *Sudor Anglicus* in the fifteenth century, the ſcurvy, and the *Morbus Hungaricus*, the *Lues Moraviæ*, *Novus Morbus Luneburgenſis*, and ſome others, in the laſt century of all, may be in part cauſed by the exotic ſteams this diſcourſe treats of. But this conſideration I willingly reſign to phyſicians.

AND now, if the two forementioned ſuſpicions, the one about ſubterraneal, the other about ſidereal, effluviums, ſhall prove to be well grounded, they may lead us to other ſuſpicions and further thoughts about things of no mean conſequence; three of which I ſhall venture to make mention of in this place.

I. FOR we may hence be awakened to conſider, whether divers changes of temperature and conſtitution in the air, not only as to manifeſt qualities, but as to the more latent ones, may not ſometimes in part, if not chiefly, be derived from the paucity or plenty, and peculiar nature of one or both of theſe ſorts of effluviums. And in particular, we find in the moſt approved writers ſuch ſtrange phænomena to have ſeveral times happened in great plagues and contagious diſeaſes, fomented and communicated, nay (as many eminent phyſicians believed) begun, by ſome latent peſtiferous, or other malignant, diatheſis or conſtitution of the air, as have obliged many of the learnedeſt of them to have recourſe to the immediate operation of the angels, or of the power and wrath of God himſelf, or at leaſt to ſome unaccountable influence of the ſtars; none of the ſolutions of which difficulties ſeem preferable to what may be gathered from our conjecture; ſince of phyſical agents, of which we know nothing ſo much, as that they are to us invisible and probably of a heteroclite nature, it need be no great wonder, that the operation ſhould alſo be abſtruſe, and the effects uncommon. And on this occaſion it may be conſidered, that there are clearer inducements to perſwade us, that another quality of the atmoſphere, its gravity, may be altered by unſeen effluviums, aſcending from the ſubterraneous regions of our globe; and we have often perceived by the mercurial baroſcope the weight of the air to be notably encreaſed, when we could not perceive in the air, nor ſurface of earth, any cauſe, to which we could aſcribe ſo notable a change. And this gives me a riſe to add, that I have ſometimes allowed myſelf to doubt, whether even the ſun itſelf may not now and then alter the gravity of the atmoſphere otherwiſe than by its beams of heat. And I remember, I deſired ſome virtuoſi of my acquaintance to aſſiſt me in the enquiry, whether any of the ſpots, that appear about the ſun, may not, upon their ſudden diſſolution, have ſome of their diſcuſſed and diſperſed matter thrown off, as far as to our atmoſphere, and, that copiouſly enough to produce ſome ſenſibly alterations in it, at leaſt as to gravity.

II. Another thing, that our two forementioned ſuſpicions, if allowed of, will ſuggeſt, is, that it may not ſeem altogether improbable, that ſome bodies, we are converſant with, may have a peculiar diſpoſition and fitneſs to be wrought on by, or to be aſſociated with, ſome of thoſe exotic effluvia, that are emitted by unknown bodies lodged under ground, or that proceed from this or that planet. For what we call ſympathies and antipathies depending indeed on the peculiar textures and other modifications of the bodies, between whom theſe friendſhips and hoſtilities are ſaid to be exerciſed, I ſee not, why it ſhould be impoſſible, that there be a cognation betwixt a body of a congruous or convenient texture, (eſpecially as to the ſhape and ſize of its pores,) and the effluviums of any other body, whether ſubterraneal or ſidereal. We ſee, that convex burning-glaſſes, by virtue of their figure and the diſpoſition of their pores, are fitted to be pervaded by the beams of light and to refract them, and thereby to kindle combuſtible matter; and the ſame beams of the ſun will impart a lucidneſs to the Bolonian ſtone. And as for ſubterraneal bodies, I elſewhere * mention two minerals, which being prepared, (as I there intimate,) the ſteams of the one, aſcending without adventitious heat, and wandering through the air, will not ſenſibly work on other bodies; but if they meet with that, which we prepared, they will immediately have an operation on it, whoſe effect will be both manifeſt and laſting.

III. I now paſs on to the other thing, that the two formerly mentioned ſuſpicions may ſuggeſt, which is, that if they be granted to be well founded, we may be allowed to conſider, whether among the bodies we are acquainted with here below, there may not be found ſome, that may be receptacles, if not alſo attractives, of the ſidereal, and other exotic effluviums, that rove up and down in our air.

Some of the myſterious writers about the philoſophers-ſtone ſpeak great things of the excellency of what they call their philoſophical magnet, which, they ſeem to ſay, attracts and (in their phraſe) corporiſes the univerſal ſpirit, or (as ſome ſpeak) the ſpirit of the world. But theſe things being abſtruſities, which the writers of them profeſſed to be written for, and to be underſtood only by the ſons of art; I, who freely acknowledged I cannot clearly apprehend them, ſhall leave them in their own worth as I found them, and only, for brevity ſake, make uſe of the received word of a magnet, which I may do in my own ſenſe, without avowing the received doctrine of attraction. For by ſuch a magnet as I here propoſe to ſpeak of, I mean not a body, that can properly attract our foreign effluviums; but ſuch an one, as is fitted to detain and join with them, when by virtue of the various motions, that belong to the air as a fluid, they happened to accoſt the magnet. Which may be illuſtrated by the known way of making oil of tartar (as the chemiſts call it) *per deliquium*. For, though the ſpagyriſts and others ſuppoſe, that the fiery ſalts draw to it the aqueous vapours, yet indeed it does but arreſt, and embody with ſuch of thoſe, that wander through the air, as chance in their paſſage to accoſt it.

And, without receding from the Corpuſcularian principles, we may allow ſome of the bodies, we ſpeak of, a greater reſemblance to magnets, than what I have been mentioning. For not only ſuch a magnet may upon the bare account of adheſion by juxta-poſition, or contact, detain the effluviums, that would glide along it, but theſe may be the more firmly arreſted by a kind of precipitating faculty, that the magnet may have in reference to ſuch effluviums; which, if I had time, I could illuſtrate by ſome inſtances; nay, I dare not deny it to be poſſible, but, that in ſome circumſtances of time, or place, one of our magnets may, as it were, fetch in ſuch ſteams, as would indeed paſs near it, but would not otherwiſe come to touch it. On which occaſion I remember, I have in certain caſes been able to make ſome bodies, not all of them electrical, attract (as they ſpeak) without being excited by rubbing, &c. far leſs light bodies, than the effluviums we are ſpeaking of.

But this it may ſuffice to have glanced at, it not being here my purpoſe to meddle with the myſtical theories of the chemiſts; but rather to intimate, that, without adopting, or rejecting them, one may diſcourſe like a naturaliſt about magnets of celeſtial, and other emanations, that appear not to have been conſidered, not to ſay thought of, either by the ſcholaſtick, or even the mechanical philoſophers.

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* See the Experiment in the diſcourſe of the Determinate Nature of Effluviums.

OF

OF CELESTIAL AND AERIAL MAGNETS.

If now, upon what I have granted in the cloſe of the paſt diſcourſe, you ſhould urge the queſtion further, and preſs me to declare, whether, as I think it no impoſſible thing, that nature ſhould make, ſo I think it no unpracticable or hopeleſs thing, that men ſhould find, or art ſhould prepare, uſeful magnets of the exotic effluviums of the lower region of the earth, or the upper of the world: it would much diſtreſs me to give any other anſwer, than that I think it extremely difficult, and not abſolutely impoſſible; and therefore I would not diſcourage any curious or induſtrious man from attempting to ſatisfy himſelf by experiments, becauſe even a ſeemingly ſlight diſcovery in a thing of this nature may be of no ſmall uſe in the inveſtigation of the nature of the air, eſpecially in ſome particular places, and of the correſpondency, which, by the intervention of the air, the ſuperficial part of the terreſtrial globe may have both with the ſubterraneal regions of the earth, and the celeſtial ones of the univerſe. Some of the things I have tried or ſeen relating to this diſcovery, I muſt, for certain reaſons, leave here unmentioned; and only advertiſe you, that ſeveral bodies, which experience has aſſured us do imbibe or retain ſomething from the air, as ſome calcined minerals, ſome marchaſites, ſome ſalts, as well factitious as natural, &c. may be fit to be often expoſed to it, and then weighed again, and further diligently examined, whether that, which makes the increment of weight, be a mere imbibed moiſture, or alſo ſomewhat elſe; and likewiſe, whether it be ſeparable from the body or not, or however have endowed it with any conſiderably quality; and if you chance to meet with a good magnet, you may then vary experiments with it, by expoſing it long to the air in regions differing much in climate, or ſoil, or both, by expoſing it by day only, or by night, at ſeveral ſeaſons of the year, in ſeveral temperatures of the air, at ſeveral conſiderable aſpects of the ſtars and planets, by making it more or leſs frequently part with what it has gained from the air; and in ſhort, by having regard to variety of circumſtances, which your curioſity and ſagacity may ſuggeſt. For, by thus diverſifying the experiment many ways, you may perhaps, by one or other of them, make ſome unexpected and yet important diſcovery of what effluviums the air, in particular places and times, abounds with, or wants, and perchance too, of ſome correſpondency between the terreſtrial and etherial globes of the world.

I ſhall neither be ſurprized, nor quarrel with you, if you tell me, that theſe are extravagant thoughts; but if I had been fortunate in preſerving all, that trial, obſervation, or other productions of ſome curioſity, I once had for ſuch enquiries, procured me, you would not, perhaps, think me ſo very extravagant. But though I muſt not here make any further mention of them, and ſhall only take notice of one body, namely vitriol, whether crude, or unripe, and (as chemiſts ſpeak) embrionated, or ſpagyrically prepared; yet ſome phænomena of theſe vitriolate ſubſtances may for the preſent, I hope, ſomewhat moderate your cenſure for my putting you upon obſervations, that I fear you yourſelf will judge unpromiſing, and leſs favourable perſons, than you would think phantaſtical. And to let you ſee by a pregnant inſtance, that the air may not only have a notable operation upon vitriol, and that, after a ſtrong fire could work no farther on it, but that this operation was conſiderably diverſified by circumſtances; I ſhall begin what I have to alledge, with what the experienced *Zwelfer* occaſionally obſerved, and relates to uſher in a caution about a chemical preparation of vitriol: For, having informed his reader, that the colcothar, that is made by a ſtrong diſtillation, is not corroſive, he denies, that (to uſe his own words) *ſtatim à diſtillatione ſal ex eodem, affusâ aquâ, elici queat; ſed tum prius,* (continues he,) *ubi aliquandiu aeri expoſitum fuerit; tunc enim ſal præbet quandoque candidum, quandoque purpureum, aſpectu pulcherrimum, quod aliquando in copia acquiſivi, & penes me aſſervo, quandoque etiam nitroſum.*

Which teſtimony of this candid ſpagyriſt has much the more weight with me, becauſe I find, what he affirms of the ſaltleſneſs of newly and ſtrongly calcined vitriol to be very agreeable to ſome of my experiments about colcothar of blue (venereal) vitriol; which ſalt or mineral (I mean vitriol) is ſo odd a concrete, that I have thought fit more than once to recommend the making experiments about it to ſeveral curious perſons, that had better opportunity to continue them than I, whoſe reſidence was not ſo fixed. And I remember, that one of theſe, a perſon induſtrious and verſed in chemical operations, gave me this account, that not only he had differing kinds of ſalts from colcothar expoſed to the air for many months, and robbed at convenient times of what it had acquired, but that in tract of time he found it ſo altered, that he obtained from it a pretty quantity of true running mercury.

AND now, to reſume and conclude what I was ſaying about colcothar, there are two or three things I would propoſe to be obſerved by you, or any virtuoſo, that would aſſiſt me in theſe trials about this odd *calcinatum*, (for to call it *terra damnata* were to injure it.)

THE firſt is, to take notice of ſome circumſtances, that moſt obſervers would overlook; ſuch as (beſides the nature of the ſoil) the temperature of the air, the month of the year, and the winds, the weight of the atmoſphere, the ſpots of the ſun, if any be, the moon's age, and her place in the Zodiac, and the principal aſpects of the planets, and the other chief ſtars. For, though it be a boldneſs to affirm, that any, or perhaps all of theſe together, will have any intereſt in the production of the ſalt or other ſubſtance, to be made or diſcloſed in the colcothar; yet in things new and exorbitant, it may be ſometimes raſh and peremptory to deny, even ſuch things as cannot, without raſhneſs, be poſitively aſſerted; and in our caſe, the ſmall trouble of taking notice of circumſtances will be richly paid by the leaſt diſcovery made in things ſo abſtruſe and conſiderable. And as we cannot yet knowingly pronounce, ſo much as negatively, whether the libration of the moon, and the motion of the ſun (and perhaps of ſome of the other planets) about their own centers, and conſequently their obverting ſeveral parts of their bodies to us, may have an operation upon our atmoſphere; ſo, for aught I know, there may be in thoſe vaſt internal parts of the earth, whoſe thin cruſt only has been here and there dug into by men, conſiderable maſſes of matter, that may have periodical revolutions, or accenſions, or euſtations, or fermentations, or, in ſhort, ſome other notable commotions, whoſe effluvia and effects may have operations, yet unobſerved, on the atmoſphere, and on ſome particular bodies expoſed to it; though theſe periods may be perhaps either altogether irregular, or have ſome kind of regularity differing from what one would expect. As we ſee, that the ſea has thoſe grand intumeſcencies, we call ſpring tides, not every day, nor at any conſtant day of the month or week, but about the full and new moon; and theſe ſpring tides are moſt notably heightened, not every month, but twice a year, at or about the vernal and autumnal equinoxes; which obſervations have not been near ſo ancient and known, as the daily ebbing and flowing of the ſea. The Eteſians of the ancients I ſhall not now inſiſt on, nor the obſervations, that I think I elſewhere mentioned of the elder inhabitants of the *Caribbee-iſlands*, who, when the Europeans firſt reſorted thither, were wont to have hurricanes but once in ſeven years; afterwards they were moleſted with them but once in three years; and of late they are troubled with them almoſt every year. And a phyſician, that lived there, told me, that he had ſcarce ever obſerved them to come but within the compaſs of two months joining to one another. In which inſtances, and divers others, that may be noted of what changes happened to great quantities of matter, nature ſeems to affect ſomething of periodical, but not in a way, that appears too regular.

ONE may add on this occaſion, that memorable paſſage related by the learned *Varenius*[*] of thoſe hot ſprings in *Germany*, that he calls *Thermæ Piperinæ*, of which he affirms in more than one place, that they have this peculiarity, that they annually begin and ceaſe to flow at certain times; the former about the third of *May*, and the latter near the middle of *September*, at which time they are wont to reſt till the following ſpring. But though, for aught I know, our geographer's obſervation will hold in hot ſpring; yet it muſt not be extended to all, at leaſt, if we admit that, which is related by the accurate *Johannes de Laet*, (I ſuppoſe out of *Ximenes*, or the famous conqueror of *Mexico*, *Cortes*,) who tells us, that in the Mexican province, *Xilotepec fons celebratur, qui quatuor continuis annis ſcaturit, deinde quatuor ſequentibus deficit, & rurſus ad priorem modum erumpit, &, quod mirabile, pluviis diebus, parciùs, quum ſudum eſt tempus & aridum, copioſiùs, exuberat.*

Amer, *lib.* V. *cap.* 7.

BUT this is not a place to enlarge upon the grounds of my ſuſpecting, there may be ſome periodical motions and commotions within the terreſtrial globe; what has been mentioned being only to invite you to take notice of circumſtances in your obſervations of colcothar, ſome of which may, with the more ſhew of probability, be kept expoſed for a long time, becauſe that bars of windows, and other erected irons, I have found to acquire, in tract of time, from the effluvia of the earth, a ſettled magnetiſm.

THE other main thing I would recommend, is, that notice be taken not only of the kind of vitriol, the colcothar is made of; (for I generally uſed blue Dantzick vitriol) as martial vitriol, Hungarian vitriol, Roman vitriol, &c. to which I have, for curioſity, added vitriol made by ourſelves of the ſolution of the more ſaline parts of marchaſites in water, without the uſual additament of iron, or copper; but alſo, to what degree the calcination is made, and how far the calcined matter is freed from the ſalt by water. For theſe circumſtances, at leaſt in ſome places, may be of moment, and perhaps may afford us good hints of the conſtitution of the atmoſphere in particular parts, as well as of the beſt preparation of colcothar for detaining the exotic effluviums. And I would the rather have experiments tried again in other places with coleothar not calcined to the utmoſt, nor yet ſo exquiſitely edulcorated, but that ſome ſaline particles ſhould be left in it for future encreaſe; becauſe I have more than once purpoſely tried in vain, that the *Caput Mortuum* of blue vitriol, whereof the oil and other parts had been driven off with a violent and laſting fire, would not, when freſh, impart any ſaltneſs to the water; nor do I think, that out of ſome ounces purpoſely edulcorated I obtained one grain of ſalt. And this ſaltleſs colcothar being expoſed, ſome by me, and ſome by a friend, that had conveniency in another place not far off, to the air, ſome for many weeks and ſome for divers months, we did not find it to have manifeſtly encreaſed in weight, or to have acquired any ſenſible ſaltneſs,

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[*] *Varenius, lib.* 1. *Geograph. Univerſ. Thermæ omnes ferè quas novimus ſine ceſſatione fluunt, exceptis Piperinis Germaniæ, &c.*

neſs, which, ſuppoſing the vitriol to have nothing extraordinary, gave me the ſtronger ſuſpicion of ſome peculiarity in the air of that part of *London*, where the trials had been made, at leaſt, during thoſe times, wherein we made them; becauſe not only former experience, made here in *England*, had aſſured me, that ſome colcothars will gain no deſpicable acceſſion of weight by being expoſed to the air; but accidentally complaining of my lately mentioned diſappointment to an ingenious traveller, that had, in divers countries, been curious to examine their vitriols, he aſſured me, that, though he uſually dulcified his colcothar very well, yet within four or five weeks he found it conſiderably impregnated by the air it was expoſed to.

IT remains, that I add one intimation more about vitriol, which is, that I have found it to have ſo great a correſpondency with the air, that it would not be amiſs to try, not only colcothar of differing vitriols (whether barely made the common way, or without any metalline addition to the vitriol ſtones or ore,) but other preparations of vitriol too, ſuch as expoſing vitriol, only calcined to whiteneſs by the ſun-beams, or further to an higher colour by a gentle heat, or throughly calcined, and then impregnated with a little of its own oil. For ſuch vitriolate ſubſtances as theſe the air may work upon, nay even liquid preparations of vitriol may be peculiarly affected by the air, and thereby perhaps be uſeful to diſcover the preſent conſtitution, or foretel ſome approaching changes of it. Of the uſe of which conjecture, namely the peculiar action of the air on ſome vitriolate liquors, I remember I ſhewed ſome virtuoſi a new inſtance in an experiment, whereof this was the ſum:

[I elſewhere mention a compoſition, that I deviſed, to make with ſublimate, copper, and ſpirit of ſalt, a liquor of a green exceeding lovely. But in the deſcription of it I mentioned not (having no need to do it there) a circumſtance as odd, as the liquor it ſelf was grateful. For the air has ſo much intereſt in the production of this green, that when you have made the ſolution of the copper and mercury with the ſpirit of ſalt, that ſolution will not be green, nor ſo much as greeniſh, as long as you keep it ſtopped in the bolt-head, or ſuch like glaſs, wherein it is made. But if you pour it out into a vial, which, by not being ſtopped, leaves it expoſed to the air, it will after a while ſooner or later attain that delightful green, that ſo much endears it to the beholder's eye. This appeared ſo odd an experiment to the virtuoſi, to whom I firſt related it, that thoſe, that could not gueſs by what means I attained it, could ſcarce believe it. But that troubled not me, who, to ſatisfy myſelf not only of the truth of the experiment, but that it was not ſo contingent as many others, repeated it ſeveral times, and found the ſolution, till the air made it floriſh, to be of a muddy reddiſh colour, quite differing from green. So that I remember, that having once kept ſome of the liquor in the ſame glaſs egg, wherein the ſolution had been made, it looked like very dirty water, whilſt the other part of the ſame ſolution, having been expoſed to the air, emulated the colour of an emerald. In which change it is remarkable, that to clarify this liquor and give it a tranſparent greeneſs, I perceived not, that any precipitation of foul matter was made, to which the alteration could be aſcribed; and yet to make it the more probable, that this change proceeded not from a ſubſidence made of ſome opacating matter effected by ſome days reſt, I kept ſome of the ſolution ſealed up in a fine vial ſeveral months, without finding it at the end of that time other than a dark or muddy liquor, which in ſhort time it ceaſed to be, when, the hermetic ſeal being broken off, the air was permitted to work upon it. And this I further obſerved in our various experiments on this liquor, that, according to the quality of the matter and other circumſtances, the greenneſs was not attained to but at certain periods of time, now and then diſcloſing itſelf within two or three days, and ſometimes not before nine or ten.]

WITH how little confidence of ſucceſs trials, that have the aims of theſe I have been ſpeaking of, are to be attempted, not only conſideration but experience have made me ſenſible. But yet I would not diſcourage men's curioſity from venturing even upon ſlight probabilities, where the nobleneſs of the ſubjects and ſcope may make even ſmall attainments very deſirable. And till trial have been made on occaſions of great moment, it is not eaſy to be ſatisfied, that men have not been wanting to themſelves; which I ſhall only illuſtrate by propoſing what, I preſume, will not need, that I ſhould make an application of it. Thoſe adventurous navigators, that have made voyages for diſcovery in unknown ſeas, when they firſt diſcerned ſomething obſcure near the horizon, at a great diſtance off, have often doubted, whether what they had ſo imperfect a ſight of, were a cloud, or an iſland, or a mountain: but though ſometimes it were more likely to be the former, as that, which more frequently occurred, than the latter; yet they judged it adviſable to ſteer towards it, till they had a clearer proſpect of it; for if it were a deluding meteor, they would not however ſuſtain ſo great a loſs in that of a little labour, as, in caſe it were a country, they would in the loſs of what might prove a rich diſcovery: and if they deſiſted too ſoon from their curioſity, they could not rationally ſatisfy themſelves, whether they ſlighted a cloud, or neglected a country.

SOME ADDITIONAL
EXPERIMENTS
RELATING TO THE
SUSPICIONS about the HIDDEN QUALITIES of the AIR.

THE eſſay about Suſpicions of ſome Hidden Qualities of the Air having been detained ſomewhat long at the preſs, that it might come abroad accompanied with the other tracts deſigned to attend it, whilſt I was rumaging among ſeveral papers to look for ſome other things, I met now and then with an experiment or obſervation, that ſeemed to relate to ſome of the things delivered in that tract; and though they be in themſelves of no great moment, I am content to annex them to the reſt, becauſe, as in that company they may ſignify ſomewhat, ſo I am unwilling, that any matter of fact, relating to ſuch a ſubject, ſhould periſh to ſave the labour of tranſcribing.

EXPERIMENT I.

HAVING occaſion to dulcify ſome calx of Dantzick vitriol, from which the oil had been a good while before diſtilled; water was put upon two large portions of it, that the liquor might be impregnated with the vitriolate particles remaining in the calx; the water put upon one of theſe portions was, ſoon after it was ſufficiently impregnated, filtrated and gently abſtracted, by which means it afforded many drachms of a kind of ſalt of vitriol, that ſeemed to differ very little from the vitriol, that had been calcined: but the water, that was put upon the other portion of calcined vitriol, was in a wide-mouthed veſſel left in the air for a month or ſix weeks; after which time, when it came to be abſtracted after the manner formerly recited, it afforded many drachms of a ſalt, that did not then, nor long after, look at all like common vitriol, or like the other, but ſhot white almoſt like ſalt-petre, or ſome other untincted ſalt. Whether this experiment will conſtantly ſucceed, and at other ſeaſons of the year than that it was made in, which was ſummer, I had not the opportunity to make a full trial, though I endeavoured it. But that the air may have a great ſtroke in varying the ſalts obtainable from calcined vitriol, ſeemed the more probable, becauſe we had ſome colcothar, that had lain many months, if not ſome years, in the air, but in a place ſheltered from the rain; and having cauſed a lixivium to be made of it, to try what ſort or plenty of ſaline particles it would yield, we found, when the ſuperfluous moiſture was exhaled, that they began to ſhoot into ſalt far more white than vitriol, and very differing from it in its figure and way of concretion.

EXPERIMENT II.

This was made at *Oxford*.

WE took colcothar of venereal vitriol carefully dulcified, and leaving it in my ſtudy in the months of *January* and *Febuary*, by weighing it carefully before an ounce of it was expoſed to the air, and after it had continued there ſome weeks, we found it to have encreaſed in weight four grains and about a quarter, beſides ſome little duſt, that ſtuck to the glaſs.

THIS ſlight experiment is here mentioned, that, being compared with the next enſuing trial, it may appear, that the difference of airs, ſeaſons, calces of vitriol, or other circumſtances, may produce a notable diſparity in the increment of weight, the expoſed bodies gain in the air.

EXPERIMENT III.

WE put eight ounces of outlandiſh vitriol, calcined to a deep redneſs, into a ſomewhat broad and flat metalline veſſel, and ſet it by upon a ſhelf, in a ſtudy, that was ſeldom frequented; and at the ſame time, that we might obſerve what increment would be gained by expoſing to the air a larger ſuperficies of the powder, in reference to the bulk, we put into another metalline veſſel, ſmaller than the other, only two ounces of colcothar, and ſet it on the ſame ſhelf with the other, this was done at the vernal equinox, (the twelfth of *March*;) on the twenty-fifth of *June* we weighed theſe powders again, and found the eight ounces to have gained one drachm and ſeventeen grains; but the two ounces had acquired the ſame weight within a grain: then putting them back into their former veſſels, we left them in the ſame place as formerly, till the twenty-fourth of *Auguſt*, when we found cauſe to ſuppoſe, that the greater parcel of colcothar had met with ſome miſchance, either by mice or otherwiſe; but the leſſer parcel weighed twenty-ſix grains heavier than it did in *June*, amounting now to two ounces, one dram, forty-two grains, having increaſed, in leſs than ſix months, above an hundred grains, and conſequently above a tenth part of its firſt weight.

No trial was made to diſcover, what this acquired ſubſtance may be, that we might not diſturb the intended proſecution of the experiment.

EXPERIMENT IV.

BECAUSE in moſt of the experiments of ſubſtances expoſed to be impregnated by the air, or detain its ſaline or other exotic particles, we employed bodies prepared and much altered by the previous operation of the fire; we thought fit to make ſome trials with bodies unchanged by the fire; and to this purpoſe we took a marchaſite, which was partly of a ſhining and partly of a darkiſh colour, and which ſeemed well-diſpoſed to afford vitriol: of this we took ſeveral ſmaller lumps, that amounted to two ounces; theſe were kept in a room, where they were freely acceſſible to the air, which, by reaſon that the houſe, that was ſeated in the country, ſtood high, was eſteemed to be very pure. After the marchaſites had been kept in this room ſomewhat leſs than ſeven weeks, we weighed them again in the ſame balance, and found the two ounces to have gained above twelve grains in weight.

EXPERIMENT V.

THE experiment uſed at the latter end of our paper, about celeſtial and aerial magnets, ſeeming to ſome virtuoſi very ſtrange, and the way, that I employed in making that liquor, that turns green in the air, being ſomewhat troubleſome, I remember I thought fit to try, upon the ſame ground, a way of producing the ſame phænomenon more eaſy and more expeditious. And though perhaps this way will not ſucceed ſo conſtantly, nor always ſo well as the other, yet, for its eaſineſs and cheapneſs, it will not probably be unwelcome to thoſe, that are deſirous to ſee the odd phænomenon.

WE took then, more than once, filings of clean crude copper, and having put on them a convenient quantity of good ſpirit of ſalt, we ſuffered the menſtruum in heat (which need not be very great) to work upon the metal, which it uſually does ſlowly, and not like aqua fortis. When the liquor had by this operation acquired a thick and muddy colour, we decanted it into a clean glaſs with a wide mouth, which being left for a competent time in the open air, the expoſed liquor came to be of a fair green, though it did not appear, that any thing was precipitated at the bottom, to make it clear.

EXPERIMENT VI.

PERHAPS it may not be impertinent to add, that I once or twice obſerved the fumes of a ſharp liquor to work more quickly or manifeſtly on a certain metal ſuſtained in the air, than did the menſtruum itſelf, that emitted thoſe fumes on thoſe parts of the metal, that it covered. And this brings into my mind, that, aſking divers queſtions of a chemiſt, that had been in *Hungary*, and other parts, purpoſely to ſee the mines; he anſwered me, among other things, that, as to the ladders and other wooden work employed in one or more of the deep Hungarian mines, thoſe, that were in the upper part of the grooves, any thing near the external air, would, by the fretting exhalations, be rendered unſerviceable, in not many months; whereas thoſe ladders, and pieces of timber, &c. that were employed in the lower part of the mine, would hold good for two or three times as long.

EXPERIMENT VII.

WE took, about the bigneſs of a nutmeg, of a certain ſoft but conſiſtent body, that we had cauſed to be chemically prepared, and which, in the free air, would continually emit a thick ſmoke: this being put into a vial, and placed in a middle-ſized receiver in our engine, continued for ſome time to afford manifeſt fumes, whilſt the exhauſtion was making; till at length, the air having been more and more pumped out, the viſible aſcenſion of fumes out of the vial quite ceaſed; and the matter having remained ſome time in this ſtate, the ſmoking ſubſtance was ſo altered, that it would not emit fumes, not only when the air was let into the receiver, but not in a pretty while after the vial was taken out of it, till it had been removed to the window, where the wind blowing in freſh and freſh air, it began to ſmoke as formerly.

THE other phænomena of this experiment belong not to this place; but there are two, which will not be impertinent here, and the latter of them may deſerve a ſerious reflection.

THE firſt of them was, that the ſubſtance hitherto mentioned had been kept in a large glaſs, wherein it had been diſtilled at leaſt five or ſix weeks, and yet would ſmoke very plentifully upon the contact of the air, and be kept from ſmoking, though the chemical receiver were ſtopped but with a piece of paper.

THE ſecond was, that, when the vial was put unſtopped in the receiver, and the receiver cloſe luted on, though no exhauſtion were made, yet the white fumes did very quickly ceaſe to aſcend into the receiver, as if this ſmoke participated of the nature of flame, and preſently glutted the air, or otherwiſe made it unfit (and yet without diminution of its gravity) to raiſe the body, that ſhould aſcend.

ANIMADVERSIONS

UPON

Mr. *HOBBES*'s

PROBLEMATA DE VACUO.

PREFACE.

UPON the coming abroad of Mr. *Hobbes*'s *Problemata Phyſica*, finding them in the hands of an ingenious perſon, that intended to write a cenſure of them, which ſeveral employments, private and publick, have, it ſeems, hindered him to do, I began, as is uſual on ſuch occaſions, to turn over the leaves of the book, to ſee what particular things it treated of. This I had not long done, before I found, by obvious paſſages in the third chapter, or dialogue, as well as by the title, which was *Problemata de Vacuo*, that I was particularly concerned in it; upon which I deſired the poſſeſſor of the book, who readily conſented, to leave me to examine that dialogue, on which condition I would leave him to deal with all the reſt of the book. Nor did I look upon the reflections I meant to make, as repugnant to the reſolutions I had taken againſt writing books of controverſy, ſince the explications, Mr. *Hobbes* gave of his problems, ſeemed to contain but ſome variations of, or an appendix to, his tract *De Natura Aeris*, which, being one of the two firſt pieces, that were publiſhed againſt what I had written, was one of thoſe, that I had expreſly reſerved myſelf the liberty to anſwer. But the animadverſions I firſt made upon Mr. *Hobbes*'s Problems *de Vacuo*, having been caſually miſlaid ere they were finiſhed; before I had occaſion to reſume my taſk, there paſſed time enough to let me perceive, that his doctrine, which it will eaſily be thought, that the vacuiſts diſapproved, was not much reliſhed by moſt of the Pleniſts themſelves, the moderneſt Peripateticks, and the Carteſians; each of them maintaining the fullneſs of the world, upon their own grounds, which are differing enough from thoſe of our author, the natural indiſpoſition I have to polemical diſcourſes, eaſily perſuaded me to let alone a controverſy, that did appear needful. And I had ſtill perſiſted in my ſilence, if Mr. *Hobbes* had not, as it were, ſummoned me to break it by publiſhing again his explications, which in my Examen of his dialogue *De Natura Aeris*, I had ſhewn to be erroneous.

AND I did not grow at all more ſatisfied, to find him ſo conſtant, as well as ſtiff an adverſary to interſperſed vacuities, by comparing what he maintains in his dialogue *De Vacuo*, with ſome things, that he teaches, eſpecially concerning God, the cauſe of motion, and the imperviouſneſs of glaſs, in ſome other of his writings, that are publiſhed in the ſame volume with it. For ſince he aſſerts, that there is a God, and owns Him to be the Creator of the World; and ſince, on the other ſide, the penetration of dimenſions is confeſſed to be impoſſible, and he denies, that there is any vacuum in the univerſe; it ſeems difficult to conceive, how in a world, that is already perfectly full of body, a corporeal Deity, ſuch as he maintains in his *Append. ad Leviath. cap.* 3. can have that acceſs, even to the minute parts of the mundane matter, that ſeems requiſite to the attributes and operations, that belong to the Deity, in reference to the world. But I leave divines to conſider, what influence the conjunction of Mr. *Hobbes*'s two opinions, the corporeity of the Deity, and the perfect plenitude of the world, may have on theology. And perhaps, I ſhould not in a phyſical diſcourſe have taken any notice of the propoſed difficulty, but that, to prevent an imputation on the ſtudy of nature's works, (as if it taught us rather to degrade, than admire their author,) it ſeemed not amiſs to hint (*in tranſitu*) that Mr. *Hobbes*'s groſs conception of a corporeal God is not only unwarranted by ſound philoſophy, but ill-befriended even by his own.

MY adverſary having propoſed his problems by way of dialogue between *A.* and *B*; it will not, I preſume, be wondered at, that I have given the ſame form to my animadverſions; which come forth no earlier, becauſe I had divers other treatiſes, that I was more concerned for, to publiſh before them.

BUT, becauſe it will probably be demanded, why, in a tract, that is but ſhort, my animadverſions ſhould take up ſo much room? it will be requiſite, that I here give an account of the bulk of this treatiſe.

And firſt, having found, that there was not any one problem, in whoſe explication, as propoſed by Mr. *Hobbes*, I ſaw cauſe to acquieſce, I was induced, for the reader's eaſe, and that I might be ſure to do my adverſary no wrong, to tranſcribe his whole dialogue, bating ſome few tranſitions, and other clauſes not needful to be transferred hither.

Next, I was not willing to imitate Mr. *Hobbes*, who recites in the dialogue we are conſidering the ſame experiments, that he had already mentioned in his tract *De Natura Aeris*, without adding as his own (that I remember) any new one to them. But my unwillingneſs to tire the reader with bare repetitions of the arguments I employed in my Examen of that tract, invited me to endeavour to make him ſome amends for the exerciſe of his patience by inſerting, as occaſion was offered, five or ſix new experiments, that will not perhaps be ſo eaſily made by every reader, that will be able (now that I have perſpicuouſly propoſed them) to underſtand them.

And laſtly, ſince Mr. *Hobbes* has not been content to manage himſelf and his way of treating of phyſical matters, but has been pleaſed to ſpeak very ſlightingly of experimentarian philoſophers (as he ſtiles them) in general, and, which is worſe, to diſparage the making of elaborate experiments; I judged the thing, he ſeemed to aim at, ſo prejudicial to true and uſeful philoſophy, that I thought it might do ſome ſervice to the leſs knowing, and leſs wary ſort of readers, if I tried to make his own explications enervate his authority, and by a ſomewhat particular Examen of the ſolutions he has given of the problems I am concerned in, ſhew, that it is much more eaſy to undervalue a frequent recourſe to experiments, than truly to explicate the phænomena of nature without them. And ſince our author, ſpeaking of his Problemata Phyſica, (which is but a ſmall book) ſcruples not to tell his majeſty, to whom he dedicates them, that he has therein compriſed (to ſpeak in his own terms) the greateſt and moſt probable part of his phyſical meditations; and ſince by the alterations, he has made in what he formerly writ about the phænomena of my engine, he ſeems to have deſigned to give it a more advantageous form; I conceive, that by theſe ſelected ſolutions of his, one may, without doing him the leaſt injuſtice, make an eſtimate of his way of diſcourſing about natural things. And though I would not intereſt the credit of experimentarian philoſophers in no conſiderabler a paper than this; yet if Mr. *Hobbes*'s explication and mine be attentively compared, it will not, I hope, by them be found, that the way of philoſophiſing he employs is much to be preferred before that, which he undervalues.

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[*] *Credo,* (ſays Mr. *Hobbes* in his *Dialogus Phyſicus:*) *Nam motus hic Reſti tutionis Hobbii eſt, & ab illo primo & ſolo explicatus in Lib. de Corpore, cap.* 21. *Art* 1. *Sine qua Hypotheſi, quantuſcunque labor, ars, ſumptus, ad rerum Naturalium inviſibiles cauſas inveniendas adhibeatur, fruſtra erit.* And ſpeaking of the Gentlemen (to whom it were not here proper for me to give Epithets) that uſed to meet at *Greſham-College*, and are known by the Name of the *Royal-Society*, he thus treats them and their way of inquiring into Nature: *Conveniant, ſtudia conferant, Experimenta faciant quantum volunt, niſi & Principiis utantur meis, nihil proficient.*

A. *Fateris ergo nihil hactenus à Collegis tuis promotam eſſe ſcientiam Cauſarum Naturalium, niſi quod unus eorum Machinam invenerit, quâ motus excitari Aeris poſſit talis, ut partes Sphæræ ſimul undiquaque tendant ad Centrum, & ut Hypotheſes Hobbianæ, antè quidem ſatis probabiles, hinc reddantur probabiliores.*

B. *Nec fateri pudet, nam eſt aliquid prodire tenus, ſi non datur altra.*

A. *Quid tenus? quorſum autem tantus apparatus & ſumptus Machinarum factu difficilium, ut eatenus tantum prodiretis, quantum antè prodierat Hobbius? Cur non inde potius incepiſtis, ubi ille deſiit? Cur principiis ab illo poſitis non eſtis uſi? Cumque Ariſtoteles rectè dixit, ignorato motu ignorari Naturam, &c.*

— *Ad Cauſas autem, propter quas proficere ne paululum quidem potuiſtis, nec poteritis, accedunt etium aliæ, ut odium Hobbii, &c.*

ANIMADVERSIONS, &c.

A. May, one, without too bold an inquiſitiveneſs, aſk, what book you are reading ſo attentively?

B. You will eaſily believe you may, when I ſhall have anſwered you, that it was Mr. *Hobbes*'s lately publiſhed tract of phyſical problems, which I was peruſing.

A. What progreſs have you made in it?

B. I was finiſhing the third dialogue or chapter, when you came in, and finding myſelf, though not named, yet particularly concerned, I was peruſing it with that attention, which it ſeems you take notice of.

A. Divers of your experiments are ſo expreſly mentioned there, that one need not be ſkilled in decyphering to perceive, that you are intereſted in that chapter; and therefore ſeeing you have heedfully read it over, pray give me leave to aſk your judgment, both of Mr. *Hobbes*'s opinion, and his reaſonings about a vacuum.

B. Concerning his opinion, I am ſorry I cannot now ſatiſfy your curioſity, having long ſince taken, and ever ſince kept a reſolution to decline, at leaſt, until a time, that is not yet come, the declaring myſelf either for or againſt the Pleniſts. But as to the other part of your queſtion, which is about Mr. *Hobbes*'s arguments for the abſolute plenitude of the world, I ſhall not ſcruple readily to anſwer, that his ratiocinations ſeem to me far ſhort of that cogency, which the noiſe he would make in the world, and the way, wherein he treats both ancient and modern philoſophers, that diſſent from him, may warrant us to expect.

A. You will allow me the freedom to tell you, that, to convince me, that your reſentment of his explicating divers of the phænomena

mena of your pneumatick engine otherwiſe than you have been wont to do, (and perhaps in terms, that might well have been more civil) has had no ſhare in dictating this judgment of yours; the beſt way will be, that entering for a while into the party of the vacuiſts, you anſwer the arguments he alledges in his chapter to confute them.

B. Having always, as you know, forborn to declare myſelf either way in this controverſy, I ſhall not tye myſelf ſtrictly to the principles and notions of the vacuiſts, nor, though but for a while, oppoſe myſelf to thoſe of the Pleniſts: but ſo far I ſhall comply with your commands, as either upon the doctrine of the vacuiſts, or upon other grounds, to conſider, whether this dialogue of Mr. *Hobbes* have cogently proved his, and the ſchools aſſertion, *Non dari vacuum*; and whether he has rightly explained ſome phænomena of nature, which he undertakes to give an account of, and eſpecially ſome produced in our engine, whereof he takes upon him to render the genuine cauſes. And this laſt inquiry is that, which I chiefly deſign.

A. By this I perceive, that if you can make out your own explications of your adverſary's problems *de vacuo*, and ſhew them to be preferable to his, you will think you have done your work; and that it is but your ſecondary ſcope to ſhew, that in Mr. *Hobbes*'s way of ſolving them, he gives the vacuiſts an advantage againſt him, though not againſt the Pleniſts in general.

B. You do not miſtake my meaning, and therefore without any further preamble, let us now proceeed to the particular phænomena conſidered by Mr. *Hobbes*; the firſt of which is an experiment propoſed by me in the one and thirtieth of the phyſico-mechanical experiments concerning the adheſion of two flat and poliſhed marbles, which I endeavoured to ſolve by the preſſure of the air. And this experiment Mr. *Hobbes* thinks ſo convincing an one to prove the plenitude of the world, that, though he tells us he has many cogent arguments to make it out, yet he mentions but this one, becauſe that, he ſays, ſuffices.

A. The confidence he thereby expreſſes of the great force of the argument does the leſs move me, becauſe I remember, that formerly in his elements of philoſophy he thought it ſufficient to employ one argument to evince the plenitude of the world, and for that one he pitched upon the vulgar experiment of a gardener's watering-pot: but whether he were wrought upon by the objections made to his inference from that phænomenon in your examen of his dialogue *De Natura Aeris*, or by ſome other conſiderations, I will not pretend to divine. But I plainly perceive, he now prefers the experiment of the cohering marbles.

B. Of which it will not be amiſs, though the paſſage be ſomewhat long, to read you his whole diſcourſe out of the book I have in my hand.

A. It is fit, that you, who for my ſake are content to take the pains of anſwering what he ſays, ſhould be eaſed of the trouble of reading it, which I will therefore, with your leave, take upon me. His diſcourſe then about the marbles is this:

A. Ad *probandam univerſi plenitudinem, nullum noſtin' argumentum cogens?*

B. Imo *multa: unum autem ſufficit ex eo ſumptum, quod duo corpora plana, ſi ſe mutuò ſecundùm amborum planitiem communem tangant, non facile in inſtante divelli poſſunt; ſucceſſivè verò facillimè. Non dico, impoſſibile eſſe duo duriſſima marmora ita cohærentia divellere, ſed difficile; & vim poſtulare tantam, quanta ſufficit ad duritiem lapidis ſuperandam. Siquidem verò majore vi ad ſeparationem opus ſit quàm illa, quâ moventur ſeparata, id ſignum eſt, non dari vacuum,*

A. Assertiones *illæ demonſtratione indigent. Primò autem oſtende, quomodo ex duorum duriſſimorum corporum, conjunctorum ad ſuperficies exquiſite læves, diremptione difficili, ſequatur plenitudo mundi?*

A. Si *duo plana, dura, polita corpora (ut marmora) collocentur unum ſupra alterum, ita ut eorum ſuperficies ſe mutuò per omnia puncta exactè, quantum fieri poteſt, contingant, illa ſine magna difficultate ita divelli non poſſunt, ut eodem inſtante per omnia puncta dirimantur. Veruntamen marmora eadem, ſi communis eorum ſuperficies ad horizontem erigatur, aut non valde inclinetur, alterum ab altero facillimè (ut ſcis) etiam ſolo pondere dilabentur. Nonne cauſa hujus rei hæc eſt, quod labenti marmori ſuccedit aer, & relictum locum ſemper implet?*

A. Certissime. *Quid ergo?*

B. Quando *verò eadem uno inſtante divellere conaris, nonne multo major vis adhibenda eſt; quam ob cauſam?*

A. Ego, *& mecum (puto) omnes cauſam ſtatuunt, quod ſpatium totum inter duo illa marmora divulſa ſimul uno inſtante implere aer non poteſt, quantacunque celeritate fiat divulſio.*

B. An *qui ſpatia in aere dari vacua contendunt, in illo aere ſolo dari negant, qui marmora illa conjuncta circumdat?*

A. Minime, *ſed ubique interſperſa.*

B. Dum *ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per conſequens vacuum exprimunt, vacuum faciunt locum per revulſionem relictum; nulla ergo ſeparationis erit difficultas, ſaltem non major, quàm eſt difficultas corpora eadem movendi in aere, poſtquam ſeparata fuerint. Itaque quoniam, conceſſo vacuo, difficultas marmora illa dirimendi nulla eſt, ſequitur per difficultatis experientiam, nullum eſſe vacuum.*

A. Recte *quidem illud infers. Mundi autem plenitudine ſuppoſita, quomodo demonſtrabis poſſibile omnino eſſe, ut divellantur?*

B. Cogita *primo corpus aliquod ductile, nec nimis durum, ut ceram, in duas partes diſtrabi, quæ tamen partes non minus exacte in communi plano ſe mutuo tangunt quàm læviſſima marmora. Jam quo pacto diſtrabatur cera, conſideremus. Nonne perpetuo attenuatur, donec in filum evadat tenuiſſimum, & omni dato craſſo tenuius, & ſic tandem divellitur? Eodem modo etiam duriſſima columna in duas partes diſtrabetur, ſi vim tantam adhibeas, quanta ſufficit ad reſiſtentiam duritiei ſuperandam. Sicut enim in* *cera partes primò extimæ diſtrahuntur, in quarum locum ſuccedit aer; ita etiam in corpore quantumlibet duro aer locum ſubit partium extimarum, quæ primæ vulſionis viribus dirumpuntur. Vis autem, quæ ſuperat reſiſtentiam partium extimarum duri, facilè ſuperabit reſiſtentiam reliquarum. Nam reſiſtentia prima eſt à toto duro, reliquarum verò ſemper à reſiduo.*

A. Ita *quidem videtur conſideranti, quàm corpora quædam, præſertim verò duriſſima, fragilia ſint.*

Does this ratiocination ſeem to you as cogent, as it did to the propoſer of it?

B. You will quickly think it does not; and perhaps you would think it ſhould not, if you pleaſe to conſider with me ſome of the reflections, that the reading of it ſuggeſted to me.

And firſt, without declaring for the vacuiſt's opinion, I muſt profeſs myſelf unſatisfied with Mr. *Hobbes*'s way of arguing againſt them: for, where he ſays, *Dum ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per conſequens vacuum exprimunt, vacuum faciunt locum per revulſionem relictum; nulla ergo ſeparationis erit difficultas, ſaltem non major, quàm eſt difficultas corpora eadem movendi in aere, poſtquam ſeparata fuerint. Itaque quoniam, conceſſo vacuo, difficultas marmora illa dirimendi nulla eſt, ſequitur per difficultatis experientiam, nullum eſſe vacuum.* Methinks he expreſſes himſelf but obſcurely, and leaves his readers to gueſs, what the word *dum* refers to. But that, which ſeems to be his drift in this paſſage, is, that, ſince the vacuiſts allow interſperſed vacuities, not only in the air, that ſurrounds the conjoined marbles, but in the reſt of the ambient air, there is no reaſon, why there ſhould be any difficulty in ſeparating the marbles, or at leaſt any greater difficulty than in moving the marbles in that air after their ſeparation. But, not to conſider, whether his adverſaries will not accuſe his phraſe of ſqueezing out a vacuum, as if it were a body, they will eaſily anſwer, that notwithſtanding the vacuities they admit in the ambient air, a manifeſt reaſon may be given in their hypotheſis of our finding a difficulty in the divulſion of the marbles. For, the vacuities they admit, being but interſperſed, and very ſmall, and the corpuſcles of the atmoſphere being, according to them, endowed with gravity, there lean ſo many upon the upper ſurface of the uppermoſt marble, that that ſtone cannot be at once perpendicularly drawn up from the lower marble contiguous to it, without a force capable to ſurmount the weight of the aerial corpuſcles, that lean upon it. And this weight has already ſo conſtipated the neighbouring parts of the ambient air, that he, that would perpendicularly raiſe the upper marble from the lower, ſhall need a conſiderable force to make the revulſion, and compel the already contiguous parts of the incumbent air to a ſubingreſſion into the pores or intervals, intercepted between them. For the conatus of him, that endeavours to remove the upper marble, whilſt the lower ſurface of it is fenced from the preſſure of the atmoſphere, by the contact of the lower marble, which ſuffers no air to come in between them, is not aſſiſted by the weight or preſſure of the atmoſphere, which, when the marbles are once ſeparated, preſſing as ſtrongly againſt the undermoſt ſurface of the upper marble, as the incumbent atmoſpherical pillar does againſt the upper ſurface of the ſame marble, the hand, that endeavours to raiſe it in the free air, has no other reſiſtance, than that ſmall one of the marble's own weight to ſurmount.

A. But what ſay you to the reaſon, that Mr. *Hobbes*, and, as he thinks, all others give of the difficulty of the often-mentioned divulſion? namely, *Quòd ſpatium totum inter duo illa marmora divulſa ſimul uno inſtante implere aer non poteſt, quantacunque celeritate fiat divulſio.*

B. I ſay, that, for aught I know, the pleniſts may give a more plauſible account of this experiment, than Mr. *Hobbes* has here done; and therefore, abſtracting from the two oppoſite hypotheſes, I ſhall further ſay, that the genuine cauſe of the phænomenon ſeems to be that, which I have already aſſigned; and that difficulty of raiſing the upper ſtone, that accompanies the air's not being able to come in all at once, to poſſeſs the ſpace left between the ſurfaces of the two marbles upon their ſeparation, proceeds from hence, that, till that ſpace be filled with the atmoſpherical air, the hand of him, that would lift up the ſuperior marble, cannot be fully aſſiſted by the preſſure of the air againſt the lower ſurface of that marble.

A. This is a paradox, and therefore I ſhall deſire to know on what you ground it.

B. Though I mention it but as a conjecture propoſed *ex abundanti*, yet I ſhall on this occaſion countenance it with two things; the firſt, that, ſince I declare not for the hypotheſis of the Pleniſts, as it is maintained by Mr. *Hobbes*, I am not bound to allow, what the common explication, adopted by my adverſary, ſuppoſes; namely, that either nature abhors a vacuum (as the ſchools would have it,) or that there could be no divulſion of the marbles, unleſs, at the ſame time, the air were admitted into the room, that divulſion makes for it. And a vacuiſt may tell you, that, provided the ſtrength employed to draw up the ſuperior marble be great enough to ſurmount the weight of the aerial corpuſcles accumulated upon it, the divulſion would enſue, though, by divine Omnipotence, no air, or other body, ſhould be permitted to fill the room made for it by the divulſion; and that the air's ruſhing into that ſpace does not neceſſarily accompany, but in order of nature and time follow upon, a ſeparation of the marbles, the air, that ſurrounded their contiguous ſurfaces, being by the weight of the collaterally ſuperior air, impelled into the room newly made by the divulſion. But I ſhall rather countenance what you call my paradox, by an experiment I purpoſely made in our pneumatical receiver, where, having accommodated two flat and poliſhed marbles, ſo that the lower being fixed, the upper might be laid upon it, and drawn up again as there ſhould be occaſion, I found, that if, when the receiver was well exhauſted, the upper marble was, by a certain contrivance, laid flat upon the lower, they would not then cohere as formerly, but be with great eaſe ſeparated, though it did not, by any phænomenon appear, that any air could come to ruſh in, to poſſeſs the place given it, by the receſs of the upper marble, whoſe very eaſy avulſion is as eaſily explicable by our hypotheſis; ſince the preſſure of that little air, that remained in the receiver, being too faint to make any at all conſiderable reſiſtance to the avulſion of the upper marble, the hand, that drew it up, had very little more than the ſingle weight of the ſtone to ſurmount.

A. An Anti-pleniſt had expected, that you would have obſerved, that the difficult ſeparation of the marbles in the open air does rather prove, that there may be a vacuum, than that there can be none. For in caſe the air can ſucceed as faſt at the ſides, as the divulſion is made, a vacuiſt may demand, whence comes the difficulty of the ſeparation? And if the air cannot fill the whole room made for it by the ſeparated marbles, at the ſame inſtant they are forced aſunder, how is a vacuum avoided for that time, how ſmall ſoever, that is neceſſary for the air to paſs from the edges to the middle of the room newly made?

B. What the Pleniſts will ſay to your argument, I leave them to conſider; but I preſume, they will be able to give a more plauſible account of the phænomenon we are treating of, than is given by Mr. *Hobbes*.

A. What induces you to diſlike his explication of it?

B. Two things; the one, that I think the cauſe he aſſigns improbable; and the other, that I think another, that is better, has been aſſigned already.

And firſt, whereas Mr. *Hobbes* requires to the divulſion of the marbles a force great enough to ſurmount the hardneſs of the ſtone, this is aſſerted gratis, which it ſhould not be; ſince it ſeems very unlikely, that the weight of ſo few pounds, as will ſuffice to ſeparate two coherent marbles of about an inch, for inſtance, in diameter, ſhould be able to ſurmount the hardneſs of ſuch ſolid ſtones, as we uſually employ in this experiment. And though it be generally judged more eaſy to bend, if it may be, or break a broader piece of marble *cæteris paribus*, than a much narrower; yet, whereas neither I, nor any elſe that I know, nor I believe Mr. *Hobbes*, ever obſerved any difference in the reſiſtance of marbles to ſeparation from the greater or leſſer thickneſs of the ſtones; I find by conſtant experience, that, *cæteris paribus*, the broadneſs of the coherent marble does exceedingly encreaſe the difficulty of disjoining them: inſomuch, that, whereas not many pounds, as I was ſaying, would ſeparate marbles of an inch, or a leſſer diameter; when I encreaſed their diameter to about four inches, if I miſ-remember not, there were ſeveral men, that ſucceſſively tried to pull them aſunder, without being able by their utmoſt force to effect it.

A. But what ſay you to the illuſtration, that Mr. *Hobbes*, upon the ſuppoſition of the world's plenitude, gives of our phænomenon, by drawing aſunder the oppoſite parts of a piece of wax?

B. To me it ſeems an inſtance improper enough. For firſt, the parts, that are to be divided in the wax, are of a ſoft and yielding conſiſtence, and according to him, of a ductile, or, if you pleaſe, of a tractile nature, and not, as the parts of the coherent marbles, very ſolid and hard. Next, the parts of the wax do not ſtick together barely by a ſuperficial contact of two ſmooth planes, as do the marbles we are ſpeaking of; but have their parts implicated, and as it were intangled with one another. And therefore, they are far from a diſpoſition to ſlide off, like the marbles, from one another, in how commodious a poſture ſo ever you place them. Beſides, it is manifeſt, that the air has opportunity to ſucceed in the places ſucceſſively deſerted by the receding parts of the attenuated wax; but it is neither manifeſt, nor as yet well proved, by Mr. *Hobbes*, that the air does after the ſame manner ſucceed between the two marbles, which, as I lately noted, are not forced aſunder after ſuch a way, but are, as himſelf ſpeaks, ſevered in all their points at the ſame inſtant.

A. I know, you forget not what he ſays, of the dividing of a hard column into two parts, by a force ſufficient to overcome the reſiſtance of its hardneſs.

B. He does not here either affirm, that he, or any he can truſt, has ſeen the thing done; nor does he give us any ſuch account of the way wherein the pillar is to be broken, whether in an erected, inclined, or horizontal poſture; nor deſcribe the particular circumſtances, that were fit to be mentioned in order to the ſolution of the phænomenon. Wherefore, till I be better informed of the matter of fact, I can ſcarce look upon what Mr. *Hobbes* ſays of the pillar, as other than his conjecture, which now I ſhall the rather paſs by, not only becauſe the caſe is differing from that of our poliſhed marbles, which are actually diſtinct bodies, and only contiguous in one commiſſure; but alſo, becauſe I would haſten to the ſecond reaſon of my diſlike of Mr. *Hobbes*'s explication of our phænomenon, which is, that a better has been given already, from the preſſure of the atmoſphere upon all the ſuperficial parts of the upper marble, ſave thoſe, that touch the plane of the lower.

A. You would have put fair for convincing Mr. *Hobbes* himſelf, at leaſt would have put him to unuſual ſhifts, if you had ſucceeded in the attempt you made, among other of your phyſico-mechanical experiments, to disjoin two coherent marbles, by ſuſpending them horizontally in your pneumatical receiver, and pumping out the air, that invironed them; for, from your failing in that attempt, though you rendered a not improbable reaſon of it, Mr. *Hobbes* took occaſion, in his dialogue *De Natura Aeris*, to ſpeak in ſo high a ſtrain as this: *Nihil iſthic erat, quod ageret pondus; experimento hoc excogitari contra opinionem eorum, qui vacuum aſſerunt, aliud argumentum fortius aut evidentius non potuit. Nam ſi duorum cohærentium alterutrum ſecundùm eam viam, in qua ja-*

cent ipſæ contiguæ ſuperficies, propulſum eſſet, facile ſepararentur, Aere proximo in locum relictum ſucceſſivè ſemper influente; ſed illa ita divellere, ut ſimul totum amitterent contactum, impoſſibile eſt, mundo pleno. Oporteret enim aut motum fieri ab uno termino ad alium in inſtante, aut duo corpora eodem tempore in eodem eſſe loco: quorum utrumvis dicere, eſt abſurdum.

B. You may remember, that where I relate that experiment, I expreſſed a hope, that, when I ſhould be better accommodated than I then was, I might attempt the trial with proſperous ſucceſs; and accordingly afterwards, having got a leſſer engine than that I uſed before, wherewith the air might be better pumped out, and longer kept out, I cheerfully repeated the trial. To ſhew then, that when two coherent marbles are ſuſtained horizontally in the air, the cauſe, why they are not to be forced aſunder, if they have two or three inches in diameter, without the help of a conſiderable weight, is the preſſure I was lately mentioning of the ambient air; I cauſed two ſuch coherent marbles to be ſuſpended in a large receiver, with a weight at the lowermoſt, that might help to keep them ſteady, but was very inconſiderable to that, which their coheſion might have ſurmounted; then cauſing the air to be pumped by degrees out of the receiver, for a good while the marbles ſtuck cloſe together, becauſe, during that time, the air could not be ſo far pumped out, but that there remained enough to ſuſtain the ſmall weight, that endeavoured their divulſion: but when the air was further pumped out, at length the ſpring of the little, but not a little expanded air, that remained, being grown too weak to ſuſtain the lower marble and its ſmall clog, they did, as I expected, drop off.

A. This will not agree over-well with the confident and triumphant expreſſions juſt now recited.

B. I never envied Mr. *Hobbes*'s forwardneſs to triumph, and am content, his conjectures be recommended by the confidence, that accompanies them, if mine be by the ſucceſs, that follows them. But to confirm the explication given by me of our phænomenon, I ſhall add, that as the laſt mentioned trial, which I had ſeveral times occaſion to repeat, ſhews, that the coheſion of our two contiguous marbles would ceaſe upon the withdrawing of the preſſure of the atmoſphere; ſo by another experiment I made, it appears, that the supervening of that preſſure ſufficed to cauſe that coheſion. For, in proſecution of one of the lately mentioned trials, having found, that when the receiver was well exhauſted, two marbles, though conſiderably broad, being laid upon one another after the requiſite manner, their adheſion was, if any at all, ſo weak, that the uppermoſt would be eaſily drawn up from off the other; we laid them again one upon the other, and then letting the external air flow into the receiver, we found, according to expectation, that the marbles now cohered well, and we could not raiſe the uppermoſt, but accompanied with the lowermoſt. But I am ſenſible, I have detained you too long upon the ſingle experiment of the marbles: And though I hope the ſtreſs Mr. *Hobbes* lays on it, will plead my excuſe, yet, to make your patience ſome amends, I ſhall be the more brief in the other particulars, that remain to be conſidered in his dialogue *De Vacuo*. And it will not be difficult for me to keep my promiſe without injuring my cauſe, ſince almoſt all theſe particulars being but the ſame, which he has already alledged in his dialogue *De Natura Aeris*, and I ſoon after anſwered in my Examen of that dialogue, I ſhall need but to refer you to the paſſages, where you may find theſe allegations examined, only ſubjoining here ſome reflections upon thoſe few and ſlight things, that he has added in his problems *De Vacuo*.

A. I may then, I ſuppoſe, read to you the next paſſage to that long one, you have hitherto been conſidering, and it is this: *Ad vacuum nunc revertor: Quas cauſas ſine ſuppoſitione vacui redditurus es illorum effectuum, qui oſtenduntur per Machinam illam, quæ eſt in Collegio* Greſhamenſi?

B. *Machina illa—*

A. Stop here, I beſeech you a little, that, before we go any further, I may take notice to you of a couple of things, that will concern our ſubſequent diſcourſe.

Whereof the firſt is, that it appears by Mr. *Hobbes*'s Dialogue about the Air, that the explications he there gave of ſome of the phænomena of the Machina Boyliana, were directed partly againſt the virtuoſi, that have ſince been honoured with the title of the Royal Society, and partly againſt the author of that engine, as if the main thing therein deſigned were to prove a vacuum. And ſince he now repeats the ſame explications, I think it neceſſary to ſay again, that if he either takes the Society or me for profeſſed vacuiſts, he miſtakes, and ſhoots beſides the mark; for, neither they nor I have ever yet declared either for or againſt a vacuum.

And the other thing I would obſerve to you, is, that Mr. *Hobbes* ſeems not to have rightly underſtood, or at leaſt not to have ſufficiently heeded in what chiefly conſiſts the advantage, which the vacuiſts may make of our engine againſt him: for, whereas in divers places he is very ſolicitous to prove, that the cavity of a pneumatical receiver is not altogether empty, the vacuiſts may tell him, that ſince he aſſerts the abſolute plenitude of the world, he muſt, as indeed he does, reject not only great vacuities, but alſo thoſe very ſmall and interſperſed ones, that they ſuppoſe to be intercepted between the ſolid corpuſcles of other bodies, particularly of the air: ſo that it would not confute them to prove, that in our receiver, when moſt diligently exhauſted, there is not one great and abſolute vacuity, or, as they ſpeak, a *vacuum coacervatum*, ſince ſmaller and diſſeminated vacuities would ſerve their turn. And therefore they may think their pretenſions highly favoured, as by ſeveral particular effects, ſo by this general phænomenon of our engine, that it appears by ſeveral circumſtances, that the common or atmoſpherical air, which, before the pump is ſet to work, poſſeſſed the whole cavity of our receiver, far the greateſt part is by the intervention of the pump made to paſs out the cavity into the open air, without being able, at leaſt for a little while, to get in again; and yet it does not appear by any thing alledged by Mr. *Hobbes*, that any other body ſucceeds to fill adequately the places deſerted by ſuch a multitude of aerial corpuſcles.

A. If I gueſs aright, by thoſe words, (viz. "it appears not by any thing alledged by Mr. *Hobbes*,") you deſign to intimate, that you would not in general prejudice the pleniſts.

B. Your conjecture was well founded: for I think divers of them, and particularly the Carteſians, who ſuppoſe a ſubtile matter or æther fine enough to permeate glaſs, though our common air cannot do it, have not near ſo difficult a taſk to avoid the arguments the vacuiſts may draw from our engine, as Mr. *Hobbes*, who, without having recourſe to the poroſity of glaſs, which indeed is impervious to common air, ſtrives to ſolve the phænomena, and prove our receiver to be always perfectly full, and therefore as full at any one time as at any other of common or atmoſpherical air, as far as we can judge of his opinion by the tendency or import of his explications.

A. Yet, if I were rightly informed of an experiment of yours, Mr. *Hobbes* may be thereby reduced either to paſs over to the vacuiſts, or to acknowledge ſome ætherial or other matter more ſubtil than air, and capable of paſſing through the pores of glaſs; and therefore, to ſhew yourſelf impartial between the vacuiſts and their adverſaries in this controverſy, I hope you will not refuſe to gratify the pleniſts by giving your friends a more particular account of the experiment.

B. I know which you mean, and remember it very well. For, though I long ſince deviſed it, yet having but the other day had occaſion to peruſe the relation I writ down of one of the beſt trials, I think I can repeat it, almoſt in the very words, which if I miſtake not, were theſe:

There was taken a bubble of thin white glaſs, about the bigneſs of a nutmeg, with a very ſlender ſtem, of about four or five inches long, and of the bigneſs of a crow's-quill. The end of the quill being held in the flame of a lamp blown with a pair of bellows, was readily and well ſealed up, and preſently the globous part of the glaſs, being held by the ſtem, was kept turning in the flame, until it was red hot and ready to melt: then being a little removed from the flame, as the included air began to loſe of its agitation and ſpring, the external air manifeſtly and conſiderably preſſed in one of the ſides of the bubble. But the glaſs being again, before the cold could crack it, held as before in the flame, the rarified air diſtended and plumped up the bubble; which being the ſecond time removed from the flame, was the ſecond time compreſſed; and, being the third time brought back to the flame, ſwelled as before, and removed, was again compreſſed, (either this time or the laſt by two diſtinct cavities;) until at length, having ſatisfied ourſelves, that the included air was capable of being condenſed or dilated without the ingreſs or egreſs of air (properly ſo called) we held the bubble ſo long in the flame, ſtrengthened by nimble blaſts, that not only it had its ſides plumped up, but a hole violently broken in it by the over-rarified air, which, together with the former watchfulneſs, we employed from time to time to diſcern, if it were any where cracked or perforated, ſatisfied us, that it was until then entire.

A. I confeſs, I did not readily conceive before, how you could, (as I was told you had,) make a ſolid veſſel, wherein there was no danger of the air's getting in or out, whoſe cavity ſhould be ſtill poſſeſſed with the ſame air, and yet the veſſel be made by turns bigger and leſſer. And, though I preſently thought upon a well ſtopped bladder, yet I well foreſaw, that a diſtruſtful adverſary might make ſome objections, which are by your way of proceeding obviated; and the experiment agrees with your doctrine in ſhewing, how impervious we may well think your thick pneumatick receivers are to common air, ſince a thin glaſs bubble, when its pores were opened or relaxed by flame, would not give paſſage to the ſpringy particles of the air, though violently agitated; for if thoſe particles could have got out of the pores, they never would have broke the bubble, as at length a more violent degree of heat made them do; nor probably would the compreſſion, that afterwards enſued, of the bubble by the ambient air, be checked near ſo ſoon, if thoſe ſpringy corpuſcles had not remained within to make the reſiſtance. Methinks, one may hence draw a new proof of what I remember you elſewhere teach, that the ſpring of the air may be much ſtrengthened by heat. For, in our caſe, the ſpring of the air was thereby inabled to expand the compreſſed glaſs, it was impriſoned in, in ſpite of the reſiſting preſſure of the external air; and yet, that this preſſure was conſiderable, appears by this, that the weight of ſo ſmall a column of atmoſpherical air, as could bear upon the bubble, was able to preſs in the heated glaſs, in ſpite of the reſiſtance of its tenacity and arched figure.

B. Yet that, which I mainly deſigned in this experiment, was, (if I were able) to ſhew and prove at once, by an inſtance not liable to the ordinary exceptions, the true nature of rarefaction and condenſation, at leaſt of the air. For to ſay nothing of the Peripatetick rarefaction and condenſation, ſtrictly ſo called, which I ſcruple not to declare, I think to be phyſically inconceptible or impoſſible; it is plain by our experiment, that, when the bubble, after the glaſs had been firſt thruſt in towards the center, was expanded again by heat, the included air poſſeſſed more room than before, and yet it could perfectly fill no more room than formerly, each aerial particle taking up, both before and after the heating of the bubble, a portion of ſpace adequate to its own bulk; ſo that in the cavity of the expanded bubble we muſt admit either vacuities interſperſed between the corpuſcles of the air, or that ſome fine particles of the flame, or other ſubtil matter, came in to fill up to thoſe intervals, which matter muſt have entered the cavity of the glaſs at its pores: and afterwards, when the red-hot bubble was removed from the flame, it is evident, that ſince the groſſer particles of the air could not get through the glaſs, which they were not able to do, even when vehemently agitated by an ambient flame, the compreſſion of the bubble, and the condenſation of the air, which was neceſſarily conſequent upon it, could not, ſuppoſing the plenitude of the world, be performed without ſqueezing out ſome of the ſubtile matter contained in the cavity of the bubble, whence it could not iſſue but at the pores of the glaſs. But I will no longer detain you from Mr. *Hobbes*'s explications of the *Machina Boyliana*; to the firſt of which you may now, if you pleaſe, advance.

A. THE paſſage I was going to read, when you interrupted me, was this:

B. MACHINA *illa eoſdem effectus producit, quos produceret in loco non magno magnus incluſus ventus.*

A. QUOMODO *ingreditur iſtuc ventus? Machinam noſti cylindrum eſſe cavum, æneum, in quem protruditur cylindrus alius ſolidus ligneis, corio tectus, (quem ſuctorem dicunt) ita exquiſitè congruens, ut ne minimus quidem aer inter corium & æs intrare (ut putant) poſſit.*

B. SCIO, *& quò ſuctor facilius intrudi poſſit, foramen quoddam eſt in ſuperiori parte cylindri, per quod aer (qui ſuctoris ingreſſum alioqui impedire poſſit) emittatur. Quod foramen aperire poſſunt, & claudere, quoties uſus poſtulat. Eſt etiam in cylindri cavi receſſu ſummo datus aditus aeri in globum concavum vitreum, quem etiam aditum claviculâ obturare & aperire poſſunt, quoties volunt. Denique in globo vitreo ſummo relinquitur foramen ſatis amplum, (claviculâ item claudendum & recludendum) ut in illum, quæ volunt, immittere poſſint, experiendi cauſâ.*

B. THE imaginary wind, to which Mr. *Hobbes* here abſcribes the effects of our engine, he formerly had recourſe to in the thirteenth page of his Dialogue; and I have ſufficiently anſwered that paſſage of it in a part of my Examen, to which I therefore refer you.

A. I preſume, you did not overlook the compariſon Mr. *Hobbes* annexes to what I laſt read out of the problems, ſince he liked the conceit ſo well, that we meet with it in his Dialogue *De Natura Aeris*. The words (as you ſee) are theſe: *Tota denique machina non multum differt, ſi naturam ejus ſpectes, à ſclopeto ex ſambuco, quo pueri ſe delectant, imitantes ſclopetos militum, niſi quòd major ſit, & majori arte fabricatus, & pluris conſtet.*

B. I could ſcarce, for the reaſon you give, avoid taking notice of it. And if Mr. *Hobbes* intended it for a piece of raillery, I willingly let it paſs, and could more eaſily forgive him a more conſiderable attempt than this, to be revenged on an engine, that has deſtroyed ſeveral of his opinions: but if he ſeriouſly meant to make a phyſical compariſon, I think he made a very improper one. For not to urge, that one may well doubt how he knows, that in the encloſed cavity of his pot-gun, there is a very vehement wind, ſince that does not neceſſarily follow from the compreſſion of the encluded air: in Mr. *Hobbes*'s inſtrument, the air, being forcibly compreſſed, has an endeavour to expand itſelf, and when it is able to ſurmount the reſiſtance of its priſon, that part, that is firſt disjoined, is forcibly thrown downwards; whereas in our engine it appears by the paſſage lately cited of our Examen, that the air is not compreſſed, but expanded, in our receiver, and if an intercourſe be opened, or the veſſel be not ſtrong enough, the outward air violently ruſhes in; and if the receiver chance to break, the fragments of the glaſs are not thrown outwards, but forcibly inwards.

A. So that, whether or no Mr. *Hobbes* could have pitched upon a compariſon more ſuitable to his intentions, he might eaſily have employed one more ſuitable to the phænomena.

B. I preſume, you will judge it the leſs agreeable to the phænomena, if I here ſubjoin an experiment, that poſſibly you will not diſlike; which I deviſed to ſhew, not only that in our exhauſted receivers there is no ſuch ſtrong endeavour outwards, as moſt of Mr. *Hobbes*'s explications of the things, that happen in them are built upon, but that the weight of the atmoſpherical air, when it is not reſiſted by the counterpreſſure of any internal air, is able to perform what a weight of many pounds would not ſuffice to do.

A. I ſhall the more willingly learn an experiment to this purpoſe, becauſe in your receivers the rigidity of the glaſs keeps us from ſeeing, by any manifeſt change of its figure, whether, if it could yield without breaking, it would not be preſſed in, as your hypotheſis requires.

B. THE deſires to obviate that very difficulty, for their ſatisfaction, that had not yet penetrated the grounds of our hypotheſis, made me think of employing, inſtead of a receiver, of glaſs, one of a ſtiff and tough, but yet ſomewhat flexible, metal. And accordingly, having provided a new pewter porringer, and whelmed it upſide down upon an iron plate, faſtened to the upper end of our pnematical pump, we carefully faſtened, by cement, the orifice to the plate; and though the inverted veſſel, by reaſon of its ſtiffneſs and thickneſs, and the convexity of its ſuperficies, were ſtrong enough to have ſupported a great weight without changing its figure; yet, as ſoon as, by an exſuction or two, the remaining part of the included air was brought to ſuch a degree of expanſion, that its weakened ſpring was able to afford but little aſſiſtance to the tenacity and firmneſs of the metal, the weight of the pillar of the incumbent atmoſphere (which, by reaſon of the breadth of the veſſel, was conſiderably wide alſo) did preſently and notably depreſs the upper part of the porringer, both leſſening its capacity and changing its figure; ſo that, inſtead of the convex ſurface, the receiver had before, it came to a concave one, which new figure was ſomewhat, though not much, increaſed by the further withdrawing of the included and already rarified air. The experiment ſucceded alſo another common porringer of the ſame metal. But in ſuch kind of veſſels, made purpoſely of iron plates, it will ſometimes ſucceed and ſometimes not, according to the diameter of the veſſel, and the thickneſs of the plate, which was ſometimes ſtrong enough, and ſometimes too weak to reſiſt the preſſure of the incumbent air. And ſometimes I found alſo, that the veſſel would be thruſt in, not at the top but ſide-ways, in caſe that ſide were the only part, that were made too thin to reſiſt the preſſure of the ambient; which phænomenon I therefore take notice of, that you may ſee, that that powerful preſſure may be exerciſed laterally as well as perpendicularly.

PERHAPS this experiment, and that I lately recited of an hermetically ſealed bubble, by their fitneſs to diſprove Mr. *Hobbes*'s doctrine, may do ſomewhat towards the letting him ſee, that he might have ſpared that not over-modeſt and wary expreſſion, where, ſpeaking of the gentlemen, that meet at *Greſham-college*, (of whom I pretend not to be one of the chief) he is pleaſed to ſay, *Experimenta faciant quantum volunt, niſi principiis utantur meis, nihil proficient.* But let us, if you pleaſe, paſs on to what he further alledges to prove, that the ſpace in the exhauſted receiver, which the vacuiſts ſuppoſe to be partly empty, is full of air. *Video*, (ſays A.) *ſi ſuctor trudatur uſque ad fundum cylindri æenei, obturenturque foramina, ſecuturum eſſe, dum ſuctor retrahitur, locum in cylindro cavo relictum fore vacuum. Nam ut in locum ejus ſuccedat aer, eſt impoſſibile.* To which B. anſwers, *Credo equidem, ſuctorem cum cylindri cavi ſuperficie ſatis arctè cohærere ad excludendum ſtramen & plumam, non autem aerem neque aquam. Cogita enim, quod non ita accuratè congruerent, quin undiquaque interſtitium relinqueretur, quantum tenuiſſimi capilli capax eſſet. Retracto ergo ſuctore, tantum impelleretur aeris, quantum viribus illis conveniret, quibus aer propter ſuctoris retractionem reprimitur, idque ſine omni difficultate ſenſibili. Quanto autem interſtitium illud minus eſſet, tantum ingrederetur aer velocius: vel ſi contactus ſit, ſed non per omnia puncta, etiam tunc intrabit aer, modò ſuctor majore vi retrahatur. Poſtremò, etſi contactus ubique exactiſſimus ſit, vi tamen ſatis auctâ per cochleam ferream, tum corium cedet, tum ipſum æs; atque ita quoque ingredietur aer. Credin' tu, poſſibile eſſe duas ſuperficies ita exactè componere, ut has compoſitas eſſe ſupponunt illi; aut corium ita durum eſſe, ut aeri, qui cochleæ ope incutitur, nihil omnino cedat? Corium, quanquam optimum, admittit aquam, ut ipſe ſcis, ſi fortè feciſti unquam iter vento & pluvia* ὑόμενος ϗ ἀήμενος. *Itaque dubitare non potes, quin retractus ſuctor tantum aeris in cylindrum adeoque in ipſum recipiens incutiat, quantum ſufficit ad locum ſemper relictum* perfectè *implendum. Effectus ergo, qui oritur à retractione ſuctoris, alius non eſt quàm ventus* (*inquam*) *vehementiſſimus, qui ingreditur undiquaque inter ſuctoris ſuperficiem convexam, & cylindri æenei concavam, proceditque* (*versâ claviculâ*) *in cavitatem globi vitrei, ſive* (*ut vocatur*) *recipientis.*

THE ſubſtance of this ratiocination having been already propoſed by Mr. *Hobbes*, in his dialogue of the air, the eleventh page, I long ſince anſwered it in my Examen; and therefore I ſhall only now take notice *in tranſitu* of ſome ſlight, whether additions or variations, that occur in what you have been reading. And, firſt, I ſee no probability in what he *gratis* aſſerts, that ſo thick a cylinder of braſs, as made the chief part of the pump of our engine, ſhould yield to the ſucker, that was moved up and down in it, though by the help of an iron rack. And whereas he adds, that the leather, that ſurrounds the more ſolid part of the ſucker, would yield to ſuch a force; it ſeems, that that compreſſion of the leather, ſhould, by thruſting the ſolid parts into the pores, make the leather rather leſs, than more fit to give paſſage to the air. Nor would it however follow, notwithſtanding Mr. *Hobbes*'s example, that, becauſe a body admits water, it muſt be pervious to air; for I have ſeveral times, by ways elſewhere taught, made water penetrate the pores of bladders, and yet bladders reſiſt the paſſage of the air ſo well, that even when air included in them was ſufficiently rarified by heat, or by our engine, it was neceſſary for the air to break them before it could get out; which would not have been, if it could have eſcaped through their pores. What Mr. *Hobbes* inculcates here again concerning his *ventus vehementiſſimus*, you will find anſwered in the place of my Examen I lately directed you to.

A. WE may then proceed to Mr. *Hobbes*'s next explication, which he propoſes in theſe terms:

A. CAUSAM *video nunc unius ex machinæ mirabilibus, nimirum cur ſuctor, poſtquam eſt aliquatenus retractus & deinde amiſſus, ſubitò recurrit ad cylindri ſummitatem. Nam aer, qui vi magna fuit impulſus, rurſus per repercuſſionem ad externa vi eadem revertitur.*

B. ATQUE *hoc quidem argumenti ſatis eſt, etiam ſolum, quòd locus à ſuctore relictus non eſt vacuus. Quid enim aut attrahere aut impellere ſuctorem potuit ad locum illum, unde retractus erat, ſi cylindrus fuiſſet vacuus? Nam ut aeris pondus aliquod id efficere potuiſſet, falſum eſſe ſatis ſupra demonſtravi ab eo, quod aer in aere gravitare non poteſt. Noſti etiam, quod cum è recipiente aerem omnem* (*ut illi loquuntur*) *exegerint, poſſunt tamen trans vitrum id quod intus ſit videre, & ſonum, ſi quis fiat, inde audire. Id quod ſolum, etſi nullum aliud argumentum eſſet* (*ſunt autem multa,*) *ad probandum,* nullum *eſſe in recipiente vacuum, abundè ſufficit.*

B. HERE are ſeveral things joined together, which the author had before ſeparately alledged in his often-mentioned dialogue. The firſt is, the cauſe he aſſigns of the aſcenſion of the ſucker, forcibly depreſſed to the bottom of the exhauſted cylinder, and then let alone by him that pumped; to which might be added, that this aſcenſion ſucceeded, when the ſucker was clogged with an hundred pound weight. This explication of Mr. *Hobbes* you will find examined in my diſcourſe. And as to his denying, that the weight or preſſure of the air could drive up the ſucker in that phænomenon, becauſe the air does not weigh in air, we may ſee the contrary largely proved in divers places of my Examen, and more particularly and expreſsly in the firſt pages of the third chapter. And whereas he ſays, in the laſt place, that the viſibility of bodies included in our receivers, and the propagation of ſound, (which, by the way, is not to be underſtood of all ſound, that may be heard, though made in the exhauſted receiver,) are alone ſufficient arguments to prove no vacuum; I have conſidered that paſſage in the anſwer I made to the like allegation, in a part of the Examen; and ſhall only obſerve here, that, ſince the vacuiſts can prove, that much of the air is pumped out of the exhauſted receiver, and will pretend, that, notwithſtanding many interſperſed vacuities, there may be in the receiver corporeal ſubſtance enough to tranſmit light and ſtronger ſounds, Mr. *Hobbes* has not performed what he pretended, if he have but barely proved, that there may be ſubſtances capable of conveying light and ſound in the cavity of our receiver, ſince he triumphantly aſſerts, *nullum eſſe in recipienti vacuum.* But we may leave Mr. *Hobbes* and his adverſaries to diſpute out this point, and go on to the next paſſage.

A. WHICH follows in theſe words:

Ad illud autem, quod ſi veſica aliquatenus inflata in recipiente includatur, paulo poſt per exuctionem aeris inflatur vehementiùs, & dirumpitur, quid reſpondes?

B. *Motus partium aeris undiquaque concurrentium velociſſimus & per concurſum in ſpatiis breviſſimis numeroque infinitis gyrationis velociſſimæ veſicam in locis innumerabilibus ſimul & vi magna, inſtar totidem terebrarum, penetrat, præſertim ſi veſica, antequam immittatur, quò magis reſiſtat aliquatenus inflata ſit. Poſtquam autem aer penetrans ſemel ingreſſus eſt, facile cogitare potes, quo pacto deinceps veſicam tendet, & tandem rumpet. Verùm ſi antequam rumpatur, verſâ claviculâ, aer externus admittatur, videbis veſicam propter vehementiam motus temperatum diminutâ tenſione rugoſiorem. Nam id quoque obſervatum eſt. Jam ſi hæc, quam dixi, cauſa minùs tibi videatur veriſimilis, vide an tu aut alius quicunque imaginari poteſt, quo pacto veſica diſtendi & rumpi poſſit à viribus vacui, id eſt, nihili.*

B. THIS explication Mr. *Hobbes* gives us in the 19th page of his dialogue *De Natura aeris*, and you may find it at large confuted in the latter part of the third chapter of my Examen. Nor does, what he here ſays in the cloſe about the *vires vacui* or *nihili*, deſerve to detain us, ſince there is no reaſon at all, that the vacuiſts ſhould aſcribe to nothing a power of breaking a bladder, of whoſe rupture the ſpring of the included air ſupplies them ſo eaſily with a ſufficient cauſe.

AFTER what Mr. *Hobbes* has ſaid of the breaking of a bladder, he proceeds to an experiment, which he judges of affinity with it, and his academian having propoſed this queſtion:

Unde fit, ut animalia tam cito, nimirum ſpatio quatuor minutorum horæ, in recipiente interficiantur?

FOR anſwer to it our author ſays:

B. *Nonne animalia ſic incluſa inſugunt in pulmones aerem vehementiſſimè motum? quo motu neceſſe eſt, ut tranſitus ſanguinis ab uno ad alterum cordis ventriculum interceptus, non multò pòſt ſiſtatur. Ceſſatio autem ſanguinis mors eſt. Poſſunt tamen animalia ceſſante ſanguine reviviſcere, ſi aer externus ſatis maturè intromittatur, vel ipſa in aerem temperatum, antequam refrixerit ſanguis, extrahantur.*

THIS explication is not probable enough, to oblige me to add any thing about it to what I have ſaid in my Examen; eſpecially the moſt vehement motion, aſcribed to the air in the receiver, having been before proved to be an imaginary thing. You may therefore, if you pleaſe, take notice of the next explication.

[*Idem aer* (ſays he) *in recipiente carbones ardentes extinguit, ſed & illi, ſi, dum ſatis calidi ſunt, eximantur, relucebunt. Notiſſimum eſt, quòd in fodinis carbonum terreorum (cujus rei experimentum ipſe vidi) ſæpiſſime è lateribus foveæ ventus quidam undiquaque exit, qui foſſores interficit, ignemque extinguit, qui tamen reviviſcunt, ſi ſatis cito ad aerem liberum extrahantur.*]

THIS compariſon, which Mr. *Hobbes* here ſummarily makes, he more fully diſplayed in his dialogue *De Natura Aeris*, and I conſidered, what he there alledged, in my Examen. And though I will not contradict Mr. *Hobbes* in what he hiſtorically aſſerts in this paſſage; yet I cannot but ſomewhat doubt, whether he mingles not his conjecture with the bare matter of fact. For, though I have with ſome curioſity viſited mines in more places than one, and propoſed queſtions to men, that have been converſant in other mines, both elſewhere and in *England* (and particularly in *Derbyſhire* where Mr. *Hobbes* lived long;) yet I could never find, that any ſuch odd and vehement wind, as Mr. *Hobbes* aſcribes the phænomenon to, had been by them obſerved to kill the diggers, and extinguiſh well-lighted coals themſelves: and indeed, it ſeems more likely, that the damp, by its tenacity or ſome peculiarly malign quality, did the miſchief, than a wind, of which I found not any notice taken; eſpecially ſince we ſee, what vehement winds men will be able to endure for a long time, without being near killed by them; and that it ſeems very odd, that a wind, that Mr. *Hobbes* does not obſerve to have blown away the coals, that were let down, ſhould be able (inſtead of kindling them more fiercely) to blow them out.

A. THE laſt experiment of your engine, that your adverſary mentions in theſe problems, is delivered in this paſſage:

A. *Si phialam aquæ in recipiens dimiſeris, exucto aere bullire videbis aquam. Quid ad hoc reſpondebis?*

B. *Credo ſanè in tanta aeris motitatione ſaltaturam eſſe aquam, ſed ut calefiat, nondum audivi. Sed imaginable non eſt*, ſaltationem illam à vacuo naſci poſſe.

B. This phænomenon he likewiſe took notice of, and attempted to explicate in his above-mentioned dialogue, which gave me occaſion, to ſhew, how unlikely it is, that the vehement motion of the air ſhould be the cauſe of it; but he here tells us, that it is not imaginable, that this dancing of the water (as he is pleaſed to call it) proceeds from a vacuum, nor do I know any man, that ever pretended, that a vacuum was the efficient cauſe of it. But the vacuiſts perhaps will tell him, that, though the bubbling of the water be not an effect of a vacuum, it may be a proof of it againſt him; for they will tell him, that it has been formerly proved, that a great part of the atmoſpherical air is by pumping removed out of our exhauſted receiver, and conſequently can no more, as formerly, preſs upon the ſurface of the water. Nor does Mr. *Hobbes* ſhew what ſucceeds in the room of it; and therefore it will be allowable, for them to conclude againſt him (though not perhaps againſt the Carteſians) that there are a great many interſperſed vacuities left in the receiver, which are the occaſion, though not the proper efficient cauſe of the phænomenon. For they will ſay, that the ſpringy particles of the yet included air, having room to unbend themſelves in the ſpaces deſerted by the air, that was pumped out, the aerial and ſpringy corpuſcles, that lay concealed in the pores of the water, being now freed from the wonted preſſure, that kept them coiled up in the liquor, expanded themſelves into numerous bubbles, which becauſe of their comparative lightneſs, are extruded by the water, and many of them appear to have riſen from the bottom of it. And Mr. *Hobbes*'s vehement wind, to produce the ſeveral circumſtances of this experiment, muſt be a laſting one. For, after the agitation of the pump has been quite left off, provided the external air be kept from getting in, the bubbles will ſometimes continue to riſe for an hour after. And that, which agrees very well with our explication, and very ill with that of Mr. *Hobbes*'s, is, that, when by having continued to pump a competent time, the water has been freed from the aerial particles, that lurked in it before, though one continue to pump as luſtily as did, yet the water will not at all be covered with bubbles, as it was, the air, that produced them, being ſpent; though, according to Mr. *Hobbes*'s explications, the wind in the receiver continuing, the dance of the water ſhould continue too.

A. I eaſily gueſs, by what you have ſaid already, what you may ſay of that epiphonema, wherewith Mr. *Hobbes* (in his 18th page) concludes the explications of the phænomena of your engine. [*Spero jam te certum eſſe*, ſays he, *nullum eſſe machinæ illius phænomenon, quo demonſtrari poteſt ullum in univerſo locum dari corpore omni vacuum.*]

B. If you gueſſed aright, you gueſſed, that I would ſay, that as to the phænomena of my engine, my buſineſs was to prove, that he had not ſubſtituted good explications of them in the place of mine, which he was pleaſed to reject. And as for the proving a vacuum by the phænomena of my engine, though I declared, that was not the thing intended, yet I ſhall not wonder, that the vacuiſts ſhould think thoſe phænomena give them an advantage againſt Mr. *Hobbes*. For, though in the paſſage recited by you he ſpeak more cautiouſly than he is wont to do, yet, by what you may have already obſerved in his argumentations, the way he takes to ſolve the phænomena of our engine, is by contending, that our receiver, when we ſay it is almoſt exhauſted, is as full as ever (for he will have it perfectly full,) of common air; which is a conceit ſo contrary to I know not how many phænomena, that I do not remember I have met with or heard of any naturaliſt, whether vacuiſt or pleniſt, that having read my phyſico-mechanical experiments and his dialogue, has embraced his opinion.

A. After what you have ſaid, I will not trouble you with what he ſubjoins about vacuum in general, where having made his academian ſay, [*Mundum ſcis finitum eſſe, & per conſequens vacuum eſſe oportere totum illud ſpatium, quod eſt extra mundum infinitum. Quid impedit, quo minus vacuum illud cum aere mundano permiſceatur?*] he anſwers: *De rebus tranſmundanis nihil ſcio.* For I know, that it concerns not you to take notice of it. But poſſibly the vacuiſts will think he fathers upon them an impropriety they would not be guilty of, making them ſpeak, as if they thought, the *ultra-mundan vacuum* were a real ſubſtance, that might be brought into this world, and mingled with our air. And ſince, for aught I know, Mr. *Hobbes* might have ſpared this paſſage, if he had not deſigned it ſhould intrude the ſlighting anſwer he makes to it; I ſhall add, that by the account Mr. *Hobbes* has given of ſeveral phænomena within the world, it is poſſible, that the vacuiſts may believe his profeſſion of knowing nothing of things beyond it.

After the *Experimenta Boyliana* (as your other adverſary calls them;) Mr. *Hobbes* proceeds to the Torricellian experiment, of which he thus diſcourſes:

A. *Quid de experimento cenſes Torricelliano, probante vacuum per argentum vivum hoc modo: eſt in ſeq. figura ad* A, *pelvis, ſive aliud vas, & in ea argentum vivum uſque ad* B; *eſt autem* C D *tubus vitreus concavus repletus quoque argento vivo. Hunc tubum ſi digito obturaveris, erexeriſque in vaſe* A, *manumque abſtuleris, deſcendet argentum vivum à* C; *verùm non effundetur totum in pelvim, ſed ſiſtetur in diſtantia quadam, puta in* D. *Nonne ergo neceſſarium eſt, ut pars tubi inter* C *&* D *ſit vacua? non enim puto negabis, quin ſuperficies tubi concava & argenti vivi convexa ſe mutuo exquiſitiſſimè contingant.*

B. *Ego neque nego contactum, neque vim conſequentiæ intelligo.*

By which paſſage it ſeems, that he ſtill perſiſts in the ſolution of this experiment, which he gave in his dialogue *De Natura Aeris*, and formerly did, for the main, either propoſe, or adopt, in his elements of philoſophy.

B. This opinion or explication of Mr. *Hobbes* I have, as far as concerns me, conſidered in my Examen, to which it may well ſuffice me to refer you. But yet let me take notice of, what he now alledges:

B. *Si quis* (ſays he) *in argentum vivum, quod in vaſe eſt, veſicam immerſerit inflatam, nonne illa amotâ manu emerget?*

A. *Ita certè, etſi eſſet veſica ferrea vel ex materia quacunque præter aurum.*

B. *Vides igitur ab aere penetrari poſſe argentum vivum.*

A. *Etiam, & quidem illâ ipſâ vi, quam à pondere accipit argenti vivi.*

I confeſs this allegation did a little ſurprize me: it concerned Mr. *Hobbes* to prove, that as much air, as was diſplaced by the deſcending mercury, did at the orifice of the tube, immerſed in ſtagnant mercury, inviſibly aſcend to the upper part of the pipe. To prove this he tells us, that a bladder full of air being depreſſed in quick-ſilver, will, when the hand, that depreſſed it, is removed, be ſqueezed up by the very weight of the mercury, whence it follows, that air may penetrate quick-ſilver. But I know not, who ever denyed, that air invironed with quick-ſilver may thereby be ſqueezed upwards; but, ſince even very ſmall bubbles of air may be ſeen to move in their paſſage through mercury, I ſee not, how this example will at all help the propoſer of it. For it is by mere accident, that the air included in the bladder comes to be buoyed up, becauſe the bladder itſelf is ſo; and if it were filled with water inſtead of air, or with ſtone inſtead of water, it would nevertheleſs emerge, as himſelf confeſſes it would do, if it were made of iron, or of any matter beſides gold, becauſe all other bodies are lighter in ſpecie than quick-ſilver. But ſince the immerſion of the bladder is manifeſt enough to the ſight, I ſee not how it will ſerve Mr. *Hobbes*'s turn, who is to prove, that the air gets into the Torricellian tube inviſibly; ſince it is plain, that even heedful obſervations can make our eyes diſcover no ſuch trajection of the air; which (to add that enforcement of our argument) muſt not only paſs unſeen through the ſuſtained quick-ſilver, but muſt likewiſe unperceivedly dive, in ſpite of its comparative lightneſs, beneath the ſurface of the ponderous ſtagnant mercury, to get in at the orifice of the erected tube. But let us, if you pleaſe, hear the reſt of his diſcourſe about this experiment.

A. Though it be ſomewhat prolix, yet, according to my cuſtom hitherto, I will give it you verbatim.

B. Simul *atque argentum vivum deſcenderit ad* D, *altius erit in vaſe* A *quàm antè, nimirum plus erit argenti vivi in vaſe quàm erat ante deſcenſum, tanto quantum capit pars tubi* C, D. *Tanto quoque minus erit aeris extra tubum quàm ante erat. Ille autem aer, qui ab argento vivo loco ſuo extruſus eſt, (ſuppoſitâ univerſi plenitudine) quò abire poteſt niſi ad eum locum, qui in tubo inter* C & D *à deſcenſu argenti vivi relinquebatur? ſed quâ, inquies, viâ in illum locum ſucceſſurus eſt? Quà, niſi per ipſum corpus argenti vivi aerem urgentis? Sicut enim omne grave liquidum, ſui ipſius pondere, aerem, quem deſcendendo premit, aſcendere cogit (ſi via alia non detur) per ſuum ipſius corpus; ita quoque aerem quem premit aſcendendo, (ſi via alia non detur) per ſuum ipſius corpus tranſire cogit. Manifeſtum igitur eſt, ſuppoſitâ mundi plenitudine poſſe aerem externum ab ipſa gravitate argenti vivi cogi in locum illum inter* C & D. *Itaque phænomenon ilud neceſſitatem vacui non demonſtrat. Quoniam autem corpus argenti vivi penetrationi, quæ fit ab aere, non nihil reſiſtit, & aſcenſioni argenti vivi in vaſe* A *reſiſtit aer; quando illæ duæ reſiſtentiæ æquales erunt, tunc in tubo ſiſtetur alicubi argentum vivum; atque ibi eſt* D.

B. In anſwer to this explication I have in my Examen propoſed divers things, which you may there meet with: and indeed his explication has appeared ſo improbable to thoſe, that have written of this experiment, that I have not found it embraced by any of them, though, when divers of them oppoſed it, the phænomena of our engine were not yet divulged. Not then needleſly to repeat what has been ſaid already, I ſhall on this occaſion only add one experiment, that I afterwards made, and it was this: having made the Torricellian experiment (in a ſtraight tube) after the ordinary way, we took a little piece of a fine bladder, and raiſing the pipe a little in the ſtagnant mercury, but not ſo high as the ſurface of it, the piece of bladder was dexterouſly conveyed in the quick-ſilver, ſo as to be applied by one's finger to the immerſed orifice of the pipe, without letting the air get into the cavity of it; then the bladder was tied very ſtraight and carefully to the lower end of the pipe, whoſe orifice, as we ſaid, it covered before, and then the pipe being ſlowly lifted out of the ſtagnant mercury, the impendent quick-ſilver appeared to lean but very lightly upon the bladder, being ſo near an exact æquilibrium with the atmoſpherical air, that, if the tube were but a very little inclined, whereby the gravitation of the quick-ſilver, being not ſo perpendicular, came to be ſomewhat leſſened, the bladder would immediately be driven into the orifice of the tube, and to the eye, placed without, appear to have acquired a concave ſuperficies inſtead of the convex it had before. And when the tube was re-erected, the bladder would no longer appear ſucked in, but be again ſomewhat protuberant. And if, when the mercury in the pipe was made to deſcend a little below its ſtation into the ſtagnant mercury, if, I ſay, at that nick of time, the piece of bladder were nimbly and dexterouſly applied, as before, to the immerſed orifice, and faſtened to the ſides of the pipe, upon the lifting the inſtrument out of the ſtagnant mercury, the cylinder of that liquor being now ſomewhat ſhort of its due height, was no longer able fully to counterpoiſe the weight of the atmoſpherical air, which conſequently, though the glaſs were held in an erected poſture, would preſs up the bladder into the orifice of the pipe, and both make and maintain there a cavity ſenſible both to the touch and the eye.

A. What did you mainly drive at in this experiment?

B. To ſatisfy ſome ingenious men, that were more diffident of, than ſkilful in hydroſtaticks, that the preſſure of the external air is capable of ſuſtaining a cylinder of twenty-nine or thirty inches of mercury, and upon a ſmall leſſening of the gravitation of that ponderous liquor, to preſs it up higher into the tube. But a farther uſe may be made of it againſt Mr. *Hobbes*'s pretenſion. For, when the tube is again erected, the mercury will ſubſide as low as at firſt, and leave as great a ſpace as formerly was left deſerted at the top; into which, how the air ſhould get to fill it, will not appear eaſy to them, that, like you and me, know by many trials, that a bladder will rather be burſt by air than grant it paſſage. And if it ſhould be pretended, either, that ſome air from without had yet got through the bladder, or, that the air, that they may preſume to have been juſt before included between the bladder and the mercury, made its way from the lower part of the inſtrument to the upper; it is obvious to anſwer, that it is no way likely, that it ſhould paſs all along the cylinder unſeen by us; ſince when there are really any aerial bubbles, though ſmaller than pins heads, they are eaſily diſcernible. And in our caſe, there is no ſuch reſiſtance of the air to the aſcenſion of the ſtagnant mercury, as Mr. *Hobbes* pretends in the Torricellian experiment made the uſual way.

A. But, whatever becomes of Mr. *Hobbes*'s explication of the phænomenon; yet may not one ſtill ſay, that it affords no advantage to the vacuiſts againſt him?

B. Whether or no it do againſt other Pleniſts, I ſhall not now conſider; but I doubt, the vacuiſts will tell Mr. *Hobbes*, that he is fain in two places of the explication, we have read, to ſuppoſe the plenitude of the world, that is, to beg the thing in queſtion, which it is not to be preſumed they will allow.

A. But may not Mr. *Hobbes* ſay, that it is as lawful for him to ſuppoſe a plenum, as for them to ſuppoſe a vacuum.

B. I think he may juſtly ſay ſo; but it is like they will reply, that, in their way of explicating the Torricellian experiment, they do not ſuppoſe a vacuum as to air, but prove it. For they ſhew a great ſpace, that having been juſt before filled with quickſilver, is now deſerted by it, though it appeared not, that any air ſucceeded in its room; but rather, that the upper end of the tube, is either totally, or near totally ſo devoid of air, that the quickſilver may without reſiſtance, be barely inclining the tube, be made to fill it to the very top: whereas, Mr. *Hobbes* is fain to have recourſe to that, which he knows they deny, the plenitude of the world, not proving by any ſenſible phænomena, that there did get in, through the quickſilver, air enough to fill the deſerted part of the tube, but only concluding, that ſo much air muſt have got in there, becauſe, the world being full, it could find no room any where elſe; which the vacuiſts will take for no proof at all, and the Carteſians, though Pleniſts, who admit an etherial matter capable of paſſing through the pores of glaſs, will, I doubt, look upon but as an improper explication.

A. I remember on this occaſion another experiment of yours, that ſeems unfavourable enough to Mr. *Hobbes*'s explication; and you will perhaps call it to mind, when I tell you, that it was made in a bended pipe almoſt filled with quickſilver.

B. To ſee, whether we underſtand one another, I will briefly deſcribe the inſtrument I think you mean. We took a cylindrical pipe of glaſs, cloſed at the upper end, and of that length, that being dexterouſly bent at ſome inches from the bottom, the ſhorter leg was made as parallel, as we could, to the longer: in this glaſs we found an expedient, (for it is not eaſy to do,) to make the Torricellian experiment, the quickſilver in the ſhorter leg ſerving inſtead of the ſtagnant quickſilver in the uſual baroſcope, and the quickſilver in the longer leg reaching above that in the ſhorter, about eight or nine and twenty inches. Then, by another artifice, the ſhorter leg, into which the mercury did not riſe within an inch of the top, was ſo ordered, that it could in a trice be hermetically ſealed, without diſordering the quickſilver. And this is the inſtrument, that I gueſs you mean.

A. It is ſo, and I remember, that it is the ſame with that, which in the paradox about ſuction you call, whilſt the ſhorter leg remains unſealed, a travelling baroſcope. But when I ſaw you make the experiment, that leg was hermetically ſealed, an inch of air in its natural or uſual conſiſtence being left in the upper part of it, to which air you outwardly applied a pair of heated tongs.

B. Yet that, which I chiefly aimed at in the trial, was not the phænomenon I perceive you mean; for my deſign was, by breaking the ice for them, to encourage ſome, that may have more ſkill and accommodation than I then had, to make an attempt, that I did not find to have been made by any; namely, to reduce the expanſive force of heat in every way included air, if not in ſome other bodies alſo, to ſome kind of meaſure, and, if it were poſſible, to dermine it by weight. And I preſumed, that, at leaſt, the event of my trial would much confirm ſeveral explications of mine, by ſhewing, that heat is able, as long as it laſts, very conſiderably to encreaſe the ſpring or preſſing power of the air. And in this conjecture I was not miſtaken; for, having ſhut up, after the manner newly recited, a determinate quantity of uncompreſſed air, which (in the experiment you ſaw) was about one inch; we warily held a pair of heated tongs near the outſide of the glaſs, (without making it touch the inſtrument, for fear of breaking it,) whereby the air being agitated, was enabled to expand itſelf to double its former dimenſions, and conſequently had its ſpring ſo ſtrengthened by heat, that it was able to raiſe all the quickſilver in the longer leg, and keep up, or ſuſtain, a mercurial cylinder of about nine and twenty inches high, when, by its expanſion, it would, if it had not been for the heat, have loſt half the force of its elaſticity. But whatever I deſign in this experiment, pray tell me, what uſe you would make of it againſt Mr. *Hobbes*.

A. I believe, he will find it very difficult to ſhew, what keeps the mercury ſuſpended in the longer leg of the travelling baroſcope, when the ſhorter leg is unſtopped, at which it may run out; ſince this inſtrument may, as I have tried, be carried to diſtant places, where it cannot with probability be pretended, that any air has been diſplaced by the fall of the quickſilver in the longer leg, which perhaps fell long before above a mile off. And when the ſhorter leg is ſealed, it will be very hard for Mr. *Hobbes* to ſhew there the odd motions of the air, to which he aſcribes the Torricellian experiment. For, if you warily incline the inſtrument, the quick-ſilver will riſe to the top of the longer leg, and immediately ſubſide, when the inſtrument is again erected, and yet no air appears to paſs through the quick-ſilver interpoſed between the ends of the longer and the ſhorter leg. But that, which I would chiefly take notice of in the experiment, is, that upon the external application of a hot body to the ſhorter leg of the baroſcope, when it was ſealed up, the included air was expanded from one inch to two, and ſo raiſed the whole cylinder of mercury in the longer leg, and, whilſt the heat continued undiminiſhed, kept it from ſubſiding again. For, if the air were able to get unſeen through the body of the quick-ſilver, why had it not been much more able, when rarified by heat, to paſs through the quick-ſilver, than for want of doing ſo to raiſe and ſuſtain ſo weighty a cylinder of mercury? I ſhall not ſtay to enquire on this occaſion, how Mr. *Hobbes* will, according to his hypotheſis, explicate the rarefaction of the air to double its former dimenſions, and the condenſation of it again; eſpecially ſince, aſſerting that part of the upper leg, that is unfilled with the Quick-ſilver, to be perfectly full of air, he affirms that, which I doubt he cannot prove, and which may very probably be diſproved by the experiment you mention in the diſcourſe about ſuction, where you ſhew, to another purpoſe, that in a travelling baroſcope, whoſe ſhorter leg is ſealed, if the end of the longer leg be opened, whereby it comes indeed to be filled with air, the preſſure of that air will enable the ſubjacent mercury notably to compreſs the air included in the ſhorter leg.

B. I leave Mr. *Hobbes* to conſider what you have objected againſt his explication of the Torricellian experiment; to which I ſhall add nothing, though, perhaps, I could add much, becauſe I think it may be well ſpared, and our conference has laſted long already.

A. I will then proceed to the laſt experiment recited by Mr. *Hobbes* in his *problemata de vacuo.*

A. *Si phialam, collum habentem longiuſculum, eandémque omni corpore præter aerem vacuam ore ſugas, continuoque phialæ os aquæ immergas, videbis aquam aliquouſque aſcendere in phialam. Quî fieri hoc poteſt, niſi factum ſit vacuum ab exuctione aeris, in cujus locum poſſit aqua illa aſcendere?*

B. *Conceſſo vacuo, oportet quædam loca vacua fuiſſe in illo aere, etiam qui erat intra phialam ante ſuctionem. Cur ergo non aſcendebat aqua ad ea implenda abſque ſuctione? is qui ſugit phialam, neque in ventrem quicquam, neque in pulmones, neque in os è phiala exugit. Quid ergo agit? Aerem commovet, & in partibus ejus conatum ſugendo efficit per os exeundi, & non admittendo, conatum redeundi. Ab his conatibus contrariis componitur circumitio intra phialam, & conatus exeundi quaquaverſum. Itaque phialæ ore aquæ immerſo, aer in ſubjectam aquam penetrat è phiala egrediens, & tantundem aquæ in phialam cogit.*

Præterea vis illa magna ſuctionis facit, ut ſugentis labra cum collo phialæ aliquando arctiſſimè cohæreant propter contactum exquiſitiſſimum.

B. As to the firſt clauſe of Mr. *Hobbes*'s account of our phænomenon, the vacuiſts will eaſily anſwer his queſtion, by acknowledging, that there were indeed interſperſed vacuities in the air contained in the vial before the ſuction; but they will add, there was no reaſon, why the water ſhould aſcend to fill them, becauſe, being a heavy body, it cannot riſe of itſelf, but muſt be raiſed by ſome prevalent weight or preſſure, which then was wanting. Beſides, that there being interſperſed vacuities as well in the reſt of the air, that was very near the water, as in that contained in the vial, there was no reaſon, why the water ſhould aſcend to fill the vacuities of one portion of air rather than thoſe of another. But when once by ſuction a great many of the aerial corpuſcles were made to paſs out of the vial, the ſpring of the remaining air being weakened, whilſt the preſſure of the ambient air, which depends upon its conſtant gravity, is undiminiſhed, the ſpring of the internal becomes unable to reſiſt the weight of the external air, which is therefore able to impel the interpoſed water, with ſome violence, into the cavity of the glaſs, until the air, remaining in that cavity, being reduced almoſt to its uſual denſity, is able, by its ſpring, and the weight of the water got up into the vial, to hinder any more water from being impelled up. For, as to what Mr. *Hobbes* affirms, that, *Is qui ſugit phialam neque in ventrem quicquam, neque in pulmones, neque in os quicquam exugit*; how it will agree with what he elſewhere delivers about ſuction, I leave him to conſider. But I confeſs, I cannot but wonder at his confidence, that can poſitively aſſert a thing ſo repugnant to the common ſentiments of men of all opinions, without offering any proof for it. But I ſuppoſe, they, that are by trial acquainted with ſucking, and have felt the air come in at their mouths, will prefer their own experience to his authority. And as to what he adds, that the perſon, that ſucks, agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wiſh, he had ſhewn us by what means a man, that ſucks, makes this odd commotion of the air; eſpecially in ſuch vials as I uſe to employ about the experiment, the orifice of whoſe neck is ſometimes leſs than a pin's head.

A. That there may be really air extracted by ſuction out of a glaſs, methinks you might argue from an experiment I ſaw you make with a receiver, which was exhauſted by your pump, and conſequently by ſuction. For I remember, when you had counterpoiſed it with very good ſcales, and afterwards by turning a ſtop-cock, let in the outward air, there ruſhed in as much air to fill the ſpace, that had been deſerted by the air pumped out, as weighed ſome ſcruples (conſiſting of twenty grains a-piece) though the receiver were not of the largeſt ſize.

B. You did well to add that clauſe; for, the *Magdeburgic* experiment, mentioned by the induſtrious *Schottus*, having been made with a vaſt receiver, the re-admitted air amounted to a whole ounce and ſome drachms. But to return to Mr. *Hobbes*, I fear not, that he will perſuade you, that have ſeen the experiment he recites, that as ſoon as the neck of the vial is unſtopped under water, the air, that whirled about before, makes a ſally out, and forces in as much water. For, if the orifice be any thing large, you will, inſtead of feeling an endeavour to thruſt away your finger, that ſtopped it, find the pulp of your finger ſo thruſt inward, that a *Peripatetick* would affirm, that he felt it ſucked in. And that intruſion may be the reaſon, why the lip of him, that ſucks, is oftentimes ſtrongly faſtened to the orifice of the vial's neck, which Mr. *Hobbes* aſcribes to a moſt exquiſite contact, but without clearly telling us, how that extraordinary contact is effected. And when your finger is removed, inſtead of perceiving any air go out of the vial through the water, (which, if any ſuch thing happen, you will eaſily diſcover by the bubbles,) you ſhall ſee the water briſkly ſpring up in a ſlender ſtream to the top of the vial, which it could not do, if the cavity were already full of air. And to let you ſee, that when the air does really paſs in or out of the vial immerſed under water, it is very eaſy to perceive its motions, if you dip the neck of the vial in water, and then apply to the globulous part of it either your warm hands or any other competent heat, the internal air being rarified; you ſhall ſee a portion of it, anſwerable to the degree of heat you applied, manifeſtly paſs through the water in ſucceſſive bubbles, whilſt yet you ſhall not ſee any water get into the vial to ſupply the place deſerted by that air. And if, when you have (as you may do by the help of ſucking) filled the neck and part of the belly of the vial with water, you immerſe the orifice into ſtagnant water, and apply warm hands to the globulous part as before, you will find the water in the vial to be driven out, before any bubbles paſs out of the vial into the ſurrounding water; which ſhews, that the air is not ſo forward to dive under the water, and much leſs under ſo ponderous a liquor as quickſilver,) as Mr. *Hobbes* has ſuppoſed.

A. That it is the preſſure of the external air, that (ſurmounting the ſpring of the internal) drives up the water into the vial we have been ſpeaking of, does, I confeſs, follow upon your hypotheſis: but an experimentarian philoſopher, as Mr. *Hobbes* calls you among others, may poſſibly be furniſhed with an experiment to confirm this to the eye.

B. You bring into my mind what I once deviſed to confirm my hypotheſis about ſuction, but found a while ſince, that I had omitted it in my diſcourſe about that ſubject. And therefore I ſhall now repeat to you the ſubſtance at leaſt of the memorial, that was written of that experiment, by which the great intereſt of the weight of the atmoſpherical air in ſuction will appear, and in which alſo ſome things will occur, that will not well agree with Mr. *Hobbes*'s explication, and prevent ſome of his allegations againſt mine.

A. Having not yet met with an experiment of this nature, ſuch an one, as you ſpeak of, will be welcome to me.

B. We took a glaſs bubble, whoſe long ſtem was both very ſlender and very cylindrical; then by applying to the outſide of the ball or globulous part a convenient heat, we expelled ſo much of the air, as that, when the end of the pipe was dipped in water, and the inward air had time to recover its former coolneſs, the water aſcended either to the top of the pipe or very near it. This done, we gently and warily rarified the air in the cavity of the bubble, 'till by its expanſion it had driven out almoſt all the water, that had got up into the ſtem, that ſo it might attain, as near as could be, to that degree of heat and meaſure of expanſion, that it had when the water began to riſe in it. And we were careful to leave two or three drops of water unexpelled at the bottom of the pipe, that we might be ſure, that none of the included air was by this ſecond rarefaction driven out at the orifice of it; as the depreſſion of the water ſo low aſſured us, on the other ſide, that the included air wanted nothing conſiderable of the expanſion it had when the water began to aſcend into the pipe. Whilſt the air was in this rarified ſtate, we preſently removed the little inſtrument out of the ſtagnant water into ſtagnant quickſilver, which in a ſhort time began to riſe in the pipe. Now, if the aſcenſion of the liquor were the effect of nature's abhorrence of a vacuum; or of ſome internal principle of motion; or of the compreſſion and propagated pulſion of the outward air by that, which had been expelled; why ſhould not the mercury have aſcended to the top of the pipe, as the water did before? But *de facto* it did not aſcend half, or perhaps a quarter ſo far; and if the pipe had been long enough, as well as it was ſlender enough, I queſtion, whether the mercury would have aſcended (in proportion to the length of the ſtem) half ſo high as it did.

Now of this experiment, which we tried more than once, I ſee not, for the reaſon lately ex-

expreſſed, how any good account will be given without our hypotheſis, but according to that it is clear.

A. I think I perceive, why you ſay ſo; for the aſcenſion of liquors being an effect of the prevalency of the external air's preſſure againſt the reſiſtance it meets with in the cavity of the inſtrument, and the quickſilver being bulk for bulk many times heavier than water, the ſame ſurpluſage of preſſure, that was able to impel up water to the top of the pipe, ought not to be able to impel up the quickſilver to any thing near that height. And if it be here objected, as it very plauſibly may be, that the raiſed cylinder of mercury was much longer than it ought to have been in reference to a cylinder of water, the proportion in gravity between thoſe two liquors (which is almoſt that of fourteen to to one) being conſidered; I anſwer, that when the cylinder of water reached to the pipe, the air poſſeſſed no more than the cavity of the globulous part of the inſtrument, being very little aſſiſted to dilate itſelf by ſo light a cylinder as that of water: but when the quickſilver came to be impelled into the inſtrument by the weight of the external air, that ponderous body did not ſtop its aſcent as ſoon as it came to be equiponderant to the formerly expelled cylinder of water; becauſe, to attain that height, it reached but a little way into the pipe, and left all the reſt of the cavity of the pipe to be filled with part of that air, which formerly was all ſhut up in the cavity of the bubble; by which means the air, included in the whole inſtrument, muſt needs be in a ſtate of expanſion, and thereby have its ſpring weakened, and conſequently diſabled to reſiſt the preſſure of the external air, as much as the ſame included air did before, when it was leſs rarified; on which account, the undiminiſhed weight or preſſure of the external air was able to raiſe the quickſilver higher and higher, till it had obtained that height, at which the preſſure, compounded of the weight of the mercurial cylinder, and the ſpring of the internal air (now leſs rarified than before,) was equivalent to the preſſure of the atmoſphere or external air.

B. You have given the very explication I was about to propoſe; wherefore I ſhall only add, that, to confirm this experiment by a kind of inverſion of it, we drove by heat a little air out of the bubble, and dipped the open end of the pipe into quickſilver, which by this means we made to aſcend, till it had filled about a fourth part, or leſs, of the pipe, when that was held erected. Then carefully removing it without letting fall any quickſilver, or letting in any air, we held the orifice of the pipe a little under the ſurface of a glaſs full of water, and applying a moderate heat to the outſide of the ball, we warily expelled the quickſilver, yet leaving a little of it to make it ſure, that no air was driven out with it; then ſuffering the included air to cool, the external air was found able to make the water not only aſcend to the very top of the pipe, and thence ſpread itſelf a little into the cavity of the ball, but to carry up before it the quickſilver, that had remained unexpelled at the bottom of the ſtem. And if in making the experiment we had firſt raiſed, as we ſometimes did, a greater quantity of quickſilver, and afterwards drove it out, the quantity of water, that would be impelled into the cavity of the pipe and ball, would be accordingly increaſed.

A. IN this experiment it is manifeſt, that ſomething is driven out of the cavity of the glaſs, before the water or quickſilver begins to aſcend in it: and here alſo we ſee not, that the air can paſs through the pores of quickſilver or water, but that it drives them on before it, without ſenſibly mixing with them. In this experiment there appears not at all any circular wind, as Mr. *Hobbes* fancies in the ſucked vial we are diſputing of, nor any tendency outwards of the included air upon the account of ſuch a wind; but, inſtead of theſe things, that the aſcenſion of the liquors into the cavity of the pipe depends upon the external air, preſſing up the liquors into that cavity, may be argued by this, that the ſame weight of the atmoſphere impelled up into the pipe ſo much more of the lighter liquor, water, than of the heavier liquor, mercury.

B. YOU have ſaid enough on this experiment; but it is not the only I have to oppoſe to Mr. *Hobbes* his explication: for, that there is no need of the ſallying of air out of a vial, to make the atmoſpherical air preſs againſt a body, that cloſes the orifice of it, when the preſſure of the internal air is much weakened; I have had occaſion to ſhew ſome virtuoſi, by ſucking out, with the help of an inſtrument, a conſiderable portion of the air contained in a glaſs; for having then, inſtead of unſtopping the orifice under water, nimbly applied a flat body to it, the external air preſſed that body ſo forcibly againſt it, as to keep it faſtened and ſuſpended, though it were clogged with a weight of many ounces.

A. ANOTHER experiment of yours Mr. *Hobbes*'s explication brings into my mind, by which it appears, that, if there be ſuch a circular wind, as he pretends, produced by ſuction in the cavity of the vial, it muſt needs be ſtrangely laſting. For I have ſeen more than once, that, when you have by an inſtrument ſucked much of the air out of a vial, and afterwards carefully cloſed it, though you kept the ſlender neck of it ſtopped a long time, perhaps for ſome weeks or months, yet when it was opened under water, a conſiderable quantity of the liquor would be briſkly impelled up into the neck and belly of the vial. So that, though I will not be ſo pleaſant with Mr. *Hobbes*, as to mind you on this occaſion of thoſe writers of natural magick, that teach us to ſhut up articulate ſounds in a veſſel, which being tranſported to a diſtant place, and opened there, will render the words, that are committed to it; yet I muſt needs ſay, that ſo laſting a circular wind, as, according to Mr. *Hobbes*, your experiments exhibited, may well deſerve our wonder.

B. YOUR admiration would perchance increaſe, if I ſhould aſſure you, that, having with the ſun-beams produced ſmoke in one of thoſe well-ſtopped vials, this circular wind did not at all appear to blow it about, but ſuffered it to riſe, as it would have done if the incuded air had been very calm. And now I ſhall add but one experiment more, which will not be liable to ſome of the things, as invalid as they are, which Mr. *Hobbes* has alledged in his account of the vial, and which will let you ſee, that the weight of the atmoſpherical air is a very conſiderable thing; and which may alſo incline you to think, that, whilſt Mr. *Hobbes* does not admit a ſubtiler matter, than common air, to paſs through the pores of cloſe and ſolid bodies, the air, he has recourſe to will ſometimes come too late to prevent a vacuum. The experiment, which was partly accidental, I lately found regiſtered to this ſenſe, if not in theſe words: [Having, to make ſome diſcovery of the weight of the air, and for other purpoſes, cauſed an æolipile, very light, conſidering its bulk, to be made by a famous artiſt, I had occaſion to put it ſo often into the fire for ſeveral trials, that at length the copper ſcaled off by degrees, and left the veſſel much thinner than when it firſt came out of the artificer's hands; and a good while after, this change in the inſtrument being not in my thoughts, I had occaſion to employ it, as formerly, to weigh how many grains it would contain of the air at ſuch a determinate conſtitution of the atmoſphere, as was to be met with, where I then chanced to be. For the making this experiment the more exactly, the air was, by a ſtrong but warily applied fire, ſo carefully driven away, that, when clapping a piece of ſealing-wax to the pin-hole, at which it had been forced out, we hindered any communication betwixt the cavity of the inſtrument and the external air, we ſuppoſed the æolipile to be very well exhauſted, and therefore laid it by, that, when it ſhould be grown cold, we might, by opening the orifice with a pin, again let in the outward air, and obſerve the encreaſe of weight, that would thereupon enſue: but the inſtrument, that, as I was ſaying, was grown thin, had been ſo diligently freed from air, that the very little that remained, and was kept by the wax from receiving any aſſiſtance from without, being unable, by its ſpring, to aſſiſt the æolipile to ſupport the weight of the ambient air; this external fluid did, by its weight, preſs againſt it ſo ſtrongly, that it compreſſed it, and ſhrunk it ſo conſiderably inwards, and in more than one place ſo changed its figure, that, when I ſhewed it to the virtuoſi, that were aſſembled at *Greſham-college*, they were pleaſed to command it of me to be kept in their repoſitory, where I preſume it is ſtill to be ſeen.

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OF

OF THE
CAUSE
OF
ATTRACTION BY SUCTION,
A
PARADOX.

PREFACE.

HAVING, about twelve years ago, ſummarily expreſſed and publiſhed my opinion of the cauſe of ſuction, and a while before, or after, brought to the Royal Society the glaſs inſtrument I employed to make it out; I deſiſted for ſome time to add any thing about a problem, that I had but occaſionally handled: only, becauſe the inſtrument, I mentioned in my Examen of Mr. *Hobbes*'s opinion, and afterwards uſed at *Greſham-college*, was difficult enough to be well made, and not to be procured ready made, I did, for the ſake of ſome virtuoſi, that were curious of ſuch things, deviſe a ſlight and eaſy made inſtrument, deſcribed in the following tract, chap. iv. in which the chief phænomena, I ſhewed before the Society, were eaſily producible. But afterwards the miſtakes and erroneous opinions, that in print, as well as in diſcourſe, I met with, even among the learned men, about ſuction, and the curioſity of an ingenious perſon, engaged me to reſume that ſubject and treat of it, as if I had never before meddled with it, for the reaſon intimated in the beginning of the enſuing paper. And finding, upon the review of my latter Animadverſions on Mr. *Hobbes*'s *Problemata de Vacuo*, that ſome paſſages of this tract are referred to there, I ſaw myſelf thereby little leſs than engaged to annex that diſcourſe to thoſe animadverſions. And this I the rather conſented to, becauſe it contains ſome experiments, that I have not elſewhere met with, which, together with ſome other parts of that eſſay, may, I hope, prove of ſome uſe to illuſtrate and confirm our doctrine about the weight and ſpring of the air, and ſupply the leſs experienced than ingenious friends to our hypotheſis, with more grounds of anſwering the latter objections of ſome learned men, againſt whoſe endeavours I perceive it will be uſeful to employ variety of experiments and other proofs, to evince the ſame truth; that ſome or other of theſe may meet with thoſe arguments or evaſions, with which they ſtrive to elude the force of the reſt.

THE title of the following eſſay may ſufficiently keep the reader from expecting to find any other kind of attraction diſcourſed of, than that which is made by ſuction. But yet thus much I ſhall here intimate in general, that I have found by trials purpoſely made, that the examples of ſuction are not the only noted ones of attraction, that may be reduced to pulſion.

OF THE CAUSE OF ATTRACTION BY SUCTION.

CHAP. I.

I MIGHT, Sir, ſave myſelf ſome trouble in giving you that account you deſire of me about ſuction, by referring you to a paſſage in my Examen, I long ſince writ, of Mr. *Hobbes*'s *Dialogus Phyſicus de Natura Aeris*, if I knew you had thoſe two books lying by you. But becauſe I ſuſpect, that my Examen may not be in your hands, ſince it is almoſt out of print, and has not for ſome years been in my own; and becauſe I do not ſo well remember, after ſo long a time, the particulars, that I writ there about ſuction, as I do in general, that the Hypotheſis I propoſed, was very incidentally and briefly diſcourſed of, upon an occaſion miniſtered by a wrong explication given of ſuction by Mr. *Hobbes*, I ſhall here decline referring you to what I there writ; and propoſing to you thoſe thoughts about ſuction, that I remember I there pointed at, I ſhall annex ſome things to illuſtrate and confirm them, that would not have been ſo proper for me to have inſiſted on in a ſhort, and but occaſional excurſion.

And I ſhould immediately proceed to what you expect from me, but, that ſuction being generally looked upon as a kind of attraction, it will be requiſite for me to premiſe ſomething about attraction itſelf. For, beſides that the cauſe of it, which I here diſpute not of, is obſcure, the very nature and notion of it is wont by naturaliſts to be either left untouched, or but very darkly delivered, and therefore will not be unfit to be here ſomewhat explained.

How general and ancient ſoever the common opinion may be, that attraction is a kind of motion quite differing from pulſion, if not alſo oppoſite to it; yet I confeſs, I concur in opinion, though not altogether upon the ſame grounds, with ſome modern naturaliſts, that think attraction a ſpecies of pulſion. And at leaſt among inanimate bodies I have not yet obſerved any thing, that convinces me, that attraction cannot be reduced to pulſion; for, theſe two ſeem to me to be but extrinſical denominations of the ſame local motion, in which, if a moved body precede the movent, or tend to acquire a greater diſtance from it, we call it pulſion; and if, upon the ſcore of the motion, the ſame body follow the movent or approach to it, we call it attraction. But this difference may conſiſt but in an accidental reſpect, which does not phyſically alter the nature of the motion, but is founded upon the reſpect, which the line, wherein the motion is made, happens to have to the ſituation of the movent. And that, which ſeems to me to have been the chief cauſe of men's miſtaking attraction for a motion oppoſite to pulſion, is, that they have looked upon both the moving and moved bodies in too popular and ſuperficial a manner, and conſidered in the movent rather the ſituation of the conſpicuous and more bulky part of the animal or other agent, than the ſituation of that part of the animal, or inſtrument, that does immediately impreſs that motion upon the mobile.

For thoſe, that attentively heed this, may eaſily take notice, that ſome part of that body, or of the inſtrument, which by reaſon of their conjunction in this operation is to be looked on but as making one with it, is really placed behind ſome part of the body to be drawn, and therefore cannot move outwards itſelf without thruſting that body forward. This will be eaſily underſtood, if we conſider, what happens when a man draws a chain after him; for though his body do precede the chain, yet his finger or ſome other part of the hand, wherewith he draws it, has ſome part or other, that reaches behind the fore part of the firſt link, and the hinder part of this link comes behind the anteriour part of the ſecond link; and ſo each link has one of its parts placed behind ſome part of the link next after it, till you come to the laſt link of all. And ſo, as the finger, that is in the firſt link, cannot move forwards, but it muſt thruſt on that link, by this ſeries of truſions the whole chain is moved forwards; and if any other body be drawn by that chain, you may perceive, that ſome part of the laſt link comes behind ſome part of that body, or of ſome intervening body, which, by its coheſion with it, ought in our preſent caſe to be conſidered as part of it. And thus attraction ſeems to be but a ſpecies of pulſion, and uſually belongs to that kind of it, which, for diſtinction's ſake, is called truſion, by which we underſtand that kind of pulſion, wherein the movent goes along with the moved body, without quitting it, whilſt the progreſs laſts; as it happens, when a gardener drives his wheel-barrow before him without letting go his hold of it.

But I muſt not here diſſemble a difficulty, that I foreſee may be ſpeciouſly urged againſt this account of attraction. For it may be ſaid, that there are attractions, where it cannot be pretended, that any part of the attrahent comes behind the attracted body; as in magnetical and electrical attractions, and in that, which is made of water, when it is drawn up into ſprings and pumps.

I need not tell you, that you know ſo well, as that partly the Carteſians, and partly other modern philoſophers, have recourſe on this occaſion, either to ſcrewed particles and other magnetical emiſſions, to explicate phænomena of this kind. And, according to ſuch hypotheſis, one may ſay, that many of theſe magnetical and electrical effluvia come behind ſome parts of the attracted bodies, or at leaſt of the little ſolid particles, that are, as it were, the walls of their pores, or procure ſome diſcuſſion of the air, that may make it thruſt the moveable towards the loadſtone or amber, &c. But if there were none of theſe, nor any other ſubtil agents, that cauſe this motion by a real, though unperceived pulſion; I ſhould make a diſtinction betwixt other attractions and theſe, which I ſhould then ſtile attraction by inviſibles. But, whether there be really any ſuch in nature, and why I ſcruple to admit things ſo hard to be conceived, may be elſewhere conſidered. And you will, I preſume, the freelier allow me this liberty, if (ſince in this place it is proper to do it,) I ſhew you, that in the laſt of the inſtances I formerly objected, (that of the drawing up of water into the barrel of a ſyringe,) there is no attraction of the liquor made by the external air. I ſay then, that by the aſcending rammer, as a part of which I here conſider the obtuſe end, plug or ſucker, there is no attraction made of the contiguous and ſubjacent water, but only there is room made for it, to riſe into, without being expoſed to the preſſure of the ſuperior air. For, if we ſuppoſe the whole rammer to be by divine omnipotence annihilated, and conſequently uncapable of exerciſing any attraction; yet, provided the ſuperior air were kept of from the water by any other way, as well as it was by the rammer, the liquor would as well aſcend into the cavity of the barrel; ſince (as I have elſewhere abundantly proved) the ſurface of the terraqueous globe being continually preſſed on by the incumbent air or atmoſphere, the water muſt be, by that preſſure, impelled into any cavity here below, where there is no air to reſiſt it; as by our ſuppoſition there is not in the barrel of our ſyringe, when the rammer, or whatever elſe was in it, had been annihilated. Which reaſoning may be ſufficiently confirmed by an experiment, whereby I have more than once ſhewn ſome curious perſons, that, if the external air, and conſequently its preſſure, be withdrawn from about the ſyringe, one may pull up the ſucker as much as he pleaſes, without drawing up after it the ſubjacent water. In ſhort, let us ſuppoſe, that a man ſtanding in an inner room does by his utmoſt reſiſtance keep ſhut a door, that is neither locked nor latched, againſt another, who with equal force endeavours to thruſt it open: in this caſe, as if one ſhould forcibly pull away the firſt man, it could not be ſaid, that this man, by his receſs from the door he endeavoured to preſs outwards, did truly and properly draw in his antagoniſt, though upon that receſs the coming in of his antagoniſt would preſently enſue; ſo it cannot properly be ſaid, that by the aſcent of the rammer, which diſplaces the ſuperior air, either the rammer itſelf, or the expelled air, does properly attract the ſubjacent water, though the ingreſs of that liquor into the barrel does thereupon neceſſarily enſue. And that, as the compariſon ſuppoſes, there is a preſſure of the ſuperior air againſt the upper part of the ſucker, you may eaſily perceive, if having well ſtopped the lower orifice of the ſyringe with your finger, you forcibly draw up the ſucker to the top of the barrel. For if then you let go the rammer, you will find it impelled downwards by the incumbent air with a notable force.

CHAP. II.

Having thus premiſed ſomething in general about the nature of attraction, as far as it is neceſſary for my preſent deſign; it will be now ſeaſonable to proceed to the conſideration of that kind of attraction, that is employed to raiſe liquors, and is by a diſtinct name called ſuction.

About the cauſe of this there is great contention between the New Philoſophers, as they are ſtiled, and the Peripateticks. For the followers of *Ariſtotle*, and many learned men, that in other things diſſent from him, aſcribe the aſcenſion of liquors upon ſuction to nature's abhorrence of a vacuum. For, ſay they, when a man dips one end of a ſtraw, or reed, into ſtagnant water, and ſucks at the other end, the air contained in the cavity of the reed paſſes into that of his lungs, and conſequently the reed would be left empty, if no other body ſucceed in the place it deſerts; but there are only (that they take notice of) two bodies, that can ſucceed, the air and the (groſſer liquor) the water; and the air cannot do it, becauſe of the interpoſition of the water, that denies it acceſs to the immerſed orifice of the reed, and therefore it muſt be the water itſelf, which accordingly does aſcend to prevent a vacuum deteſted by nature.

But many of the modern philoſophers, and generally all the Corpuſcularians, look upon this *Fuga Vacua* as but an imaginary cauſe of ſuction, though they do it upon very differing grounds. For, the atomiſts, that willingly admit of vacuities, properly ſo called, both within and without our world, cannot think, that nature hates or fears a vacuum, and declines her uſual courſe to prevent it: And the Carteſians, though they do, as well as the Peripateticks, deny, that there is a vacuum, yet ſince they affirm not only, that there is none *in rerum natura*, but that there can be none, becauſe what others call an empty ſpace having three dimenſions, hath all, that they think belonging to the eſſence of a body, they will not grant nature to be ſo indiſcreet, as to ſtrain her ſelf to prevent the making of a thing, that is impoſſible to be made.

THE Peripatetick opinion about the cauſe of ſuction, though commonly defended by the ſchools, as well modern as ancient, ſuppoſes in nature ſuch an abhorrence of a vacuum, as neither has been well proved, nor does well agree with the lately diſcovered phænomenon of ſuction. For, according to their hypotheſis, water and other liquors ſhould aſcend upon ſuction to any height to prevent a vacuum, which yet is not agreeable to experience. For I have carefully tried, that by pumping with a pump far more ſtanch than thoſe, that are uſually made, and indeed as well cloſed as we could poſſibly bring it to be, we could not, by all our endeavours, raiſe water by ſuction to above 33½ foot. The Torricellian experiment ſhews, that the weight of the air is able to ſuſtain, and ſome of our experiments ſhew, it is able to raiſe a mercurial cylinder equal in weight to as high a cylinder of water, as we were able to raiſe by pumping. For mercury being near 14 times as heavy as water of the ſame bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29 or 30 inches, it muſt be able to counterpoiſe a cylinder of water near fourteen times as long, that is, from thirty-four to near thirty-ſix foot. And very diſagreeable to the common hypotheſis, but conſonant to ours, is the experiment, that I have more than once tried, and I think elſewhere deliver'd, namely; that, if you take a glaſs pipe of about three foot long, and, dipping one end of it in water, ſuck at the other, the water will be ſuddenly made to flow briſkly into your mouth. But, if inſtead of water you dip the lower end into quick-ſilver, though you ſuck as ſtrongly as ever you can, provided, that in this caſe, as in the former, you hold the pipe upright, you will never be able to ſuck up the quick-ſilver near ſo high as your mouth; ſo that if the water aſcended upon ſuction to the top of the ſame pipe, becauſe elſe there would have been a vacuum left in the cavity of it, why ſhould not we conclude, that, when we have ſucked up the quick-ſilver as ſtrongly as we can, as much of the upper part of the tube, as is deſerted by the air, and yet not filled by the mercury, admits, in part at leaſt, a vacuum, (as to air) of which conſequently nature cannot reaſonably be ſuppoſed to have ſo great and unlimited an abhorrency, as the Peripateticks and their adherents preſume. Yet I will not determine, whether there be any more than many little vacuities, or ſpaces devoid of air, in the cavity, ſo called, of the pipe unfilled by the mercury, (ſo that the whole cavity is not one entire empty ſpace; it being ſufficient for my purpoſe, that my experiment affords a good argument *ad hominem* againſt the Peripateticks, and warrants us to ſeek for ſome other cauſe than the *fuga vacui*, why a much ſtronger ſuction, than that, which made water aſcend with eaſe into the ſucker's mouth, will not alſo raiſe quick-ſilver to the ſame height or near it.

See Cont. of Phyſ. Mech. Exp. the 15th Exp.

THOSE modern philoſophers, that admit not the *fuga vacui* to be the cauſe of the raiſing of liquors in ſuction, do generally enough agree in referring it to the action of the ſucker's thorax. For, when a man endeavours to ſuck up a liquor, he does by means of the muſcles enlarge the cavity of his cheſt, which he cannot do, but at the ſame time he muſt thruſt away thoſe parts of the ambient air, that were contiguous to his cheſt, and the diſplaced air does, according to ſome learned men, (therein, if I miſtake not, followers of *Gaſſendus*,) compreſs the contiguous air, and that the next to it, and ſo outwards, till the preſſure, ſucceſſively paſſing from one part of the air to the other, arrive at the ſurface of the liquor; and all other places being as to ſenſe full, the impelled air cannot find place, but by thruſting the water into the room made for it in the pipe, by the receſs of the air, that paſſed into the ſucker's lungs. And they differed not much from this explication, that, without taking in the compreſſion of the ambient air made by the thorax, refer the phænomenon to the propagated motion or impulſe, that is impreſſed on the air diſplaced by the thorax in its dilatation, and yet unable to move in a world perfectly filled, as they ſuppoſe ours to be, unleſs the liquor be impelled into as much of the cavity of the pipe, as faſt as it is deſerted by the air, that is ſaid to be ſucked up. But though I readily confeſs this explication to be ingenious, and ſuch as I wonder not they ſhould acquieſce in, who are acquainted but with the long known, and obvious phænomena of ſuction; and though I am not ſure, but that in the moſt familiar caſes the cauſes aſſigned by them may contribute to the effect; yet, preſerving for *Carteſius* and *Gaſſendus* the reſpect I willingly pay ſuch great philoſophers, I muſt take the liberty to tell you, that I cannot acquieſce in their theory. For I think, that the cauſe of ſuction they aſſign, is in many caſes not neceſſary, in others not ſufficient. And firſt, as to the condenſation of the air by the dilatation of the ſucker's cheſt; when I conſider the extent of the ambient air, and how ſmall a compreſſion no greater an expanſion than that of the thorax is like to make, I can ſcarce think ſo ſlight a condenſation of the free air can have ſo conſiderable an operation on the ſurface of the liquor to be raiſed, as the hypotheſis I examine requires: and that this impulſe of the air by a ſucker's dilated thorax, though it be wont to accompany the aſcenſion of the water procured by ſuction, yet is not of abſolute neceſſity to it, will, I preſume, be eaſily granted, if it can be made out, that even a propagated pulſion, abſtracted from any condenſation of air, is not ſo neceſſarily the cauſe of it, but that the effect may be produced without it. For ſuppoſe, that by divine omnipotence ſo much air, as is diſplaced by the thorax, were annihilated; yet I ſee not, why the aſcenſion of the liquor ſhould not enſue. For, when a man begins to ſuck, there is an æquilibrium, or rather æquipollency between the preſſure, which the air, contained in the pipe, (which is ſhut up with the preſſure of the atmoſphere upon it,) has, by virtue of its ſpring, upon that part of the ſurface of the water, that is environed by the ſides of the pipe, and the preſſure, which the atmoſpherical air has, by virtue of its weight, upon all the reſt of the ſurface of the ſtagnant water; ſo that, when by the dilatation of the ſucker's thorax, the air within the cavity of the pipe comes to be rarified, and conſequently loſe of its ſpring, the weight of the external air continuing in the mean time the ſame, it muſt neceſſarily happen, that the ſpring of the internal air will be too weak to compreſs any longer the gravitation of the external, and conſequently, that part of the ſurface of the ſtagnant water, that is included in the pipe, being leſs preſſed upon, than all the other parts of the ſame ſurfaces muſt neceſſarily give way, where it can leaſt reſiſt, and conſequently be impelled up into the pipe, where the air, having had its ſpring weakened by expanſion, is no longer able to reſiſt, as it did before. This may be illuſtrated by ſomewhat varying an inſtance already given, and conceiving, that within a chamber three men thruſt all together with their utmoſt force againſt a door, (which we ſuppoſe to have neither bolt nor latch) to keep it ſhut, at the ſame time the three other men have juſt equal ſtrength, and employ their force to thruſt it open. For though, whilſt their oppoſite endeavours are equal, the door will continue to be kept ſhut, yet if one of the three men, within the room, ſhould go away, there will need no new force, nor other acceſſion of ſtrength to the three men, to make them prevail, and thruſt open the door againſt the reſiſtance of thoſe, that endeavoured to keep it ſhut, who are now but two.

AND here (upon the by) you may take notice, that, to raiſe water in ſuction, there is no neceſſity of any rarified and forcibly ſtretched rope, as it were, of the air, to draw up the ſubjacent water into the pipe, ſince the bare debilitation of the ſpring of the included air may very well ſerve the turn. And though, if we ſhould ſuppoſe the air within the pipe to be quite annihilated, it could not be pretended (ſince it would not have ſo much as exiſtence) that it exerciſes an attractive power; yet in this caſe the water would aſcend into the pipe, without the aſſiſtance of nature's imaginary abhorrence of a vacuum, but by a mechanical neceſſity, plainly ariſing from this, that there would be a preſſure of the incumbent atmoſphere upon the reſt of the ſurface of the ſtagnant water, and no preſſure at all upon that part of the ſurface, that is within the pipe, where conſequently there could be no reſiſtance made to the aſcenſion of the water, every where elſe ſtrongly urged by the weight of the incumbent air.

I ſhall add on this occaſion, that, to ſhew ſome inquiſitive men, that the weak reſiſtance within a veſſel, that had but one orifice expoſed to the water, may much more contribute to the aſcenſion of that liquor into the veſſel, than either the compreſſion, or the continued or reflected impulſe of the external air; I thought fit to produce a phænomenon, which by the beholders was without ſcruple judged an effect of ſuction, and yet could not be aſcribed to the cauſe of ſuction, aſſigned by either of the ſects of philoſophers I diſſent from. The experiment was this: by a way, elſewhere delivered, the long neck of a glaſs bubble was ſealed up, and almoſt all the air had been by heat driven out of the whole cavity of the bubble or vial, and then the glaſs was laid aſide for ſome hours, or as long as we pleaſed; afterwards the ſealed apex of the neck was broken off under water. I demand now of a Peripatetic, whether the liquor ought to be ſucked or drawn into the cavity of the glaſs, and why? If he ſays, as queſtionleſs he will, that the water would be attracted to hinder a vacuum, he would thereby acknowledge, that, till the glaſs was unſtopped under water, there was ſome empty ſpace in it; for, till the ſealed end was broken off, the water could not get in, and therefore, if the *fuga vacui* had any thing to do in the aſcenſion, the liquor muſt riſe, not to prevent an empty ſpace, but to fill one, that was made before. Nor does our experiment much more favour the other philoſophers I diſſent from; for in it there is no dilatation made of the ſides of the glaſs, as in ordinary ſuction there is made of the thorax, but only there is ſo much air driven out of the cavity of the bubble, into whoſe room ſince neither common air nor water is permitted to ſucceed, it appears not, how the propagated and returning impulſe, or the circle of motion, as to common air and water, does here take place. And then I demand, what becomes of the air, that has been by heat driven out, and is by the hermetical ſeal kept out of the cavity of the bubble? If it be ſaid, that it diffuſes itſelf into the ambient air, and mingles with it, that will be granted, which I contended for, that ſo little air, as is uſually diſplaced in ſuction, cannot make any conſiderable compreſſion of the free ambient air; for, what can one cubic inch of air, which is ſometimes more than one of our glaſſes contains, do, to the condenſation ſo much as of all the air in the chamber, when the expelled corpuſcles are evenly diſtributed among thoſe of the ambient? And how comes this inconſiderable condenſation to have ſo great an effect in every part of the room, as to be able there to impel into the glaſs as much water in extent, as the whole air, that was driven out of the cavity of it? But if it be ſaid, that the expelled air condenſed only the contiguous or very neighbouring air, it is eaſy to anſwer, that it is no way probable, that the expelled particles of the air ſhould not, by the differing motions of the ambient air, be quickly made to mingle with it, but ſhould rather wait (which, if it did, we ſometimes made it do for many hours) till the veſſels, whence it was driven out, were unſtopped again. But, though this could probably be pretended, it cannot truly be aſſerted. For if you carry the ſealed glaſs quite out of the room or houſe, and unſtop it at ſome other place, though two or three miles diſtant; the aſcenſion of the water will (as I found by trial) nevertheleſs enſue; in which caſe I preſume, it will not be ſaid, that the air, that was expelled out of the glaſs, and condenſed the contiguous or near contiguous air, attended the bubble in all its motions, and was ready at hand to impel in the water, as ſoon as the ſealed apex of the vial was broken off. But I doubt not, but moſt of the embracers of the opinion I oppoſe, being learned and ingenious perſons, if they had been acquainted with theſe and the like phænomena, would rather have changed their opinion about ſuction, than have gone about to defend it by ſuch evaſions, which I ſhould not have thought worth propoſing, if I had not met with objections of this nature publickly maintained by a learned writer, on occaſion of the air's ruſhing into the exhauſted Magdenburgic engine. But as in our experiment theſe objections have no place, ſo in our hypotheſis the explication is very eaſy, as will anon be intimated.

CHAP. III.

HAVING thus ſhewn, that the aſcenſion of water upon ſuction may be cauſed otherwiſe than by the condenſation or the propagated pulſion of air contiguous to the ſucker's thorax, and thruſt out of place by it; it remains, that I ſhew, (which was one of the two things I chiefly intended) that there may be caſes, wherein the cauſe, aſſigned in the hypotheſis I am examining, will not have place. But this will be better underſtood, if, before I proceed to the proof of it, I propoſe to you the thoughts, I had many years ſince, and do ſtill retain, about the cauſe of the aſcenſion of liquors in ſuction.

To clear the way to the right underſtanding of the enſuing diſcourſe, it will not be amiſs here to premiſe a ſummary intimation of ſome things, that are ſuppoſed in our hypotheſis.

We ſuppoſe then firſt, without diſputing either the exiſtence or the nature of elementary air, that the common air we breathe in, and which I often call atmoſpherical air, abounds with corpuſcles not devoid of weight, and endowed with elaſticity or ſpringineſs, whereby the lower parts, compreſſed by the weight of the upper, inceſſantly endeavour to expand themſelves, by which expanſion, and in proportion to it, the ſpring of the air is weakened, (as other ſprings are wont to be) the more they are permitted to ſtretch themſelves.

Next, we ſuppoſe, that the terraqueous globe, being environed with this gravitating and ſpringy air, has its ſurface and the bodies placed on it, preſſed by as much of the atmoſphere, as either perpendicularly leans on them, or can otherwiſe come to bear upon them. And this preſſure is, by the Torricellian and other experiments, found to be equivalent to a perpendicularly erected cylinder of about twenty-nine or thirty inches of quick-ſilver, (for the height is differing, as the gravity of the atmoſphere happens to be various.)

Laſtly, we ſuppoſe, that air being contained in a pipe or other hollow body, that has but one orifice open to the free air, if this orifice be hermetically ſealed, or otherwiſe (as with the mouth of one, that ſucks) cloſed, the now included air, whilſt it continues without any farther expanſion, will have an elaſticity equivalent to the weight of as much of the outward air, as did before preſs againſt it. For if the weight of the atmoſphere, to which it was then expoſed, had been able to compreſs it further, it would have done ſo, and then the cloſing of the orifice, at which the internal and external air communicated, as it fenced the included air from the preſſure of the incumbent, ſo it hindred the ſame included air from expanding it ſelf; ſo that, as it was ſhut up with the preſſure of the atmoſphere upon it, that is in a ſtate of as great compreſſion, as the weight of the atmoſphere could bring it to, ſo, being ſhut up, and thereby kept from weakening that preſſure by expanſion, it muſt retain a ſpringineſs equipollent to the preſſure it was expoſed to before, which (as I juſt now noted) was as great, as the weight of the incumbent pillar of the atmoſphere could make it. But if, as was ſaid in the firſt ſuppoſition, the included air ſhould come to be dilated or expanded, the ſpring being then unbent, its ſpring, like that of other elaſtical bodies, would be debilitated anſwerably to that expanſion.

To me then it ſeems, that, ſpeaking in general, liquors are upon ſuction raiſed into the cavities of pipes and other hollow bodies, when, and ſo far as there is a leſs preſſure on the ſurface of the liquor, in the cavity, than on the ſurface of the external liquor, that ſurrounds the pipe, whether that preſſure on thoſe parts of the external liquor, that are from time to time impelled up into the orifice of the pipe, proceed from the weight of the atmoſphere, or the propagated compreſſion, or impulſe of ſome parts of the air, or the ſpring of the air, or ſome other cauſe, as the preſſure of ſome other body quite diſtinct from air.

Upon the general view of this hypotheſis, it ſeems very conſonant to the mechanical principles. For, if there be on the differing parts of the ſurface of a fluid body unequal preſſures, it is plain, as well by the nature of the thing, as by what has been demonſtrated by *Archimedes*, and his commentators, that the greater force will prevail againſt the leſſer, and that that part of the water's ſurface muſt give way, where it is leaſt preſſed. So that that, wherein the hypotheſis I venture to propoſe to you, differs from that, which I diſſent from, is not, that mine is leſs mechanical; but partly in this, that, whereas the hypotheſis, I queſtion, ſuppoſes a neceſſity of the protruſion or impulſe of the air, mine does not require that ſuppoſition, but, being more general, reaches to other ways of procuring the aſcenſion of liquors, without raiſing them by the impulſe of the air; and partly, and indeed chiefly, in that the hypotheſis, I decline, makes the cauſe of the aſcenſion of liquors to be only the increaſed preſſure of the air external to the pipe; and I chiefly make it to depend upon the diminiſhed preſſure of the air within the pipe, on the ſcore of the expanſion it is brought to by ſuction.

To proceed now to ſome experiments, that I made in favour of this hypotheſis, I ſhall begin with that which follows:

We took a glaſs-pipe bended like a ſyphon, but ſo, that the ſhorter leg was as parallel to the longer, as we could get it made, and was hermetically ſealed at the end: into this ſyphon we made a ſhift (for it is not very eaſy) to convey water, ſo that the crooked part being held downwards, the liquor reached to the ſame height in both the legs, and yet there was about an inch and a half of uncompreſſed air ſhut up in the ſhorter leg. This little inſtrument (for it was but about fifteen inches long) being thus prepared, it is plain, that according to the hypotheſis I diſſent from, there is no reaſon, why the water ſhould aſcend upon ſuction. For, though we ſhould admit, that the external air were conſiderably compreſſed, or received a notable impulſe, when the ſucker's cheſt is enlarged; yet in our caſe, that compreſſion, or protruſion, will not reach the ſurface of the water in the ſhorter leg, becauſe it is there fenced from the action of the external air by the ſides of the glaſs, and the hermetical ſeal of the top, and yet, if one ſucked ſtrongly at the open orifice in the longer leg, the water in the ſhorter would be depreſſed; and that in the longer aſcended at one ſuck about an inch and half: of which the reaſon is clear in our hypotheſis. For, the ſpring of the included air, together with the weight of the water in the ſhorter leg, and the preſſure of the atmoſpherical air, aſſiſted by the weight of the liquor in the longer leg, counter-balanced one another before the ſuction began: But when afterwards upon ſuction, the air in the longer leg came to be dilated, and thereby weakened, it was rendered unable to reſiſt the undiminiſhed preſſure of the air included in the ſhorter leg, which conſequently expanding itſelf by virtue of its elaſticity, depreſſed the contiguous water, and made it proportionably riſe in the oppſite leg, till, by the expanſion, its ſpring being more and more weakened, it arrived at an equipollency with the gravitation or preſſure of the atmoſphere. Which laſt clauſe contains the reaſon, why, when the perſon, that ſucked, had raiſed the water in the longer leg, leſs than three inches higher by repeated endeavours to ſuck, and that without once ſuffering the water to fall back again, he was not able to elevate the water in the longer, ſo much as three inches above its firſt ſtation. And if in the ſhorter leg, there was but an inch and a quarter of ſpace left for the air unfilled by the water, by divers ſkilfully reiterated acts of ſuction, he could not raiſe the liquor in the longer leg above two inches; becauſe, by that time, the air included in the ſhorter leg, had, by expanding itſelf further and further, proportionably weakened its ſpring, till at length it became as rarified, as was the air in the cavity of the longer leg, and conſequently was able to thruſt away the water with no more force than the air in the long leg was able to reſiſt. And by the recited trial it appeared, that the rarefaction uſually made of air by ſuction is not near ſo great, as one would expect, probably, becauſe by the dilatation of the lungs, the air being ſtill ſhut up, is but moderately rarified, and the air in the longer leg, can by them, be brought to no greater degree of rarity, than that of the air within the cheſt. For, whereas the included air in our inſtrument was not expanded, by my eſtimate, at one ſuck, to above the double of its former dimenſions, and by divers ſucceſſive ſucks was expanded but from one inch and an half, to leſs than four inches and an half, if the ſuction could have been conveniently made with a great and ſtanch ſyringe, the rarefaction of the air would probably have been far greater; ſince in our pneumatick engine air may, without heat, and by a kind of ſuction, be brought to poſſeſs many hundreds of times the ſpace it took up before. From this rarefaction of the air in both the legs of our inſtrument proceeds another phænomenon, readily explicable by our hypotheſis. For if, when the water was impelled up as high as the ſuction could raiſe it, the inſtrument were taken from the ſucker's mouth, the elevated water would with violence return to its wonted ſtation. For, the air, in both the legs of the inſtrument, having by the ſuction loſt much of the ſpring, and ſo of its power of preſſing; when once the orifice of the longer leg was left open, the atmoſpherical air came again to gravitate upon the water in that leg, and the air, included in the other leg, having its ſpring debilitated by the precedent expanſion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thruſt into the ſpace it poſſeſſed before the ſuction; in which ſpace it had denſity and elaſticity enough to reſiſt the preſſure, that the external air exerciſed againſt it through the interpoſed water.

But our hypotheſis about the cauſe of ſuction would not need to be ſollicitouſly proved to you by other ways, if you had ſeen what I have ſometimes been able to do in our pneumatick engine. For, there we found, by trials purpoſely deviſed, and carefully made, that a good ſyringe being ſo conveyed into our receiver, that the open orifice of the pipe or lower part was kept under water, if the engine were exhauſted, though the handle of the ſyringe were drawn up, the water would not follow it, which yet it would do, if the external air were let in again. The reaſon of which is plain in our hypotheſis; for the air, that ſhould have preſſed upon the ſurface of the ſtagnant water, having been pumped out, there was nothing to impel up the water into the deſerted cavity of the ſyringe, as there was, when the receiver was filled with air.

CHAP. IV.

BUT becauſe ſuch a conveniency as our engine, and the apparatus neceſſary for ſuch trials, are not eaſily procurable, I ſhall endeavour to confirm our hypotheſis about ſuction, by ſubjoining ſome experiments, that may be tried without the help of that engine, for the making out theſe three things:

I. THAT a liquor may be raiſed by ſuction, when the preſſure of the air, neither as it has weight nor elaſticity, is the cauſe of the elevation.

II. THAT the weight of the atmoſpherical air is ſufficient to raiſe up liquors in ſuction.

III. THAT, in ſome caſes, ſuction will not be made, as, according to the hypotheſis I diſſent from, it ſhould, although there be a dilatation of the ſucker's thorax, and no danger of a vacuum, though the liquor ſhould aſcend.

AND firſt, to ſhew, how much the riſing of liquors in ſuction depends upon the weight or preſſure of the impellent body, and how little neceſſity there is, where that preſſure is not wanting, that, in the place deſerted by the liquor, that is ſucked there ſhould ſucceed air, or ſome other viſible body, as the Peripatetic ſchools would have it; to ſhew this, I ſay, I thought on the following experiments. We took a glaſs pipe, fit to have the Torricellian experiment made with it, but a good deal longer than was neceſſary for that uſe: this pipe being hermetically ſealed at one end, the other end was ſo bent, as to be reflected upwards, and make as it were the ſhorter leg of the ſyphon as parallel as we could to the longer, ſo that the tube now was ſhaped like an inverted ſyphon, with legs of a very unequal length. This tube, notwithſtanding its inconvenient figure, we made a ſhift (for it is not eaſily done) to fill with mercury, when it was in an inclined poſture, and then erecting it, the mercury ſubſided in the longer leg, as in the Torricellian experiment, and attained to between two foot and a quarter and two foot and an half above the ſurface of the mercury in the ſhorter leg, which in this inſtrument anſwers to the ſtagnant mercury in an ordinary barometer, from which to diſtinguiſh it, I have elſewhere called this ſyphon, furniſhed with mercury, a travelling baroſcope, becauſe it may be ſafely carried from place to place. Out of the ſhorter leg of this tube, we warily took as much mercury, as was thought convenient for what we had further to do; and this we did by ſuch a way, as to hinder any air from getting into the deſerted cavity of the longer leg, by which means the mercurial cylinder (eſtimated as I lately mentioned) retained the ſame height above the ſtagnant mercury in the ſhorter: the upper and cloſed part of this travelling baroſcope, you will eaſily grant to have been free from common air, not only for other reaſons, that have been given elſewhere, but particularly for this, that if you gently incline the inſtrument, the quickſilver will aſcend to the top of the tube; which you know it could not do, if the place, formerly deſerted by it, were poſſeſſed by the air, which, by its ſpring, would hinder the aſcenſion of the mercury, (as is eaſy to be tried.) The inſtrument having been thus fitted, I cauſed one of the by-ſtanders to ſuck at the ſhorter leg, whereupon (as I expected) there preſently enſued an aſcenſion of four or five inches of mercury in that leg, and a proportionable ſubſidence of the mercury in the longer, and yet in this caſe the raiſing of the mercury cannot be pretended to proceed from the preſſure of the air. For the weight of the atmoſphere is fenced off by that, which cloſes the upper end of the longer tube, and the ſpring of the air has here nothing to do, ſince, as we have lately ſhewn, the ſpace deſerted by the mercury is not poſſeſſed by the included air, and the pulſion or condenſation of the air, ſuppoſed, by divers modern philoſophers, to be made by the dilatation of the ſucker's cheſt, and to preſs upon the ſurface of the liquors, that are to be ſucked up: this, I ſay, cannot here be pretended, in regard the ſurface of the liquor in the longer leg is every way fenced from the preſſure of the ambient air. So that it remains, that the cauſe, which raiſed the quickſilver in the ſhorter leg, upon the newly recited ſuction, was the weight of the collaterally ſuperior quickſilver in the longer leg, which being (at the beginning of the ſuction) equivalent to the weight of the atmoſphere, there is a plain reaſon, why the ſtagnant mercury, in the ſhorter leg, ſhould be raiſed ſome inches by ſuction; as mercury, ſtagnant in an open veſſel, will be raiſed by the weight of the atmoſphere, when the ſuction is made in the open air. For, in both caſes, there is a pipe, that reaches to the ſtagnant mercury, and a competent weight to impel it into the pipe; when the air in the cavity of the pipe has its ſpring weakened by the dilatation, that accompanied ſuction.

THE ſecond point formerly propoſed, which is, that the weight of the air is ſufficient to raiſe liquors in ſuction, may not be ill proved by arguments, legitimately drawn from the Torricellian experiment itſelf, and much more clearly by the firſt and fifteenth of our continued phyſico-mechanical experiments. And therefore I ſhall only here take notice of a phaenomenon, that may be exhibited by the travelling baroſcope, which, though it be much inferior to the experiments newly referred to, may be of ſome uſe on the preſent occaſion.

HAVING then provided an inſtrument like the travelling baroſcope, mentioned under the former head, but whoſe legs were not ſo unequally long, and having in it made the Torricellian experiment, after the manner lately deſcribed, we ordered the matter ſo, that there remained in the ſhorter leg the length of divers inches unfilled with ſtagnant mercury. Then I cauſed one, verſed in what he was to do, ſo to raiſe the quickſilver by ſuction to the open orifice of the ſhorter leg, that the orifice being ſeaſonably and dexterouſly cloſed, the mercury continued to fill that leg, as long as we thought fit; and then, having put a mark to the ſurface of the mercury in the longer leg, we unſtopped the orifice of the ſhorter; whereupon the mercury, that before filled it, was depreſſed, till the ſame liquor in the longer leg was raiſed five inches or more above the mark, and continued at that height. I ſaid, that the mercury, that had been raiſed by ſuction, was depreſſed, rather than that it ſubſided, becauſe its own weight could not here make it fall, ſince a mercurial cylinder of five inches was far from being able to raiſe ſo tall a cylinder of mercury, as made a counterpoiſe in the longer leg; and therefore the depreſſion we ſpeak of, is to be referred to the gravitation of the atmoſpherical air upon the ſurface of the mercury in the ſhorter leg: and I ſee no cauſe to doubt, but that, if we could have procured an inſtrument, into whoſe ſhorter leg a mercurial cylinder of many inches higher could have been ſucked up, it would, by this contrivance, have appeared, that the preſſure of the atmoſphere would eaſily impel up a far taller cylinder of mercury, than it did in our recited experiment.

THAT this is no groundleſs conjecture, may appear probable by the experiment you will preſently meet with. For, if the gravity of an incumbent pillar of the atmoſphere be able to compreſs a parcel of included air, as much as a mercurial cylinder, equivalent in weight to between thirty and five and thirty foot of water, is able to condenſe it, it cannot well be denied, that the ſame atmoſpherical cylinder may be able, by its weight, to raiſe and counter-balance eight or nine and twenty inches of quickſilver, or an equivalent pillar of water in tubes, where the reſiſtance of theſe two liquors, to be raiſed and ſuſtained by the air, depends only upon their own unaſſiſted gravity.

To confirm our doctrine of the gravitation of the atmoſphere upon the ſurface of the liquors expoſed to it, I will ſubjoin an experiment, that I deviſed to ſhew, that the incumbent air, in its natural or uſual ſtate, would compreſs other air not rarified, but in the like natural ſtate, as much as a cylinder of eight or nine and twenty inches of mercury would condenſe or compreſs it.

IN order to the making of this, I muſt put you in mind of what I have ſhewn elſewhere* at large, and ſhall further confirm by one of the experiments, that follows the next; namely, that about twenty-nine or thirty inches of quick-ſilver will compreſs air, that being in its natural or uſual ſtate (as to rarity and denſity) has been ſhut up in the ſhorter leg of our travelling or ſyphon-like baroſcope, into half the room, that included air poſſeſſed before. This premiſed, I paſs on to my experiment, which was this:

WE provided a travelling baroſcope, wherein the mercury in the longer leg was kept ſuſpended by the counterpoiſe of the air, that gravitated on the ſurface of the mercury in the ſhorter leg, which we had ſo ordered, that it reached not by about two inches to the top of the ſhorter leg. Then making a mark at the place, where the ſtagnant mercury reſted, it was manifeſt, according to our hypotheſis, that the air in the upper part of the ſhorter leg was in its natural ſtate, or of the ſame degree of denſity with the outward air, with which it freely communicated at the open orifice of the ſhorter leg; ſo that this ſtagnant air was equally preſſed upon by the weight of the collaterally ſuperior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmoſphere. Things being in this poſture, the upper part of the ſhorter leg, which had been before purpoſely drawn out to an almoſt capillary ſmallneſs, was hermetically ſealed, which, though the inſtrument was kept erected, was ſo nimbly done by reaſon of the ſlenderneſs of the pipe, that the included air did not appear to be ſenſibly heated, though for greater caution we ſtaid a while from proceeding, that, if any rarefaction had been produced in the air, it might have time to loſe it again. This done, we opened the lower end of the longer leg, (which had been ſo ordered before, that we could eaſily do it, and without concuſſion of the veſſel,) by which means the atmoſpherical air, gaining acceſs to the mercury included in the longer leg, did, as I expected, by its gravitation upon it, ſo compreſs the air included in the ſhorter leg, that, according to the eſtimate we made with the help of a ruler, (for by reaſon of the conical figure of the upper part of the glaſs we could not take preciſe meaſures,) it was thruſt into near half the room it took up before, and conſequently, according to what I put you lately in mind of, endured a compreſſion like that, which a mercurial cylinder of about twenty nine inches would have given it.

THIS experiment, as to the main of it, was for greater caution made the ſecond time with much the like ſucceſs; and though it had been more eaſy to meaſure the condenſation of the air, if, inſtead of drawing out and ſealing up the ſhorter leg of the inſtrument, we had contented our ſelves to cloſe it ſome other way; yet we rather choſe to employ *Hermes*'s ſeal, leſt, if any other courſe had been taken, it might be pretended, that ſome of the included air, when it began to be compreſſed, might eſcape out at the not perfectly and ſtrongly cloſed orifice of the leg, wherein it was impriſoned.

TO make it yet further appear, how much the aſcenſion of liquors by ſuction depends upon preſſure, rather than upon nature's imaginary abhorrence of a vacuum, or the propagated pulſion of the air; I will ſubjoin an inſtance, wherein that preſumed abhorrence cannot be pretended. The experiment was thus made:

A glaſs-ſiphon, like thoſe lately deſcribed, with one leg far longer than the other, was hermetically ſealed at the ſhorter leg, and then by degrees there was put in, at the orifice of the longer leg, as much quick-ſilver, as by its weight ſufficed to compreſs the air in the ſhorter leg into about half the room it poſſeſſed before; ſo that, according to the Peripatetick doctrine, the air muſt be in a ſtate of preternatural condenſation, and, that to a far greater degree, than (as I have tried) it is uſually brought to by cold, intenſe enough to freeze water. Then meaſuring the heighth of the quick-ſilver in the longer tube above the ſuperficies of that in the ſhorter, we found it not exceed thirty inches. Now, if liquors did riſe in ſuction *ob fugam vacui*, there is no reaſon, why this quick-ſilver in the longer part of the ſiphon ſhould not eaſily aſcend upon ſuction, at leaſt till the air in the ſhorter leg had regained its former dimenſions, ſince it cannot in this place be pretended, that, if the mercury ſhould aſcend, there would be any danger of a vacuum in the ſhorter leg of the tube, in regard, that the contiguous included air is ready at hand to ſucceed, as faſt as the mercury ſubſides in the ſhorter leg of the ſiphon. Nor can it be pretended, that, to fill the place deſerted by the quick-ſilver, the included air muſt ſuffer a preternatural rarefaction or deſcenſion; ſince it is plain in our caſe, that on the contrary, as long as the air continues in the ſtate, whereto the weight of the quick-ſilver has reduced it, it is kept in a violent ſtate of compreſſion; ſince in the ſhorter leg it was in its natural ſtate, when the mercury, poured into the longer leg, did by its weight thruſt it into about half the room it took up before. And yet, having cauſed ſeveral perſons, one of them verſed in ſucking, to ſuck divers times as ſtrongly as they could, they were neither of them able, not ſo much as for a minute of an hour, to raiſe the mercury in the longer leg, and make it ſubſide in the ſhorter for more than about an inch at moſt. And yet to ſhew you, that the experiment was not favourably tried for me, the height of the mercurial cylinder in the longer leg above the ſurface of that in the ſhorter leg was, when the ſuction was tried, an inch or two ſhorter than thirty inches, and the compreſſed air in the ſhorter leg was ſo far from having been by the exſuction expanded beyond its natural and firſt dimenſions, that it did not, when the contiguous mercury ſtood as low as we could make it ſubſide, regain ſo much as one half of the ſpace it had loſt by the precedent compreſſion, and conſequently was in a preternatural ſtate of condenſation, when it had been freed from that ſtate as far as ſuction would do it. Whence it ſeems evident, that it was not *ob fugam vacui*, that the quick-ſilver did upon ſuction aſcend one inch; for, upon the ſame ſcore it ought to have aſcended two, or perhaps more inches, ſince there was no danger, that by ſuch an aſcenſion any vacuum ſhould be produced or left in the ſhorter leg of the ſiphon; whereas, according to our hypotheſis, a clear cauſe of the phænomenon is aſſignable. For, before the ſuction was begun, there was an æquilibrium, or equipollency, between the weight of the ſuperior quick-ſilver in the longer leg, and a ſpring of the compreſſed air included in the ſhorter leg; but when the experimentor began to ſuck, his cheſt being widened, part of the air included in the upper part of the longer leg paſſed into it, and that, which remained, had by that expanſion its preſſure ſo weakened, that the air in the ſhorter leg, finding no longer the former reſiſtance, was able by its own ſpring to expand itſelf, and conſequently to depreſs the contiguous mercury in the ſame ſhorter leg, and raiſe it as much in the longer.

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* See the Author's Defence of the Doctrine touching the Spring and Weight of the Air, againſt *Fr. Linus*, chap. v.

But here a hydroſtatician, that heedfully marks this experiment, may diſcern a difficulty, that may perhaps ſomewhat perplex him, and ſeems to overthrow our explication of the phænomenon. For he may object, that if the compreſſed air in the ſhorter leg had a ſpring equipollent to the weight of the mercury in the longer leg, it appears not, why the mercury ſhould not be ſucked up in this inſtrument, as well as in the free air; ſince, according to me, the preſſure of the included air upon the ſubjacent mercury muſt be equivalent to the weight of the atmoſphere, and yet experience ſhews, that the weight of the atmoſphere will, upon ſuction, raiſe quickſilver to the height of ſeveral inches.

To clear this difficulty, and ſhew, that, though it be conſiderable, it is not at all inſuperable, be pleaſed to conſider with me, that I make indeed the ſpring of the compreſſed air to be equipollent to the weight of the compreſſing mercury, and I have a manifeſt reaſon to do it; becauſe, if the ſpring of the air were not equipollent to that weight, the mercury muſt neceſſarily compreſs the air farther, which it is granted *de facto* not to do. But then I conſider, that in our caſe there ought to be a great deal of difference between the operation of the ſpring of the included air and the weight of the atmoſphere, after ſuction has been once begun. For the weight of the atmoſphere, that impels up mercury and other liquors, when the ſuction is made in the open air, continues ſtill the ſame; but the force or preſſure of the included air is equal to the counterpreſſure of the mercury, no longer than the firſt moment of the ſuction; after which, the force of the impriſoned air ſtill decreaſes more and more, ſince this compreſſed air, being further and further expanded, muſt needs have its ſpring proportionably weakened; ſo that it need be no wonder, that the mercury was not ſucked up any more than we have related; for there was nothing to make it aſcend to a greater height, than that, at which the debilitated ſpring of the (included but) expanded air was brought to an equipollency with the undiminiſhed, and indeed ſomewhat encreaſed weight of the mercurial cylinder in the longer leg, and the preſſure of the aerial cylinder in the ſame leg, leſſened by the action of him, that ſucked. For whereas, when the orifice of this leg ſtood open, the mercury was preſſed on by a cylinder of the atmoſpherical air, equivalent to about thirty inches of quick-ſilver; by the mouth and action of him, that ſucked the tube, was freed from the external air, and by the dilatation of his thorax, the neighbouring air, that had a free paſſage through his wind-pipe to it, was proportionably expanded, and had its ſpring and preſſure weakened: by which means, the compreſſed air in the ſhorter leg of the ſyphon was enabled to impel up the mercury, until the lately mentioned equilibrium or equipollency was attained. And I muſt here take notice, that, as the quick-ſilver was raiſed by ſuction but a little way, ſo the cylinder, that was raiſed, was a very long one; whereas, when mercury is ſucked up in the free air, it is ſeldom raiſed to half that length; though, as I noted before, the impellent cauſe, which is the weight of the atmoſphere, continued ſtill the ſame, whereas in our ſyphon, when the mercury was ſucked up but an inch, the compreſſed air, poſſeſſing double the ſpace it did before, had by this expanſion already loſt a very conſiderable part of its former ſpring and preſſure.

I ſhould here conclude this diſcourſe, but that I remember a phænomenon of our pneumatick engine, which to divers learned men, eſpecially Ariſtotelians, ſeemed ſo much to argue, that ſuction is made either by a *fuga vacui*, or ſome internal principle, that divers years ago I thought fit to ſet down another account of it, and lately meeting with that account among other papers, I ſhall ſubjoin it juſt as I found it, by way of appendix to the foregoing Tract.

AMONG the more familiar phænomena of the *Machina Boyliana*, as they now call it, none leaves ſo much ſcruple in the minds of ſome ſorts of men, as this, that when one's finger is laid cloſe upon the orifice of the little pipe, by which the air is wont to paſs from the receiver into the exhauſted cylinder, the pulp of the finger is made to enter a good way into the cavity of the pipe, which doth not happen without a conſiderable ſenſe of pain in the lower part of the finger. For moſt of thoſe, that are ſtrangers to hydroſtaticks, eſpecially if they be prepoſſeſſed with the opinions generally received, both in the Peripatetick and other ſchools, perſuade themſelves, that they feel the newly mentioned and painful protuberance of the pulp of the finger, to be effected, not by preſſure, as we would have it, but diſtinctly by attraction.

To this we are wont to anſwer, that common air being a body not devoid of weight, the phænomenon is clearly explicable by the preſſure of it: for, when the finger is firſt laid upon the orifice of the pipe, no pain nor ſwelling is produced, becauſe the air, which is in the pipe, preſſes as well againſt that part of the finger, which covereth the orifice, as the ambient air doth againſt the other parts of the ſame finger. But when by pumping, the air in the pipe, or the moſt part of it, is made to paſs out of the pipe into the exhauſted cylinder, then there is nothing left in the pipe, whoſe preſſure can any thing near countervail the undiminiſhed preſſure of the external air on the other parts of the finger; and conſequently, that air thruſts the moſt yielding and fleſhy part of the finger, which is the pulp, into that place, where its preſſure is unreſiſted, that is, into the cavity of the pipe, where this forcible intruſion cauſeth a pain in thoſe tender parts of the finger.

To give ſome viſible illuſtration of what we have been ſaying, as well as for other purpoſes, I thought on the following experiment.

WE took a glaſs pipe of a convenient length, and open at both ends, whoſe cavity was near about an inch in diameter, (ſuch a determinate breadth being convenient, though not neceſſary:) to one of the ends of this pipe we cauſed to be firmly tied on a piece of very fine bladder, that had been ruffled and oiled, to make it both very limber and unapt to admit water; and care was taken, that the piece of bladder tied on ſhould be large enough, not only to cover the orifice, but to hang looſe ſomewhat beneath it.

THIS done, we put the covered end of the pipe into a glaſs-body, or cucurbit, purpoſely made more than ordinarily tall, and the pipe being held in ſuch manner, as that the end of it reached almoſt, but not quite, to the bottom of the glaſs-body, we cauſed water to be poured, both into this veſſel, and into the pipe (at its upper orifice, which was left open) that the water might aſcend equally enough, both without and within the pipe. And when the glaſs-body was full of water, and the ſame liquor was level to it, or a little higher within the pipe, the bladder at the lower orifice was kept plump, becauſe the water within the pipe did, by its weight, preſs as forcibly downwards, as the external water in the large glaſs endeavoured to preſs it inwards and upwards.

ALL this being done, we cauſed part of the water in the pipe to be taken out of it, (which may be done either by putting in and drawing out a piece of ſpunge or of linnen, or more expeditiouſly by ſucking up part of the water with a ſmaller pipe to be immediately after laid aſide;) upon which removal of part of the internal water, that, which remained in the pipe, being no longer able, by reaſon of its want of weight, to preſs againſt the inſide of the bladder near as forcibly as it did before, the external water, whoſe weight was not leſſened, preſſed the ſides and bottom of the bladder, whereto it was contiguous into the cavity of the pipe, and thruſted it up therein ſo ſtrongly, that the diſtended bladder made a kind of either thimble or hemiſphere within the pipe. So that here we have a protuberance, like that above-mentioned of the finger, effected by pulſion, not attraction; and in a caſe, where there can be no juſt pretence of having recourſe to nature's abhorrence of a vacuum, ſince the upper orifice of the pipe being left wide open, the air may paſs in and out without reſiſtance.

THE like ſwelling of the bladder in the pipe we could procure without taking out any of the internal liquor, by thruſting the pipe deeper into the water; for then the external liquor having, by reaſon of its increaſe of depth, a greater preſſure on the outſide of the bladder, than the internal liquor had on the inſide of it, the bladder muſt yield to the ſtronger preſſure, and conſequently be impelled up.

IF the bladder lying looſe at the lower end of the pipe, the upper end were carefully cloſed with one's thumb, that the upper air might not get out, until the experimentor thought fit, and if the thus cloſed pipe were thruſt almoſt to the bottom of the water, the bladder would not be protuberant inwards, as formerly; becauſe the included air, by virtue of its ſpring, reſiſted from within the preſſure of the external water againſt the outſide of the bladder: but if the thumb, that ſtopped the pipe's upper orifice, were removed, the formerly compreſſed air having liberty to expand itſelf, and its elaſticity being weakened thereby, the external water would with ſuddenneſs and noiſe enough, not to be unpleaſant to the ſpectators, drive up the bladder into the cavity of the pipe, and keep it there very protuberant.

To obviate an objection, that I foreſaw might be brought in by perſons not well verſed in hydroſtaticks, I cauſed the pipe fore-mentioned, or ſuch another, to be ſo bent near the lower end, as that the orifice of it ſtood quite on one ſide, and the parts of the pipe made an angle as near to a right one, as he, that blew it, could bring it to. This lower orifice being fitted with a bladder, and the pipe, with its contained liquor, being thruſt under water after the former manner, the lateral preſſure of the water forced the bladder into the ſhort and horizontal leg, and made it protuberate there, as it had done when the pipe was ſtraight.

Lastly, that the experiment might appear not to be confined to one liquor; inſtead of water, we put into the unbent pipe, as much red wine (whoſe colour would make it conſpicuous) as was requiſite to keep the bladder ſomewhat ſwelling outwards, when it was ſomewhat near the bottom of the water; and then it was manifeſt, that, according as we had foreſeen, the ſuperficies of the red liquor in the pipe was a good deal higher than that of the external water, and if the depth of both liquors were proportionably leſſened, the difference of height betwixt the two ſurfaces would indeed, as it ought to happen, decreaſe, but ſtill the ſurface of the wine would be the higher of the two, becauſe, being lighter in ſpecie than the common water, the æquilibrium between the preſſures of the two liquors upon the bladder would not be maintained, unleſs a greater height of wine compenſated its defect of ſpecifick gravity. And if the pipe was thruſt deeper into the water, then the bladder would be made protuberant inwards, as when the pipe had water in it. By which it appears, that theſe phænomena, without recourſe to attraction, may be explicated barely by the laws of the æquilibrium of liquors.

NEW EXPERIMENTS
ABOUT THE
PRESERVATION OF BODIES
IN VACUO BOYLIANO.

PREFACE.

MY willingneſs to make the bulk of the papers about the hidden qualities of the air leſs inconſiderable, by things, that were of affinity to the ſubject, inducing me to tumble over ſome of my *adverſaria*, I met among them with divers looſe notes, or ſhort memorials of ſome experiments I made ſeveral years ago (and ſome of a freſher date) about the preſervation of bodies by excluding the air. Wherefore I was eaſily perſuaded to ſubjoin theſe to the additional experiments laſt recited. For it ſeems not yet clear, by what manifeſt quality the excluſion of the air ſhould ſo much contribute to keep from putrefaction variety of bodies, that are uſually found very much diſpoſed to it. And therefore, till the cauſe of this preſervation be further penetrated, it may not be altogether impertinent to mention ſome experiments relating to it. And though theſe be only ſuch, as come now to hand, and were moſt of them ſet down rather as notes than relations; yet being faithfully regiſtered, and moſt of them having been made in *Vacuo Boyliano* (as they call it) they will probably be new, and ſo perhaps not altogether uſeleſs to naturaliſts, who may vary them, and requite me for them, by trying the ſame experiments, I made by the removal of the air, by the bare excluſion of adventitious air. For ſometimes through haſte I did not, and ſometimes for want of conveniency I could not try, whether the ſame phænomena would appear, if the ſame bodies were ſhut up with air in them, provided they were diligently kept from all commerce with the air about them.

NEW EXPERIMENTS, &c.

EXPERIMENT I.

A PIECE of roaſted rabbet, being exactly cloſed up in an exhauſted receiver the ſixth of *November*, was two months, and ſome few days after taken out, without appearing to be corrupted, or ſenſibly altered in colour, taſte, or ſmell.

EXPERIMENT II.

A SMALL glaſs-receiver, being half filled with pieces of white-bread, (part cruſt and part crumb) was exhauſted, and ſecured the eleventh of *March*: the receiver being opened the firſt of *April*, part of the bread was ſhaken out, and appeared not to have been conſiderably, if at all ſenſibly, impaired in that time, ſave, that the outſide of ſome pieces of crumb ſeemed to be a little, and but a little, leſs ſoft and white than before. There appeared no drops, or the leaſt dew on the inſide of the glaſs. The remaining bread was again ſecured ſoon after.

THE eighteenth of *April*, the bread was taken out again, and taſted much as it did the laſt time, the cruſt being alſo ſoft, and no drops of water appearing on the inſide of the glaſs.

EXPERIMENT III.

THIS day (being the ninth of *March*) I opened a ſmall exhauſted and ſecured receiver, wherein, about the ninth of *December*, that is, about three months ago, we had included ſome milk: upon opening an acceſs to the air, we found the milk well coloured, and turned partly into a kind of whey, and partly into a kind of ſoft curd. The taſte was not offenſive, only a little ſouriſh like whey, and the ſmell was not at all ſtinking, but ſomewhat like that of ſouriſh milk.

EXPERIMENT IV.

THE violet-leaves, that were put up, and freed and ſecured from air the fifth of *March*, being this day opened, (*April* the ſeventh) appeared not to have changed their ſhape, or colour, or conſiſtence: for, as for their odour, it could not be well judged of, becauſe he that included them had, for his own eaſe, contrary to my expreſs direction, cruſhed many of them together in thruſting them down; and by ſuch a violation of their texture, it is natural for violets to loſe their fragrancy, and acquire an earthy ſmell.

EXPERIMENT V.

HAVING carefully placed ſome violets in an exhauſted receiver, of a convenient ſize and bigneſs, and ſecured it from immediate commerce with the external air; the ſeventh month after we looked upon them again, and found they were not putrified, or reſolved into any mucilaginous ſubſtance, but kept their ſhape entire, ſome of them retaining their colour, but more of them having ſo loſt it, as to look like white violets.

EXPERIMENT VI.

NOVEMBER the fifth, we conveyed into a convenient ſhaped receiver ſome ounces of ſheep's-blood, taken from an animal, that had been killed that afternoon. And after the exhauſtion of the air, during which ſtore of bubbles were generated in the liquor, that made it ſwell notably, the included blood was kept in a place, (whoſe warmth we judged equal to that of a digeſtive furnace) for twenty days; for one or two of the firſt of which, the blood ſeem to continue fluid, and of a florid colour, which afterwards degenerated into one, that tended more to blackneſs. On the twenty-fifth of *November*, we came to ſet in the external air, and found it to ruſh into the receiver, and the glaſs containing the blood, being held in a lightſome place, the moſt part of the bottom of it ſeemed to be thinly overlaid with a coagulated ſubſtance, of a higher colour than that, which ſwarm above it, which yet, though it appeared dark, and almoſt blackiſh in the glaſs, whilſt it was looked on in the bulk, yet, if it was ſhook, thoſe parts of it, that fell down along the inſide of the glaſs, appeared of a deep, but fair colour. But whilſt the blood continued in the glaſs, it was ſuppoſed not to ſtink, ſince, even when it was poured out, though its ſmell ſeemed to me (whoſe organs of ſmelling are tender) to have I know not what, that was offenſive, yet to others it ſeemed to ſmell but as the blood of a newly killed dog.

EXPERIMENT VII.

SOME cream being put up and ſecured the ſeventeenth of *March*, in an exhauſted receiver, did this day appear to be more thick, and almoſt butter-like at the top (whoſe ſuperficies ſeemed rugged) than otherwhere; and afterwards by being well ſhaken together in the not inconveniently ſhaped glaſs, was eaſily enough reduced to butter, whoſe butter-milk, by the judgment of thoſe, who were more uſed to deal in it than I, appeared not differing from ordinary butter-milk. And I found it had, like that, a grateful ſourneſs. The butter was judged to be a little ſourer than ordinary, but was not, as they ſpeak, made.

[IN the entry of this experiment, blanks were left for the years; but the tenor of the words, and deſign of the experiment, and other circumſtances, aſſure me, that the cream continued a year in the veſſel.]

EXPERIMENT VIII.

FEBRUARY the eighteenth, we looked again upon three vials, that had been exhauſted and ſecured the fifteenth of *September* laſt; the one of theſe had in it ſome ſlices of roaſted beef, and the other ſome ſhivers of white bread, and the laſt ſome thin pieces of cheeſe; all which ſeemed to be free from putrefaction, and looked much as they did, when they were firſt put up: wherefore we thought not fit to let the air into the receiver, but left them as they were, to lengthen the deſigned trial.

EXPERIMENT IX.

FEBRUARY the eighteenth, there was a fourth vial, wherein, about ſix months before, viz. *Auguſt* the twelfth, had been incloſed and ſecured ſome July flowers and a roſe; and yet theſe being kept in the ſame place with the reſt, though they ſeemed a little moiſt, retained their ſhape and colour, eſpecially the roſe, which looked freſh enough to ſeem to have been gathered but lately.

N.B. THAT we obſerved not in any of theſe four receivers any great drops, or ſo much as dew in the upper parts, viz. thoſe, that were ſituated above the included matter.

EXPERIMENT X.

JUNE the fourth, we left ſome ſtrawberries in an exhauſted receiver, and coming to look upon them after the beginning of *November*, we found them to be diſcoloured, but not altered in ſhape, nor affording any ſign of corruption, by being at all mouldy: wherefore we thought fit to leave them ſtill in the receiver for further trial.

EXPERIMENT XI.

MAY the ſecond, 1669, a piece of roaſted beef, ſecured *September* the fifteenth, appeared to be not at all altered: as did likewiſe a piece of cheeſe ſecured in another receiver; and ſome pieces of a French roſe the ſame day (*September* the fifteenth) ſecured in a third.

N.B. THE flowers ſealed up *Auguſt* the twelfth, 1668, being this day looked upon, appeared freſh, and conſequently did ſo, after having been kept eight months and an half.

EXPERIMENT XII.

THERE was taken beer of eight ſhillings a barrel, of a year old, near a pint of which, *June* the ſeventeenth, was put into a convenient ſhaped glaſs, and it was afterwards exhauſted and ſecured from the air; the moſt part of the month of *Auguſt* proved extraordinarily hot. Towards the latter end, there was, at ſeveral times, great thunder, which made the beer in our cellar, and in moſt of thoſe of the neighbourhood, turn ſour. The firſt of *September* the beer was opened, but did not ſeem to have degenerated into any ſourneſs.

EXPERIMENT XIII.

BEING deſirous to try, whether the thunder would have ſuch effect upon ale, exactly ſtopped in glaſs veſſels, as it often has on that liquor in the ordinary wooden caſks, I cauſed ſome ale, moderately ſtrong, to be put into a conveniently ſhaped receiver; and having exhauſted the air, and ſecured a glaſs veſſel, it was put into a quiet, but not cool, place; laſt week, which was about ſix weeks after the liquor had been incloſed, there happening ſome very loud thunder, and our beer, though the caſk was kept in a good cellar, being generally noted to have been turned ſour after this thunder; I ſtaid yet a day or two longer, that the operation upon our included liquor might be the more certain and manifeſt; and then, permitting an acceſs to the outward air, we took out the ale, and found it to be good drink, and not at all ſoured.

COMPARE this with the wiſh made in the Eſſay of the great efficacy of effluviums, chap. V. that ſuch an experiment ſhould be tried.

EXPERIMENT XIV.

SEPTEMBER the twenty-firſt, 1670, ſome blackberries, included in an exhauſted receiver, were opened *June* the twentieth, 1673, and were found free from all mouldineſs and ill-ſcent; only there was found ſome liquor, that was ſour, which being taken out, the berries were ſecured again.

[AT the ſame time, was another parcel of the ſame berries exactly cloſed up in a receiver, whence the air was not pumped, to try what difference in the event would appear by this variation. But, coming in *October* the eleventh, 1673, to look upon the glaſs, we found it cracked, and the fruit all covered over with a thick mould. Nor was this the only veſſel, wherein trials, made to preſerve fruits, without any exhauſtion of the air, miſcarried.]

OCTOBER the eleventh, 1674, the ſame berries, being looked upon, appeared to have their colour altered, and much leſs black than before; but did not appear putrefied by either loſs of ſhape, or by any ſtinking ſmell, nor was the leaſt mouldineſs obſerved to be on them, though they had been kept in the ſame receiver above four year.

THAT *fructus horarii*, eſpecially ſo tender and juicy ones, ſhould, without any additament, be preſerved from putrefaction ſo many times longer than otherwiſe they would have laſted; as it is more than would be expected, ſo it may give hopes, that both odd and uſeful things of this kind, may be this way performed.

POSTSCRIPT.

THE foregoing experiments, as the memorials themſelves declare, were all of them made *in vacuo Boyliano*, nor did I intend to ſet down any other: but meeting, among thoſe memorials, with a ſhort account of a couple of trials made without the help of our pneumatic engine, I was induced to annex them, becauſe many may make the like, that will not be able to make ſuch as have been hitherto recited. And theſe two requiring no peculiarly ſhaped veſſels, it it thought, it may prove of ſome oeconomical, as well as phyſical uſe; if it be ſhewn by experience, that liquors hermetically ſealed the ordinary way in common bolt-heads, may be kept from ſouring very much beyond their uſual time of laſting.

JUNE the fourteenth, we put a convenient quantity of good ale into a bolt-head, and ſealed it up hermetically; the next year, on the fifth of *July*, we broke off the ſeal, and found the liquor very good, and without any ſenſible ſourneſs. The next day it was ſealed up again, and ſet by for thirteen months, at which time the neck of the glaſs being broken, the ale was found pretty ſour, and therefore the trial was proſecuted no farther: ſo that, though this liquor would not by this way of preſervation be kept from ſouring ſo long as the wine, to be mentioned in the following experiment, yet even a ſmall quantity of it was preſerved good at the leaſt above a year, which is very much longer than ale is wont to keep from ſouring.

JUNE the fourteenth, 1670, in a large bolt-head was hermetically ſealed up about a pint, by gueſs, of French claret wine, which, when we came to look upon, *July* the fifth, 1671, appeared very clear and high coloured, and had depoſited ſtore of feces at the bottom of the glaſs, but faſtened no tartar, that we could perceive to the ſides. Upon the breaking of the ſealed end of the glaſs, the by-ſtanders thought, that there was an eruption of included air or ſteams, and, above the ſurface of the wine, there appeared, to a pretty height, a certain white ſmoke almoſt like a miſt, and then gradually vaniſhed: the wine continued well-taſted, and was a little rough upon the tongue, but not at all ſour.

THE bolt-head was ſealed up again *July* the ſixth, 1671, and ſo ſet by till *Auguſt* the fifth, 1672, at which time it was opened again, and then the wine did ſtill taſte very well.

JUNE the twenty-ſixth, 1673, the bolt-head, with the ſame claret wine, was opened, and was found very good, and was ſealed up again.

OCTOBER the eleventh, 1674, the ſame claret wine was opened again, and appeared of a good colour, not ſour, but ſeemed ſomewhat leſs ſpiritous than other good claret wine, perhaps becauſe of the cold weather.

THIS, and the foregoing trial about the preſervation of ale, were made in Mr. *Oldenburg*'s houſe, and preſence.

An Account of the Two Sorts of the HELMONTIAN LAUDANUM, together with the Way of the Noble Baron *F. M. van Helmont*, (Son to the famous *Johannes Baptiſta)* of Preparing his LAUDANUM.

First publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. cvii. p. 147, for *October* 26, 1674.

AS for the Helmontian Laudanum, you may uſe your own liberty in ſuſpecting the receipts, that go about of it. For the name itſelf ſeems ambiguous to me, who am well informed, that there are two ſorts of the Helmontian Laudanum; the one uſed by the elder *Helmont*, the other by his ſon. The former was as a great ſecret communicated to me by an expert chemiſt, ſent by a German prince to compliment *Johannes Baptiſta Van Helmont*, ſome of whoſe manuſcripts (one of which periſhed in the fire of *London*,) he procured, together with a way of making his Laudanum, which, having received from him fourteen or fifteen years ago, I carefully prepared, and thought my labour ſo well recom, penſed by the extraordinary operations it hap- not ſo much in my hands, as thoſe of learned phyſicians and others, to whom I preſented portions of it, that I ſhould have thought the chemiſt a benefactor to phyſic, if he would have made publick, or permitted me to publiſh the way of making ſo ſucceſsful a medicine. And though the acceſs to my laboratory was ſo free to ingenious men, who knew ſuch a medicine to be preparing there, that ſome of them might eaſily ſuppoſe themſelves maſters of the ſecret; yet my juſtice to the communicator, who made a great and deſerved benefit of the laudanum, made me take that care to conceal ſome circumſtances, that men may eaſily be much more confident than ſure, that they have the right way of making the medicine. Which becauſe I durſt not communicate,

nicate, meeting two years ago with that obliging and very ingenious perſon, *F. M.* baron *Van Helmont*, ſon to the famous *Johannes Baptiſta*, I obtained from him, by word of mouth, ſome directions about the laudanum he uſes, which though he confeſſed, and I ſoon perceived to be differing from his father's, yet he ſeemed to think it not inferior and more parable. But he having, for a certain reaſon, imparted to me his proceſs only by word of mouth; leſt it ſhould ſlip out of my memory, I ſoon after committed it to writing, as the particulars I gathered from his writing occurred to me; and at the next ſeaſon cauſed the medicine to be prepared in my laboratory, where the progreſs was often watched in my abſence by a very learned and induſtrious London doctor, who having, at my requeſt, made many trials with it, and in ſome caſes, where other laudanums had been found unavailable, both uſes it, and commends it, more than I could expect from ſo wary and judicious a man. This medicine being ſomewhat more cheap and eaſy to be made than the elder *Helmont*'s, the experience of its efficacy made me deſire of the younger a permiſſion to communicate it for the publick good, and to prevent thoſe ſpurious receipts, that go about of the Helmontian laudanum: which requeſt of mine being almoſt as ſoon granted as made, I think myſelf bound both to his own readineſs, to oblige the publick, and to acquaint them with his way of making ſo conſiderable a medicine, as I practiſed it; though if I had received his directions in writing, they might have been more full and methodical. But though I perceived, that he ſometimes a little varies his preparations; yet that laudanum proving very ſucceſsful, that was made according to the annexed paper, I think it will not be amiſs to keep to that: which I wiſh could have been publiſhed, before the ſeaſon of the quinces were ſo far advanced. And I ſhall the more hope it may come abroad before it be quite too late, if you pleaſe to afford it room in the papers, wherewith I am informed you intend this week to gratify the curious:

Laudanum Helmontii Junioris.

TAKE of opium a quarter of a pound, and of the juice of quinces four pound at leaſt [**]; the opium being cut into very thin ſlices, and then as it were minced, to reduce it into ſmaller parts, is to be put into, and well mixed with, the liquor, (firſt made lukewarm) and fermented with a moderate heat for eight or ten days, rather more than leſs; then filter[*] it, and having infuſed in it of cinnamon, nutmeg and cloves, of each an ounce[‖], let them ſtand three or four days more; if it be a full week, it may be ſo much the better; then filter [§] the liquor once more, having let it boil a whalm or two after the ſpices have been put in: this being done evaporate away the ſuperfluous water to the conſiſtence of an extract, or to what conſiſtence you pleaſe.

LASTLY, incorporate very well with it two, or at moſt three ounces of the beſt ſaffron reduced to fine [†] powder.

ACCORDING to the conſiſtence you deſire to have your medicine of, you may order it ſo, as either to make it up into a maſs of pills (in which form I have cauſed it to be given,) or keep it in a liquid form; but in this later caſe the evaporation muſt have been made more ſparingly, that after the putting in of the [‡] ſaffron it may not grow too thick. In this form the doſe may be from five or ſix drops to ten or fewer, according to circumſtances; and of the pills a ſomewhat leſs quantity is required.

[**] (For near five pound would perhaps do better.)

[*] (Which circumſtance the author often omits, though I do not.)

[‖] (The author ſometimes uſes half an ounce more of each ſpice.)

[§] Or ſtrain it well through a canvaſs-bag.

[†] (Sometimes the author inſtead of the powder makes uſe of as much extract as can be obtained from that quantity of ſaffron.)

[‡] (Or its extract.)

SOME CONSIDERATIONS ABOUT THE RECONCILEABLENESS OF REASON and RELIGION.

By *T. E.* a LAY-MAN.

To which is annexed by the PUBLISHER,

A DISCOURSE of Mr. *BOYLE*, ABOUT THE POSSIBILITY of the RESURRECTION.

The PUBLISHER to the READER.

THESE conſiderations about religion and reaſon, delivered by a perſon of an excellent genius and ability to conſider the nature of the things he is wont to diſcourſe upon, being fallen into my hands, nor being forbidden to publiſh them; I thought the ſubject ſo weighty, and the way of handling it both ſo diſcrete and ſolid, that I could not forbear recommending it to the preſs, being fully perſuaded, the publick in general, as well as all perſons in particular, that are concerned for the ſafety both of reaſon and religion, and conſequently for their dignity as they are men, and their nobleneſs as they are Chriſtians, will find ſufficient cauſe to be pleaſed with the publication of it. To which I have nothing to add but that, whereas at the beginning of the following diſcourſe there is mention made of its being to conſiſt of two parts; one, to ſhew, that a Chriſtian need not lay aſide his reaſon; and the other, that he is not commanded to do ſo: the author thought fit to keep that paper, which concerned the latter, from now accompanying the former, which ſeems the moſt ſeaſonable, and likelieſt to make impreſſions on that ſort of perſons, whom he chiefly deſigns to perſuade.

The PREFACE.

IT is the juſt grief, and frequent complaint of thoſe, that take to heart the concerns of religion, that they ſee it now more furiouſly aſſaulted and ſtudiouſly undermined than ever, not only by the vicious lives of men, but by their licentious diſcourſes. I know, there have been vices in the world, as long as there have been men; and it is an obſervation as old as *Solomon*, *Eccleſ.* vii. 10. That men are apt to look upon their own times as worſe than thoſe, that preceded them. And becauſe I remember too, that in reciting this complaint he diſapproves it; I ſhall not diſpute, whether other ages have been leſs faulty than this we live in: but this I think I may ſay with as much truth as grief, that among us here in *England*, the times, to which our memory can reach, have been leſs guilty, than the preſent time is, of a ſpreading and bold profaneneſs. For though many allowed themſelves to court gold, and cups, and miſtreſſes, little leſs than now they do; yet theſe were ſtill acknowledged to be faults even by thoſe, that committed them, and the precepts and the counſels of religion were neglected or diſobeyed, but not their authority thrown off or affronted; men retaining yet ſuch a kind of reſpect for her, as the elder ſon in the parable did for his father, when, receiving a command from him to go and work in his vineyard, he anſwered, “ I go, Sir, though he went not, *Matth.* xxi. 30. But now too many of the vicious do not only ſcandalouſly violate the laws of religion, but queſtion the truth, and deſpiſe the very name of it. They rather chuſe to imitate the rebels in the other parable, and ſay of religion what they did of their lawful king, when they inſolently declared, “ That they would not “ have him to reign over them, *Luke* xix. 14. They ſeek not to hide their ſins, like *Adam*, but think either to cover or protect all others, by that greateſt of all impiety; and inſtead of cheating conſcience into ſilence, (as ſinners, not impudent, are wont to do,) by deceitful promiſes of repenting hereafter of their ſins, they endeavour to ſtifle or depoſe it, by maintaining, that repentance is a weakneſs of mind, and conſcience ought not to be looked on as the vicegerent of a deity, whoſe very exiſtence or providence they diſpute.

And that, which more troubled me, and made me moſt apprehend the ſpreading of this impiety, was, that it was propagated in a new way, that made me fear, the arguments not only of vulgar preachers, but even of learned divines themſelves, would be much leſs fit than formerly to give a check to its progreſs. For, till of late, the generality of our infidels did, either as philologers, queſtion the hiſtorical part of the ſcriptures, and perhaps cavil at ſome of the doctrines; or, if they employed philoſophical arguments, as *Pomponatius* and *Vaninus* did, they borrowed them from *Ariſtotle*, or the Peripatetick ſchool. And againſt both theſe ſorts of adverſaries, the learneder champions of the Chriſtian religion, ſuch as *Vives*, *Mornay*, and *Grotius*, had furniſhed divines with good and proper weapons. For, the hiſtorical part of the ſcriptures, and eſpecially the miracles, were ſtrongly confirmable by competent teſtimonies, and other moral proofs, ſufficient in their kind. And *Ariſtotle* being himſelf a dark and dubious writer, and his followers being on that account divided into ſects and parties, which for the moſt part had nothing to alledge but his ſingle authority; it was not difficult to anſwer the arguments drawn from the Peripatetick philoſophy; and, if that could not have been done, it had not been difficult to reject the doctrines themſelves as falſe or precarious. But our new libertines take another and ſhorter way, (though I hope it will not be a more proſperous one,) to undermine religion. For, not troubling themſelves to examine the hiſtorical or doctrinal parts of Chriſtian theology, in ſuch a way as Jews, Pagans, Mahometans, would do; theſe deny thoſe very principles of natural theology, wherein the Chriſtian, and thoſe other differing religions agree, and which are ſuppoſed in almoſt all religions, that pretend to revelation, namely, the exiſtence and providence of a Deity, and a future ſtate (after this life is ended.) For, theſe libertines own themſelves to be ſo upon the account of the Epicurean, or other mechanical principles of philoſophy; and, therefore, to preſs them with the authorities wont to be employed by preachers, is improper, ſince they are ſo far from paying any reſpect to the venerable fathers of the church, that they ſlight the generality of the heathen philoſophers themſelves, judging no writers worthy of name, but thoſe, that, like *Leucippus*, *Democritus*, *Epicurus*, &c. explicate things by matter and local motion; and therefore it is not to be expected, that they ſhould reverence any more the Peripatetick arguments of *Scotus* or *Aquinas*, than the homilies of St. *Auguſtine*, or St. *Chryſoſtom*; and to give *Ariſtotle* himſelf the title of the philoſopher, were enough to make ſome of them conclude the aſcriber were no philoſopher. And this, by the way, may excuſe me for not having brought into the following papers the ſentences of the fathers or the moraliſts, or the authority of *Ariſtotle*, or any of the ſchool philoſophers, which I ſhould have declined to employ, though my frequent removes from place to place, when I was writing theſe papers, had not denied me the convenience of a library.

Things being at this paſs, though the title of this diſcourſe acknowledges the author of it to be a layman; yet I ſhall not beg pardon for the enſuing papers as for an intrenchment upon the eccleſiaſticks. For beſides that, though I know ſome functions, yet I know no truths of religion, that have the peculiarity of the ſhew-bread under the law, *Matth.* xii. 4. with which it was lawful only for the prieſts to meddle;

dle; I will not ſo far miſtruſt the charity of churchmen, as not to ſuppoſe, that they will rather thank than blame any man, that being not altogether a ſtranger to this warfare, offers them his aſſiſtance againſt the common enemy in ſo important a quarrel, and ſo great a danger. The fathers, and other divines, being wont to compare the church militant to a ſhip, it will not be an improper extenſion of the compariſon, to ſay, that, when the veſſel is threatened with ſhipwreck, or boarded by pirates, it may be the duty, not only of profeſſed ſeamen, but any private paſſenger, to lend his helping hand in that common danger. And I wiſh I were as ſure, that my endeavours will prove ſuceeſsful, as I am, that ſuch churchmen, as I moſt eſteem, will think them neither needleſs nor unſeaſonable. Nay, perhaps my being a ſecular perſon may the better qualify me to work on thoſe I am to deal with, and may make my arguments, though not more ſolid in themſelves, yet more prevalent with men, that uſually (though how juſtly, let them conſider) have a particular pique at the clergy, and look with prejudice upon whatever is taught by men, whoſe intereſt is advantaged by having what they teach believed. And I was the more invited not to be a mere ſpectator, or a lazy deplorer of the danger I ſaw religion in, becauſe it ſeemed not unlikely, that philoſophical infidels, as they would be thought, would be leſs tractable to divines, though never ſo good humaniſts and antiquaries, than to a perſon, that reaſons with them upon their own grounds, and diſcourſes with them in their own way, having had a ſomewhat more than ordinary curioſity to acquaint himſelf with the Epicurean or Carteſian principles, and exerciſe himſelf in that philoſophy, which is very converſant with things corporeal, and ſtrives to explain them by matter and motion, and ſhakes off all authority (at leaſt that is not infallible.) Upon ſuch conſiderations as theſe, I complied with an occaſion I had of ſolemnly aſking reaſon the queſtion, that *Joſhua* once aſked the angel, that appeared to him in the plains of *Jericho*, "Art thou for "us, or for our adverſaries?" *Joſh.* v. 14; and of committing to paper thoſe thoughts, that ſhould occur to me on that ſubject. And this I the rather did, that I might thereby, as well contribute to my own ſatiſfaction, as to that of my friends. For, as I think, that there is nothing, that belongs to this life, that ſo much deſerves our ſerious care, as what will become of us when we are paſt it; ſo I think, that he, who takes a reſolution, either to embrace or reject ſo important a thing, as religion, without ſeriouſly examining, why he does it, may happen to make a good choice, but can be but a bad chooſer. And, that I might not exclude, by too early a method, thoſe things, that, for aught I knew, might hereafter be pertinent and uſeful, I threw my reflections into one book, as into a repoſitory, to be kept there only as a heap of differing materials, that, if they appeared worth it, they might be afterwards reviewed, and ſorted, and drawn into an orderly diſcourſe. But, before I began to do what I intended, a ſucceſſion of accidents (wherewith it would not be proper to trouble the reader,) quite diverted me to employments of a very diſtant nature; ſo that theſe papers, being thrown by, did, for divers years, lie neglected, with many others, till at length the perſon, for whoſe peruſal I, in the firſt place, deſigned them, joined with ſome other intelligent friends, to urge me to ſend them abroad, though I was not in a condition to give them the finiſhing ſtrokes, or ſo much as to fill up ſeveral of the blanks, my haſte had made me leave to be ſupplied when I ſhould be at leiſure. And indeed, notwithſtanding the juſt averſeneſs I had from letting a piece ſo incomplete and uncorrected appear in this critical age; yet the hopes, they confidently gave me, that this piece, ſuch as it is, might not be unacceptable nor uſeleſs, were not, I confeſs, altogether groundleſs.

NOVELTY being a thing very acceptable in this age, and particularly to the perſons I am to deal with, to whom perhaps it is none of the leaſt endearments of their errors, I deſpair not, that it will ſomewhat recommend theſe papers, to which I deſigned to commit not tranſcripts of what I thought they may have already met with in authors, but ſuch conſiderations, as a ſerious attention, and the nature of the things I treated of, ſuggeſted to me; ſo that moſt of the things will perhaps be thought new; and ſome few things coincident with what they may have elſewhere met with, may poſſibly appear rather to have been ſuggeſted by conſidering the ſame ſubjects, to other authors and to me, than to have been borrowed by me of them. But ſome few things, I confeſs, I employ, that were commonly enough employed before, and, I hope, I may, in that, have done religion no diſſervice; for having taken notice, that ſome of the more familiar arguments had a real force in them, but had been ſo unwarily propoſed, as to be liable to exceptions, that had diſcredited them; I made it my care, by propoſing them more cautiouſly, to prevent ſuch objections, which alone kept their force from being apparent.

I was not unmindful of the great diſadvantage this tract was likely to undergo, partly for want of a more curious method, and partly becauſe my other occaſions required, that if I publiſhed it at all, it muſt be left to come abroad unpoliſhed and unfiniſhed. But though this inconvenience had like to have ſuppreſſed this diſcourſe; yet the force of it was much weakened by this conſideration, that this immethodical way of writing would beſt comply with what was deſigned and pretended in this paper, which was, not to write a compleat treatiſe of the ſubject of it, but only to ſuggeſt about it ſome of thoſe many conſiderations, that (queſtionleſs) might have occurred to (what I do not pretend to) an enlightened and penetrating intellect. And the loadſtone, divers of whoſe phænomena are mentioned in the body of this little tract, ſuggeſted ſomewhat to me in reference to the publication of it, by exciting in me a hope, that, if this diſcourſe have any thing near as much truth, as I endeavoured to furniſh it with, that truth will have its operation upon ſincere lovers of it, notwithſtanding the want of regularity in the method; as a good loadſtone will not, by being rough and rudely ſhaped, be hindered from exerciſing its attractive and directive powers upon ſteel and iron.

As for the ſtile, I was rather ſhy than ambitious of bringing in the thorns of the ſchool-men, or the flowers of rhetorick; for, the latter, though they had, of their own accord, ſprung up under my pen, I ſhould have thought improper to be employed in ſo ſerious and philoſophical a ſubject: and as to the former, I declined them, in complaiſance to the humour of my infidels, who are generally ſo prejudiced againſt the ſchool-men, that ſcarce any thing can be preſented them with more diſadvantage than in a ſcholaſtick dreſs; and a demonſtration will ſcarce paſs for a good argument with ſome of them, if it be formed into a ſyllogiſm in mode and figure. That therefore, which I chiefly aimed at in my expreſſions, was ſignificancy and clearneſs, that my reader might ſee, that I was willing to make him judge of the ſtrength of my arguments, and would not put him to the trouble of divining in what it lay, nor inveigle him by ornaments of ſpeech, to think it greater than it was. I was alſo led by my reaſon, as well as by my inclination, to be careful not to rail at my infidels: and though I have ſome cauſe to think, that many of them had their underſtandings debauched by their lives, and were ſuduced from the church, not by *Diagoras* or *Pyrrho*, but by *Bacchus* and *Venus*; yet I treat them, as ſuppoſing them to be what they would be thought, friends to philoſophy: and being but a layman, I did not think myſelf obliged to talk to them, as out of a pulpit, and threaten them with damnation, unleſs they believed me, but choſe to diſcourſe to them rather as to erring virtuoſi, than wicked wretches.

THIS moderation, that I have uſed towards them, will, I hope, induce them to grant me two or three reaſonable requeſts; whereof the firſt ſhall be, that they would not make a final judgment of theſe papers, till they have peruſed them quite through; eſpecially having in their eye what is declared in the preamble, where both the deſign and ſcope of the whole diſcourſe, and what it does not pretend to, is expreſſed. The next thing I am to requeſt of them, and my readers, is, that they would not have the meaner thoughts of my arguments, for not being propoſed with the confidence, wherewith many writers are wont to recommend weaker proofs. For I wrote to intelligent men, and, in the judgment of ſuch, I never obſerved, that a demonſtration ceaſed to be thought one, for being modeſtly propoſed; but I have often known a good argument loſe of its credit by the invidious title of a demonſtration. And I muſt further beg my readers, to eſtimate my deſign in theſe papers, by the title of them, in which I do not pretend to make religion trample upon reaſon, but only to ſhew the reconcileableneſs of the one to the other, and the friendly agreement between them. I am a perſon, who looking upon it as my honour and happineſs to be both a man, and a Chriſtian, would neither write nor believe any thing, that might miſbecome me in either of thoſe two capacities. I am not a Chriſtian, becauſe it is the religion of my country, and my friends; nor, becauſe I am a ſtranger to the principles, either of the atomical, or the mechanical philoſophy. I admit no man's opinions in the whole lump, and have not ſcrupled, on occaſion, to own diſſents from the generality of learned men, whether philoſophers or divines: and when I chuſe to travel in the beaten road, it is not, becauſe I find it is the road, but becauſe I judge it is the way. Poſſibly I ſhould have much fewer adverſaries, if all thoſe, that yet are ſo, had as attentively and impartially conſidered the points in controverſy, as I have endeavoured to do. They would then, it is like, have ſeen, that the queſtion I handle, is not, whether rational beings ought to avoid unreaſonable aſſents, but whether, when the hiſtorical and other moral proofs clearly ſway the ſcales in favour of Chriſtianity, we ought to fly from the difficulties, that attend the granting of a Deity and Providence, to hypotheſes, whether Epicurean, or others, that are themſelves incumbered with confounding difficulties: on which account I conceive, that the queſtion between them and me is not, whether they, or I, ought to ſubmit to reaſon (for we both agree in thinking our ſelves bound to that;) but whether they or I ſubmit to reaſon the fullieſt informed, and leaſt biaſſed by ſenſuality, vanity, or ſecular intereſt.

I reverence and cheriſh reaſon as much, I hope, as any of them; but I would have reaſon practice ingenuity as well as curioſity, and both induſtriouſly pry into things within her ſphere, and frankly acknowledge, (what no philoſopher, that conſiders, will deny,) that there are ſome things beyond it. And in theſe it is, that I think it as well her duty to admit revelation, as her happineſs to have it propoſed to her; and, even as to revelations themſelves, I allow reaſon to judge of them, before ſhe judges by them. The following papers will, I hope, manifeſt, that the main difference betwixt my adverſaries and me is, that they judge upon particular difficulties and objections, and I upon the whole matter. And to conclude; as I make uſe of my watch to eſtimate time, when ever the ſun is abſent or clouded, but when he ſhines clearly forth, I ſcruple not to correct and adjuſt my watch by his beams caſt on a dial; ſo, wherever no better light is to be had, I eſtimate truth by my own reaſon; but where divine revelation can be conſulted, I willingly ſubmit my fallible reaſon to the ſure informations afforded by celeſtial light.

I ſhould here put an end to this long preface, but that, to the things, which have been ſaid concerning what I have written of my own, I ſee it is requiſite, that I add a few words about what I quote from other writers; eſpecially becauſe in this very preface I mention my having intended to entertain my friend with my own thoughts. Of the citations therefore, that my reader will meet with in the following papers, I have this account to give him:

1. That I had written the conſiderations and diſtinctions, to which they are annexed, before I met with theſe cited paſſages, which I afterwards inſerted in the margent, and other vacant places of my epiſtle. 2. That theſe paſſages are not borrowed from books, that treat of the truth of the Chriſtian religion, or of Chriſtian theology at all, but are taken from authors, that write of philoſophical ſubjects, and are by me applied to mine, which are uſually very diſtant from theirs. 3. If you then aſk me, why I make uſe of their authority, and did not content my ſelf with my own ratiocinations? I have this to anſwer; that my deſign being to convince another, who had no reaſon to look upon my authority, and whom I had cauſe to ſuſpect to have entertained ſome prejudices againſt any reaſons, that ſhould come from one, that confeſſedly aimed at the defending of the Chriſtian religion, I thought it very proper and expedient to let him ſee, that divers of the ſame things (for ſubſtance) that I delivered in favour of that religion, had been taught as philoſophical truths by men, that were not profeſſed divines, and were philoſophers, and ſuch ſtrict naturaliſts, too, as to be extraordinarily careful, not to take any thing into their philoſophy upon the account of revelation. And on this occaſion let me obſerve to you, that there are ſome arguments, which being clearly built upon ſenſe, or evident experiments, need borrow no aſſiſtance from the refutation of any of the propoſers or approvers, and may, I think, be fitly enough compared to arrows ſhot out of a croſs-bow, and bullets ſhot out of a gun, which have the ſame ſtrength, and pierce equally, whether they be diſcharged by a child, or a ſtrong man. But then, there are other ratiocinations, which either do, or are ſuppoſed to depend, in ſome meaſure, upon the judgment and ſkill of thoſe, that make the obſervations, whereon they are grounded, and their ability to diſcern truth from counterfeits, and ſolid things from thoſe, that are but ſuperficial ones: and theſe may be compared to arrows ſhot out of a long-bow, which make much the greater impreſſion, by being ſhot by a ſtrong and ſkilful archer. And therefore when we queſtion, what doctrines ought, or ought not to be thought reaſonable, it does not a little facilitate a propoſition's appearing (not contrary, but) conſonant to reaſon, that it is looked upon as ſuch by thoſe, that are acknowledged the maſters of that faculty.

SOME
CONSIDERATIONS
ABOUT THE
RECONCILEABLENESS
OF
REASON and RELIGION.

PART I.

AS to what you write in your friend's name, near the bottom of the firſt page of your letter, perhaps I ſhall not miſtake, if I gueſs, that, when he ſeems but to propoſe a queſtion, he means an objection; and covertly intimates, that I, among many others, am reduced to that paſs, that to embrace our religion, we muſt renounce our reaſon; and conſequently, that to be a Chriſtian, one muſt ceaſe to be a man, and much more, leave off being a philoſopher.

WHAT liberal conceſſions ſoever ſome others have been pleaſed to make on ſuch an occaſion as this, they do not concern me; who, being aſked but my own opinion, do not think my ſelf reſponſible for that of others. And therefore, that I may frame my anſwer ſo, as to meet both with the obvious ſenſe of the queſtion, and the intimated meaning of him, that propoſes it, I ſhall roundly make a negative reply, and ſay, "that I do not think, "that a Chriſtian, to be truly ſo, is obliged to "forego his reaſon; either by denying the "dictates of right reaſon, or by laying aſide "the uſe of it."

I doubt not, but this anſwer is differing enough from what your friend expects; and perhaps thoſe grants, that have been made by the indulgence, or inadvertency of many perſons, eminent for being pious or learned, may make you yourſelf ſtartle at this declaration: and therefore, though you will not, I know, expect an anſwer to what objections your friend may make, ſince he has expreſſed but what he thinks ought to be a Chriſtian's opinion, not what he has to object againſt what is ſo; yet, to ſatisfy thoſe ſcruples, that you your ſelf may retain, I ſhall endeavour (but with the brevity, that becomes a letter) to acquaint you by themſelves, with ſome of the poſitive inducements, that have led me to this opinion, and interweave ſome others, in anſwering the chief objections, that I think likely to be made againſt it.

AND this preamble, ſhort as it is, will, I hope, ſerve to keep you from miſtaking my deſign; which, as you may gather from what I have intimated, is not to give you the poſitive proofs of the Chriſtian religion (which is not here to be expected from a bare defendant,) but to give you ſome ſpecimens of ſuch general conſiderations, as may probably ſhew, that the matter (or eſſential doctrines) peculiar to the Chriſtian religion is not ſo repugnant to the principles of true natural philoſophy, as that to believe them, a man muſt ceaſe to act like a rational man, any more than he would be obliged to do by embracing other religions, or even the tenets, that have been held without diſparagement to their intellectuals, by the mere philoſophers themſelves; which laſt clauſe I add, becauſe, I preſume, you do not expect, that I ſhould be ſo ſolicitous to vindicate the Chriſtian's belief of a Deity from being irrational; ſince, beſides that, perhaps your friend would think himſelf affronted to be dealt with as an Atheiſt, without having profeſſed himſelf one, the acknowledgement of a Deity blemiſhes the Chriſtian's reaſon no more, than it does that of men of all religions, not to ſay of all mankind; and imports no other contradiction to reaſon, than what has been judged to be none at all by the greateſt, if not by all, of the philoſophers, that were famed for being guided by reaſon (without revelation.) And I ſhall venture to add (upon the by) that, as I do not, for my own part, think the Atheiſt's philoſophical objections (if your friend had produced them) to be near ſo conſiderable for weight or number, as not only thoſe few, that deny a God, but many of thoſe, that believe one, are wont to think; ſo the Chriſtian is not reduced, as is imagined, to make the Being of a Deity a mere poſtulatum; ſince, beſides the philoſophical arguments he can alledge in common with the beſt champions for a Deity, he has a peculiar hiſtorical proof, that may ſuffice; the miracles performed by Chriſt and his followers being ſuch, that if the matter of fact can be (as it may be) well evinced, they will not only prove the reſt of the Chriſtian religion, but in the firſt place, that there muſt be a God to be the Author of them.

BUT though of the two things, which my deſign obliges me to endeavour the making good of, the moſt natural order ſeems to be, that I ſhould firſt ſhew, that no precepts of Chriſtianity do command a man to lay aſide his reaſon in matters of religion; and then, that there is nothing in the nature of the Chriſtian doctrine itſelf, that makes a man need to do ſo; yet I think it not amiſs in treating of theſe two ſubjects to invert the order, and firſt conſider that difficulty, which is the principal, and which your friend and you jointly deſire to have my thoughts of; namely, "Whether there be a neceſſity for a Chriſtian "to deny his reaſon?" And then we ſhall proceed to examine, whether, though he need not diſclaim his reaſon, it be nevertheleſs his duty ſo to do?

SECTION I.

TO proceed then to the conſiderations, that make up the former part of this epiſtle; I ſhall, in the firſt place, diſtinguiſh betwixt that, which the Chriſtian religion itſelf teaches, and that, which is taught by this or that church, or ſect of Chriſtians, and much more by this or that particular divine or ſchoolman.

I need not perſuade you, who cannot but know it ſo well already, that there are many things taught about the attributes and decrees of God, the myſteries of the trinity, and incarnation, and divers other theological ſubjects, about which not only private Chriſtians, but churches of Chriſtians do not at all agree. There are too many men, whoſe ambition, or boldneſs, or ſelf-conceit, or intereſt, leads them to obtrude upon others, as parts of religion, things, that are not only ſtrangers, but oftentimes enemies to it. And there are others, who, out of an indiſcrete devotion, are ſo ſolicitous to encreaſe the number, and the wonderfulneſs of myſteries, that, to hear them propoſe and diſcourſe of things, one would judge, that they think it is the office of faith, not to elevate, but to trample upon reaſon; and that things are then fitteſt to be believed, when they are not clearly to be proved or underſtood. And indeed, when, on the one ſide, I conſider the charitable deſign of the goſpel, and the candid ſimplicity, that ſhines in what it propoſes, or commands; and on the other ſide, what ſtrange and wild ſpeculations and inferences have been fathered upon it, not only in the metaphyſical writings of ſome ſchoolmen, but in the articles of faith of ſome churches; I cannot but think, that if all theſe doctrines are parts of the Chriſtian religion, the apoſtles, if they were now alive, would be at beſt but *Catechumeni*; and I doubt not, but many of the nice points, that are now much valued and urged by ſome, would be as well diſapproved by St. Paul, as by *Ariſtotle*; and ſhould be as little entertained by an orthodox divine, as a rigid philoſopher. I do not therefore allow all that for goſpel, which is taught for ſuch in a preacher's pulpit, or even a profeſſor's chair. And therefore, if ſcholaſtick writers, of what church ſoever, take the liberty of impoſing upon the Chriſtian religion their metaphyſical ſpeculations, or any other merely human doctrines, as matters of faith, I who, not without ſome examination, think metaphyſicks themſelves not to have been for the moſt part over-well underſtood, and applied, ſhall make bold to leave all ſuch private doctrines to be defended by their own broachers or abettors; and ſhall deny, that it will follow, that in caſe of this multitude of placets, which ſome bold men have been pleaſed to adopt into the catalogue of Chriſtian verities, any, or all, ſhould be found inconſiſtent with right reaſon, the Chriſtian religion muſt be ſo too. For by that name I underſtand only that ſyſtem of revealed truths, that are clearly delivered in the ſcriptures; or by legitimate and manifeſt conſequences deduced thence. And by this one declaration, ſo many unneceſſary, and perhaps hurtful retainers to Chriſtianity will be at once thrown off, that I doubt not, but if you conſider the matter aright, you will eaſily diſcern, that by this firſt diſtinction I have much leſſened the work, that is to be done by thoſe, that are to follow it.

SECTION II.

IN the next place, among the things, that ſeem not rational in religion, I make a great difference between thoſe, in which unenlightened reaſon is manifeſtly a competent judge, and thoſe, which natural reaſon itſelf may diſcern to be out of its ſphere.

YOU will allow me, that natural theology is ſufficient to evince the exiſtence of the deity; and we know, that many of the old philoſophers, that were unaſſiſted by revelation, were, by the force of reaſon, led to diſcover and confeſs a God, that is, a being ſupremely perfect; under which notion, divers of them expreſsly repreſent him. Now, if there be ſuch a being, it is but reaſonable to conceive, that there may be many things relating to his nature, his will, and his management of things, that are without the ſphere of mere or unaſſiſted reaſon. For, if his attributes and perfections be not fully comprehenſible to our reaſon, we can have but inadequate conceptions of them; and ſince God is a Being, *toto cælo*, as they ſpeak, differing from all other beings, there may be ſome things in his nature, and in the manner of his exiſtence, which is without all example, or perfect analogy, in inferior beings. For we ſee, that even in man himſelf, the co-exiſtence and intimate union of the ſoul and body, that is, an immaterial and a corporeal ſubſtance, is without all preſident or parallel in nature. And though the truth of this union may be proved; yet, the manner of it was never yet, nor perhaps ever will be, in this life clearly underſtood, (to which purpoſe I ſhall elſewhere ſay more.) Moreover, if we ſuppoſe God to be omnipotent, (that is, to be able to do whatever involves no contradiction, that it ſhould be done,) we muſt allow him to be able to do many things, that no other agent can afford us any examples of, and ſome of them perhaps, ſuch as we, who are but finite, and are wont to judge of things by analogy, cannot conceive how they can be performed. Of the laſt ſort of things may be the recollecting a ſufficient quantity of the ſcattered matter of a dead human body, and the contriving of it ſo, that (whether alone, or with ſome addition of other particles) upon a re-conjunction with the ſoul, it may again conſtitute a living man, and ſo effect that wonder we call the reſurrection. Of the latter ſort, is the creation of matter out of nothing, and much more the like production of thoſe rational and intelligent beings, human ſouls. For as for angels (good or bad) I doubt, whether mere philoſophy can evince their exiſtence, though I think it may the poſſibility thereof. And ſince we allow the Deity a wiſdom equal to this boundleſs power, it is but reaſonable to conceive, that theſe unlimited attributes conſpiring, may produce contrivances and frame deſigns, which we men muſt be unable (at leaſt of ourſelves) ſufficiently to underſtand, and to reach to the bottom of. And by this way of arguing, it may be made to appear, that there may be many things relating to the Deity above the reach of unenlightened human reaſon. Not that I affirm all theſe things to be, in their own nature, incomprehenſible to us, (though ſome of them may be ſo,) when they are once propoſed; but that reaſon, by its own light, could not diſcover them particularly, and therefore it muſt owe its knowledge of them to divine revelation. And if God vouchſafes to diſcloſe thoſe things to us, ſince not only he muſt needs know about his own nature, attributes, &c. what we cannot poſſible know unleſs he tells us, and ſince we know, that whatever he tells us is infallibly true, we have abundant reaſon to believe rather what he declares to us concerning himſelf and divine things, than what we ſhould conclude or gueſs about them, by analogy to things of a nature infinitely diſtant from his, or by maxims framed according to the nature of inferior beings. If therefore he clearly reveal to us, that there is in the Godhead, three diſtinct perſons, and yet that God is one, we, that think ourſelves bound to believe God's teſtimony in all other caſes, ought ſure not to diſbelieve it concerning himſelf, but to acknowledge, that in an unparalleled and incomprehenſible Being, there may be a manner of exiſtence not to be paralleled in any other being, though it ſhould never be underſtood by us men, who cannot clearly comprehend, how in ourſelves two ſuch diſtant natures, as that of a groſs body, and an immaterial ſpirit, ſhould be united, ſo as to make up one man. In ſuch caſes therefore, as we are now ſpeaking of, there muſt indeed be ſomething, that looks like captivating one's reaſon, but it is a ſubmiſſion, that reaſon itſelf obliges us to make; and he, that in ſuch points as theſe, believes rather what the divine writings teach him, than what he would think, if they had never informed him, does not renounce or enſlave his reaſon, but ſuffers it to be pupil to an omniſcient and infallible inſtructor, who can teach him ſuch things, as neither his own mere reaſon, nor any others could ever have diſcovered to him.

I thought to have here diſmiſſed this propoſition, but I muſt not omit to give it a confirmation afforded me by chance, (or rather providence:) for, ſince I writ the laſt paragraph, reſuming a philoſophical enquiry, I met, in proſecuting it, with a couple of teſtimonies of the truth of what I was lately telling you, which are given, not by divines or ſchoolmen, but by a couple of famous mathematicians, that have both led the way to many of the modern philoſophers, to ſhake off the reverence wont to be borne to the authority of great names, and have advanced reaſon in a few years, more than ſuch as *Vaninus* and *Pomponatius* would do in many ages; and have always boldly, and ſometimes ſucceſsfully, attempted to explain intelligibly thoſe things, which others ſcrupled not, either openly or tacitly, to confeſs inexplicable.

THE firſt of theſe teſtimonies I met with in a little French treatiſe put out by ſome mathematician, who, though he conceals his name, appears, by his way of writing, to be a great virtuoſo, and takes upon him to give his readers in French the new thoughts of *Galilæo*, by making that the title of his book. This writer then ſpeaking of a paradox (which I but recite) of *Galilæo*'s, that makes a point equal to a circle, adds, & *per conſequent l'on peut* Pag. 22, 23. *dire*, i.e. and conſequently one may ſay, that all circles are equal between themſelves, ſince each of them is equal to a point. For though the imagination be over-powered by this idea, or notion, yet reaſon will ſuffer itſelf to be perſuaded of it. I know (continues he) divers other excellent perſons, (beſides *Galilæo*) who conclude the ſame thing by other ways; but all are conſtrained to acknowledge, that indiviſible and infinite are things, that do ſo ſwallow up the mind of man, that he ſcarce knows what to pitch on, when he contemplates them. For it will follow, from *Galilæo*'s ſpeculation, &c. which paſſage I have cited, to ſhew you, that *Galilæo* is not the only philoſopher and mathematician, who has confeſſed his reaſon quite paſſed about the attributes of what is infinite.

THE other teſtimony I mentioned to you, is that of the excellent *Des Cartes*, in the ſecond part of his principles of philoſophy, where, ſpeaking of the circle to be made by matter moving through places ſtill leſſer and leſſer, he has this ingenious acknowledgment; *fatendum tamen eſt* (ſays he) *in motu iſto aliquid reperiri, quod mens quidem noſtra percipit eſſe verum, ſed tamen quo pacto fiat non comprehendit, nempe diviſionem quarundam particularum materiæ in infinitum, ſive indefinitam, atque in tot partes ut nulla cogitatione determinare poſſimus tam exiguam, quin intelligamus ipſam in alias adhuc minores reipſa eſſe diviſam.* And in the cloſe of the next paragraph, he gives this for a reaſon, why, though we cannot comprehend this indefinite diviſion, yet we ought not to doubt of the truth of it, that we diſcern it to be of that kind of things, that cannot be comprized by our minds, as being but finite.

IF then ſuch bold and piercing wits, and ſuch excellent mathematicians, are forced to confeſs, that not only their own reaſon, but that of mankind, may be paſſed and non-pulſſed about quantity, which is an object of contemplation natural, nay, mathematical, and which is the ſubject of the rigid demonſtrations of pure mathematicks; why ſhould we think it unfit to be believed, and to be acknowledged, that in the attributes of God, who is eſſentially an infinite Being, and an *ens ſingulariſſimum*, and in divers other divine things, of which we can have no knowledge without revelation, there ſhould be ſome things, that our finite underſtandings cannot, eſpecially in this life, clearly comprehend.

SECTION III.

TO this conſideration, I ſhall, for affinity's ſake, ſubjoin another, which I leave to your liberty to look upon as a diſtinct one, or as an enlargement and application of the former.

I conſider then, that there is a great difference between a doctrine's being repugnant to the general and well-weighed rules or dictates of reaſon, in the forming of which rules, it may be ſuppoſed to have been duly conſidered; and its diſagreeing with axioms, at the eſtabliſhment whereof the doctrine in queſtion was probably never thought on. There are ſeveral rules, that paſs current, even among the moſt learned men, and which are indeed of very great uſe, when reſtrained to thoſe things whence they took their riſe, and others of the like nature; which yet ought not to overthrow thoſe divine doctrines, that ſeem not conſonant to them. For the framers of theſe rules having generally built them upon the obſervations they had made of natural and moral things, ſince (as we lately argued) reaſon itſelf cannot but acknowledge, there are ſome things out of its ſphere, we muſt not think it impoſſible, that there may be rules, which will hold in all inferior beings for which they were made; and yet not reach to that infinite and moſt ſingular Being, called God, and to ſome divine matters, which were not taken into conſideration, when thoſe rules were framed. And indeed, if we conſider God as the author of the univerſe, and the free eſtabliſher of the laws of motion, whoſe general concourſe is neceſſary to the conſervation and efficacy of every particular phyſical agent, we cannot but acknowledge, that, by with-holding his concourſe, or changing theſe laws of motion, which depend perfectly upon his will, he may invalidate moſt, if not all the axioms and theorems of natural philoſophy: theſe ſuppoſing the courſe of nature, and eſpecially the eſtabliſhed laws of motion among the parts of the univerſal matter, as thoſe upon which all the phænomena depend. It is a rule in natural philoſophy, that *cauſæ neceſſariæ ſemper agunt quantum poſſunt*; but it will not follow from thence, that the fire muſt neceſſarily burn *Daniel*'s three companions, or their clothes, that were caſt by the Babylonian king's command into the midſt of a burning fiery furnace, when the author of nature was pleaſed to withdraw his concourſe to the operation of the flames, or ſupernaturally to defend againſt them the bodies, that were expoſed to them. That men once truly dead cannot be brought to life again, hath been in all ages the doctrine of mere philoſophers; but though this be true, according to the courſe of nature, yet it will not follow, but that the contrary may be true, if God interpoſe either to recal the departed ſoul, and re-conjoin it to the body, if the organization of this be not too much vitiated, or by ſo altering the fabrick of the matter, whereof the carcaſe conſiſts, as to reſtore it to a fitneſs for the exerciſe of the functions of life. Agreeably to this, let me obſerve to you, that, though it be unreaſonable to believe a miraculous effect, when attributed only to a mere phyſical agent; yet the ſame thing may reaſonably be believed, when aſcribed to God, or to agents aſſiſted with his abſolute or ſupernatural power. That a man born blind ſhould, in a trice, recover his ſight, upon the application of clay and ſpittle, would juſtly appear incredible, if the cure were aſcribed to one, that acted as a mere man; but it will not follow, that it ought to be incredible, that the Son of God would work it. And the like may be ſaid of all the miracles performed by Chriſt, and thoſe apoſtles and other diſciples of his, that acted by virtue of a divine power and commiſſion. For in all theſe, and the like caſes, it ſuffices not to make one's belief irrational, that the things believed are impoſſible to be true, according to the courſe of nature; but it muſt be ſhewn, either that they are impoſſible, even to the power of God, to which they are aſcribed, or that the records, we have of them, are not ſufficient to beget belief in the nature of a teſtimony; which latter objection againſt theſe relations is foreign to our preſent diſcourſe. And as the rules about the power of agents will not all of them hold in God, ſo I might ſhew the like, if I had time, concerning ſome of his other attributes: inſomuch, that even in point of juſtice, wherein we think we may freelieſt make eſtimates of what may or may not be done, there may be ſome caſes, wherein God's ſupreme dominion, as maker and governor of the world, places him above ſome of thoſe rules; I ſay, ſome, for I ſay not above all thoſe rules of juſtice, which oblige all inferior beings, without excepting the greateſt and moſt abſolute monarchs themſelves. I will not give examples of his power of pardoning or remitting penalties, which is but a relaxing of his own right; but will rather give an inſtance in his power of afflicting and exterminating men, without any provocation given him by them. I will not here enter upon the controverſy *de jure Dei in creaturas*, upon what it is founded, and how far it reaches. For, without making myſelf a party in that quarrel, I think, I may ſafely ſay, that God, by his right of dominion, might, without any violation of the laws of juſtice, have deſtroyed, and even annihilated *Adam* and *Eve*, before they had eaten of the forbidden fruit, or had been commanded to abſtain from it. For man being as much and as entirely God's workmanſhip as any of the other creatures, unleſs God had obliged himſelf by ſome promiſe or pact, to limit the exerciſe of his abſolute dominion over him, God was no more bound to preſerve *Adam* and *Eve* long alive, than he was to preſerve a lamb, or a pidgeon; and therefore, as we allow, that he might juſtly recal the lives he had given thoſe innocent creatures, when he pleaſed, (as actually he often ordered them to be killed, and burned in ſacrifice to him:) ſo he might, for the declaration of his power to the angels, or for other reaſons, have ſuddenly taken away the lives of *Adam* and *Eve*, though they had never offended him. And upon the ſame grounds he might, without injuſtice, have annihilated, I ſay not, damned their ſouls; he being no more bound to continue exiſtence to a nobler, than a leſs noble creature; as he is no more bound to keep an eagle, than an oyſter always alive. I know, there is a difference betwixt God's reſuming a being he lent *Adam*, and his doing the ſame to inferior creatures: but that diſparity, if it concern any of his attributes, will concern ſome other than his juſtice; which allowed him to reſume, at pleaſure, the being he had only lent them, or lay any affliction on them, that were leſſer than that good could countervail. But, mentioning this inſtance only occaſionally, I ſhall not proſecute it any further, but rather mind you of the reſult of this and the foregoing conſideration; which is, that divinely revealed truths may ſeem to be repugnant to the dictates of reaſon, when they do but ſeem to be ſo: nor does Chriſtianity oblige us to queſtion ſuch rules, as to the caſes they were framed for, but the application of them to the nature of God, who has already been truly ſaid to be *ens ſingulariſſimum*, and to his abſolute power and will; ſo that we do not reject the rules we ſpeak of, but rather limit them; and when we have reſtrained them to their due bounds, we may ſafely admit them.

FROM men's not taking notice of, or not pondering this neceſſary limitation of many axioms delivered in general terms, ſeems to have proceeded a great error, which has made ſo many learned men preſume to ſay, that this or that thing is true in philoſophy, but falſe in divinity, or on the contrary: as for inſtance, that a virgin, continuing ſuch, may have a child, is looked upon as an article, which theology aſſerts to be true, and philoſophy pronounces impoſſible. But the objection is grounded upon a miſtake, which might have been prevented by wording the propoſitions more warily and fully. For though we grant, that phyſically ſpeaking, it is falſe, that a virgin can bring forth a child; yet that ſignifies no more, than that, according to the courſe of nature, ſuch a thing cannot come to paſs; but ſpeaking abſolutely and indefinitely, without confining the effect to mere phyſical agents, it may ſafely be denied, that philoſophy pronounces it impoſſible, that a virgin ſhould be a mother. For why ſhould the author of nature be confined to the ways of working of dependent and finite agents? And to apply the anſwer to the divines, that hold the opinion I oppoſe, I ſhall demand, why God may not out of the ſubſtance of a woman form a man, without the help of a man, as well as at the beginning of the ſubſtance of a man he formed a woman without the concurrence of a woman? And ſo that iron, being a body far heavier (*in ſpecie*, as they ſpeak,) will, if upheld by no other body, ſink in water, is a truth in natural philoſophy; but ſince phyſicks themſelves lead men to the acknowledgment of a God, it is not repugnant to reaſon, that, if God pleaſe to interpoſe his power, he may (as in *Eliſha*'s caſe) make iron ſwim, either by with-holding his concourſe to the agents, whatever they be, that cauſe gravity in bodies, or perhaps by other ways unknown to us; ſince a vigorous loadſtone may, as I have more than once tried, keep a piece of iron, which it touches not, ſwimming in the air, though this thin body muſt contribute far leſs, than water would, to the ſuſtaining it aloft.

THAT ſtrict philoſopher *Des Cartes*, who has with great wit and no leſs applauſe attempted to carry the mechanical powers of matters higher than any of the modern philoſophers; this naturaliſt, I ſay, that aſcribes ſo great a power to matter and motion, was ſo far from thinking, that what was impoſſible to them, muſt be ſo to God too, that, though he were urged by a learned adverſary with an argument, as likely as any to give him a ſtrong temptation to limit the omnipotence of God; yet even on this occaſion he ſcruples not to make this ingenious and wary acknowledgment, and that in a private letter; "For my part, ſays he, I think we ought never to ſay of any thing, that it is impoſſible to God. For all, that is true and good, being dependent on his all-mightineſs, I dare not ſo much as ſay, that God cannot make a mountain without a valley, or cannot make it true, that one and two ſhall not make three; but I ſay only, that he has given me a ſoul of ſuch a nature, that I cannot conceive a mountain without a valley, nor that the aggregate of one and of two ſhall not make three, &c. and I ſay only, that ſuch things imply a contradiction in my conception." And conſonantly to this, in his Principles of Philoſophy he gives, on a certain occaſion, this uſeful caution,—*Quod ut ſatis tutò & ſine errandi periculo aggrediamur, eâ nobis cautelâ eſt utendum, ut ſemper quàm maxime recordemur, & Deum autorem rerum eſſe infinitum, & nos omnino finitos.*

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Parte Prima. Art. 24.

SECTION IV.

IN the next place, I think we ought to diſtinguiſh between reaſon conſidered in itſelf, and reaſon conſidered in the exerciſe of it, by this or that philoſopher, or by this or that man, or by this or that company, or ſociety of men, whether all of one ſect or of more.

IF you will allow me to borrow a ſchool-phraſe, I ſhall expreſs this more ſhortly by ſaying, I diſtinguiſh between reaſon *in abſtracto*, and *in concreto*. To clear this matter, we may conſider, that whatever you make the faculty of reaſon to be in itſelf, yet the ratiocinations it produces are made by men, either ſingly reaſoning, or concurring in the ſame ratiocinations and opinions; and conſequently, if theſe men do not make the beſt uſe of their reaſoning faculty, it will not be neceſſary, that what thwarts their ratiocinations, muſt likewiſe thwart the principles or the dictates of right reaſon. For man having a will and affections as well as an intellect, though our dijudications and tenents ought indeed (in matters ſpeculative) to be made and pitched upon by our unbiaſſed underſtandings; yet really our intellectual weakneſſes, or our prejudices, or prepoſſeſſion by cuſtom, education, &c. our intereſt, paſſions, vices, and I know not how many other things, have ſo great and ſwaying an influence on them, that there are very few concluſions, that we make, or opinions, that we eſpouſe, that are ſo much the pure reſults of our reaſon, that no perſonal diſability, prejudice, or fault, has any intereſt in them.

About the Diverſity of Religion.

THIS I have elſewhere more amply diſcourſed of on another occaſion; wherefore I ſhall now add but this, that the diſtinction, I have been propoſing, does (if I miſtake not) reach a great deal further than you may be aware of. For not only whole ſects, whether in religion or philoſophy, are in many caſes ſubject to prepoſſeſſions, envy, ambition, intereſt, and other miſleading things, as well as ſingle perſons; but, which is more conſiderable to our preſent purpoſe, the very body of mankind may be embued with prejudices, and errors, and that from their childhood, and ſome alſo even from their birth, by which means they continue undiſcerned, and conſequently unreformed.

THIS you will think an accuſation as bold as high; but to let you ſee, that the philoſophers, you moſt reſpect, have made the ſame obſervation, though not applied to the ſame caſe, I muſt put you in mind, that Monſieur *Des Cartes* begins his principles of philoſophy with taking notice, that, becauſe we are born children, we make divers unright judgments of things, which afterwards are wont to continue with us all our lives, and prove radicated prejudices, that miſlead our judgments on ſo many occaſions, that he elſewhere tells us, he found no other way to ſecure himſelf from their influence, but once in his life ſolemnly to doubt of the truth of all, that he had till then believed, in order to the re-examining of his former dijudications. But I remember, our illuſtrious *Verulam* warrants a yet further prejudice againſt many things, that are wont to be looked on as the ſuggeſtions of reaſon. For having told us, that the mind of man is beſieged with four differing kinds of idols or phantaſms, when he comes to enumerate them, he teaches, that there are not only ſuch, as men get by converſation

ſation and diſcourſe one with another, and ſuch as proceed from the divers hypotheſes or theories and opinions of philoſophers, and from the perverſe ways of demonſtration, and likewiſe ſuch as are perſonal to this or that man, proceeding from his education, temperament, ſtudies &c. but ſuch as he calls *idola tribus*, becauſe they are founded in humane nature itſelf, and in the very tribe or nation of mankind; and of theſe he particularly diſcourſes of ſeven or eight; as, that the intellect of man has an innate propenſity to ſuppoſe in things a greater order and equality than it finds, and that being unable to reſt or acquieſce, it does always tend further and further; to which he adds divers other innate prejudices of mankind, which he ſolicitouſly as well as judiciouſly endeavours to remove.

Now, if not only ſingle philoſophers, and particular ſects, but the whole body of mankind be ſubject to be ſwayed by innate and unheeded prejudices and proclivities to errors about matters, that are neither divine, nor moral, nor political, but phyſical, where the attainment of truth is exceeding pleaſant to human nature, and is not attended with conſequences diſtaſteful to it: why may not we juſtly ſuſpect not only this or that philoſopher, or particular ſect; but the generality of men, of having ſome ſecret propenſities to err about divine things, and indiſpoſitions to admit truths, which not only detect the weakneſſes of our nature, and our perſonal diſabilities, and thereby offend or mortify our pride and our ambition, but ſhine into the mind with ſo clear, as well as pure and chaſte a light, as is proper both to diſcover to ourſelves and others our vices and faults, and oftentimes to croſs our deſigns and intereſts?

AND to this purpoſe we may take notice, that divers of thoſe very idols, which my lord *Bacon* obſerves to beſiege, or pervert men's judgments in reference to things natural, may probably have the ſame kind of influence (and that much ſtronger) on the minds of men in reference to ſupernatural things. Thus he takes notice, that if ſome things have once pleaſed the underſtanding, it is apt to draw all others to comport with, and give ſuffrage to them, though perhaps the inducements to the contrary belief be either more numerous or more weighty. He obſerves alſo, that man is apt to look upon his ſenſes and other perceptions as the meaſures of things, and alſo that the underſtanding of man is not ſincerely diſpoſed to receive the light of truth, but receives an infuſion as it were of adventitious colours, (that diſguiſe the light) from the will and affections, which makes him ſooner believe thoſe things, that he is deſirous ſhould be true, and reject many others upon accounts, that do no way infer their being falſe. Now if we apply theſe things to divine truths (to which it were well they were leſs juſtly applicable) and conſider, that in our youth we generally converſe but with things corporeal, and are ſwayed by affections, that have them for their objects, we ſhall not much wonder, that men ſhould be very prone, either to frame ſuch notions of divine things, as they were wont to have about others of a far different and meaner nature; or elſe to reject them for not being analogous to thoſe things, which they have been uſed to employ for the meaſures of truth and falſity. And if we conſider the inbred pride of man, which is ſuch, that if we believe the ſacred ſtory, even *Adam* in paradiſe affected to be like God, knowing good and evil; we ſhall not ſo much marvel, that almoſt every man in particular makes the notions he has entertained already, and his ſenſes, his inclinations, and his intereſts, the ſtandards, by which he eſtimates and judges of all other things, whether natural or revealed. And as *Heraclitus* juſtly complained, that every man ſought the knowledge of natural things in the microcoſm, that is, himſelf, and not in the macrocoſm, the world; ſo we may juſtly complain, that men ſeek all the knowledge, they care to find, or will admit, either in theſe little worlds themſelves, or from that great world, the univerſe; but not from the omniſcient author of them both. And laſtly, if even in purely phyſical things, where one would not think it likely, that rational beings ſhould ſeek truth with any other deſigns than of finding and enjoying it, our underſtandings are ſo univerſally biaſſed, and impoſed upon by our wills and affections; how can we admire, eſpecially if we admit the fall of our firſt parents, that our paſſions and intereſts, and oftentimes our vices, ſhould pervert our intellects about thoſe revealed truths; divers of which we diſcern to be above our comprehenſions, and more of which we find to be directly contrary to our inclinations?

SECT. V.

AND now it will be ſeaſonable for me to tell you, that I think, there may be a great difference betwixt a thing's being contrary to right reaſon, or ſo much as to any true philoſophy, and its being contrary to the received opinions of philoſophers, or to the principles or concluſions of this or that ſect of them.

FOR here I may juſtly apply to my preſent purpoſe what *Clemens Alexandrinus* judiciouſly ſaid on another occaſion, that philoſophy was neither Peripatetical, nor Stoical, nor Epicurean, but whatſoever among all thoſe ſeveral parties was fit to be approved.

AND indeed, if we ſurvey the hypotheſes and opinions of the ſeveral ſects of philoſophers, eſpecially in thoſe points, wherein they hold things repugnant to theological truths, we ſhall find many of them ſo ſlightly-grounded, and ſo diſagreeing among themſelves, that a ſevere and inquiſitive examiner would ſee little cauſe to admit them upon the bare account of his being a philoſopher, though he did not ſee any to reject them upon the account of his being a Chriſtian. And in particular, as to the Peripateticks, who by invading all the ſchools of *Europe* (and ſome in *Aſia* and *Africk*) have made their ſect almoſt Catholick, and have produced divers of the famous queſtioners of Chriſtianity in the laſt age, and the firſt of this; the world begins to be apace undeceived, as to many of their doctrines, which were as confidently taught and believed for many ages, as thoſe, that are repugnant to our religion; and there is now ſcarce any of the modern philoſophers, that allow themſelves the free uſe of their reaſon, who believes any longer, that there is an element of fire lodged under the ſuppoſed ſphere of the moon; that heaven conſiſts of ſolid orbs; that all celeſtial bodies are ingenerable and incorruptible; that the heart, rather than the brain, is the origine of nerves; that the torrid zone is uninhabitable; and I know not how many other doctrines of the Ariſtotelians, which our Corpuſcularian philoſophers think ſo little worth being believed, that they would cenſure him, that ſhould now think them worthy to be ſolicitouſly confuted; upon which ſcore I preſume you will allow me to leave thoſe, and divers others, as weak Peripatetick conceits, to fall by their own groundleſsneſs.

BUT you will tell me, that the Epicureans, and the Somatici, that will allow nothing but body in the world, nor no author of it but chance, are more formidable enemies to religion than the Ariſtotelians. And indeed I am apt to think they are ſo, but they may well be ſo, without deſerving to have any of their ſects looked upon as philoſophy itſelf, there being none of them, that I know of, that maintain any opinion inconſiſtent with Chriſtianity, that I think may not be made appear to be alſo repugnant to reaſon, or at leaſt not demonſtrable by it. You will not expect I ſhould deſcend to particulars, eſpecially having expreſly diſcourſed againſt the Epicurean hypotheſes of the origine of the world in another paper; and therefore, I ſhall obſerve to you in general, that the Carteſian philoſophers, who lay aſide all ſupernatural revelation in their inquiries into natural things, do yet both think, and, as to the two firſt of them, very plauſibly prove, the three grand principles of *Epicurus*, that the little bodies he calls atoms are indiviſible, that they all have their motion from themſelves, and, that there is a vacuum in *rerum naturâ*, to be as repugnant to mere reaſon, as the Epicureans think the notion of an incorporeal ſubſtance, or the creation of the world, or the immortality of the ſoul. And as for the new Somatici, ſuch as Mr. *Hobbes* (and ſome few others) by what I have yet ſeen of his, I am not much tempted to forſake any thing, that I looked upon as a truth before, even in natural philoſophy itſelf, upon the ſcore of what he (though never ſo confidently) delivers, by which hitherto I ſee not, that he hath made any great diſcovery either of new truths, or old errors. An honourable member of the Royal Society hath elſewhere purpoſely ſhewn, how ill he has proved his own opinions about the air, and ſome other phyſical ſubjects, and how ill he has underſtood and oppoſed thoſe of his adverſary. But to give you in this place a ſpecimen, how little their repugnancy to his principles of natural philoſophy ought to affright us from thoſe theological doctrines they contradict, I ſhall here examine the fundamental maxim of his whole phyſicks, that nothing is removed but by a body contiguous and moved; it having been already ſhewn (by the gentleman newly mentioned) that, as to the next to it, which is, that there is no vacuum, whether it be true or no, he has not proved it.

[IF no body can poſſibly be moved, but by a body contiguous and moved, as Mr. *Hobbes* teaches; I demand, how there comes to be local motion in the world? For, either all the portions of matter, that compoſed the univerſe, have motion belonging to their nature, which the Epicureans affirmed for their atoms; or ſome parts of matter have this motive power, and ſome have not; or elſe none of them have it, but all of them are naturally devoid of motion. If it be granted, that motion does naturally belong to all parts of matter, the diſpute is at an end, the conceſſion quite overthrowing the hypotheſis. If it be ſaid, that naturally ſome portions of matter have motion, and others not, then the aſſertion will not be univerſally true: for though it may hold in the parts, that are naturally moveleſs, or quieſcent, yet it will not do ſo in the others, there being nothing, that may ſhew a neceſſity, why a body, to which motion is natural, ſhould not be capable of moving, without being put into motion by another contiguous and moved. And if there be no body, to which motion is natural, but every body needs an outward movent, it may well be demanded, how there comes to be any thing locally moved in the world? which yet conſtant and obvious experience demonſtrates, and Mr. *Hobbes'* himſelf cannot deny. For if no part of matter have any motion but what it muſt owe to another, that is contiguous to it, and being itſelf in motion, impels it; and if there be nothing but matter in the world, how can there come to be any motion amongſt bodies, ſince they neither have it upon the ſcore of their own nature, nor can receive it from external agents? If Mr. *Hobbes* ſhould reply, that the motion is impreſſed upon any of the parts of the matter by God, he will ſay that, which I moſt readily grant to be true, but will not ſerve his turn, if he would ſpeak congruouſly to his own hypotheſis. For I demand, whether this ſupreme Being, that the aſſertion has recourſe to, be a corporeal or an incorporeal ſubſtance? If it be the latter, and yet be the efficient cauſe of motion in bodies, then it will not be univerſally true, that whatſoever body is moved, is ſo by a body contiguous and moved. For, in our ſuppoſition, the bodies, that God moves, either immediately, or by the intervention of any other immaterial being, are not moved by a body contiguous, but by an incorporeal ſpirit. But becauſe Mr. *Hobbes*, in ſome writings of his, is believed to think the very notion of an immaterial ſubſtance to be abſurd, and to involve a contradiction; and becauſe it may be ſubſumed, that if God be not an immaterial ſubſtance, he muſt by conſequence be a material and corporeal one, there being no *medium negationis*, or third ſubſtance, that is none of thoſe two. I anſwer, that, if this be ſaid, and ſo that Mr. *Hobbes*'s deity be a corporeal one; the ſame difficulty will recur, that I urged before. For this body will not, by Mr. *Hobbes*'s calling or thinking it divine, ceaſe to be a true body; and conſequently a portion of divine matter will not be able to move a portion of our mundane matter, without it be itſelf contiguous and moved; which it cannot be, but by another portion of divine matter, ſo qualified to impreſs a motion, nor this again, but by another portion.

AND beſides that it will breed a ſtrange confuſion, in rendering the phyſical cauſes of things, unleſs an expedient be found, to teach us how to diſtinguiſh accurately the mundane bodies from the divine, (which will perhaps prove no eaſy taſk;) I ſee not yet, how this corporeal deity will make good the hypotheſis I examine. For I demand, how this divine matter comes to have this local motion, that is aſcribed to it? If it be anſwered, that it hath it from its own nature, without any other cauſe, ſince the Epicureans affirm the ſame of their atoms, or merely mundane matter, I demand, how the truth of Mr. *Hobbes*'s opinion will appear to me, to whom it ſeems as likely by the phænomena of nature, that occur, that mundane matter ſhould have a congenit motion, as that any thing, that is corporeal, can be God, and capable of moving it; which to be, it muſt, for aught we know, have its ſubſiſtence divided into as many minute parts, as there are corpuſcles and particles in the world, that move ſeparately from their neighbouring ones. And, to draw towards a concluſion, I ſay, that theſe minute divine bodies, that thus moved thoſe portions of mundane matter, concerning which Mr. *Hobbes* denies, that they can be moved but by bodies contiguous and moved, theſe divine ſubſtances, I ſay, are, according to the late ſuppoſition, true bodies, and yet are moved themſelves, not by bodies contiguous and moved, but by a motion, which muſt be innate, derived or flowing from their very eſſence or nature, ſince no ſuch body is pretended to have a being, as cannot be referred as a portion, either to the mundane, or the divine matter. In ſhort, ſince local motion is to be found in one, if not in both, of theſe two matters, it muſt be natural to (at leaſt ſome parts of) one of them in Mr. *Hobbes*'s hypotheſis; for, though he ſhould grant an immaterial being, yet it could not produce a motion in any body, ſince, according to him, no body can be moved, but by another body contiguous and moved.]

AS then to this grand poſition of Mr. *Hobbes*, though, if it were cautiouſly propoſed, as it is by *Des Cartes*, it may perhaps be ſafely admitted, becauſe *Carteſius* acknowledges the firſt impulſe, that ſet matter a moving, and the conſervation of motion once begun, to come from God; yet, as it is crudely propoſed by the favourers of Mr. *Hobbes*, I am ſo far from ſeeing any ſuch cogent proof for it, as were to be wiſhed for a principle, on which he builds ſo much, (and which yet is not at all evident by its own light,) that I ſee no competent reaſon to admit it.

I expect your friend ſhould here oppoſe to what I have been ſaying, that formerly recited ſentence, that is ſo commonly employed in the ſchools, as well of divines as of philoſophers: that ſuch or ſuch an opinion is true in divinity, but falſe in philoſophy; or, on the contrary, philoſophically true, but theologically falſe.

UPON what warrant thoſe, that are wont to employ ſuch expreſſions, ground their practice, I leave to them to make out; but as to the objection itſelf, as it ſuppoſes theſe ways of ſpeaking to be well grounded, give me leave to conſider, that philoſophy may ſignify two things, which I take to be very differing.

FOR, firſt, it is moſt commonly employed to ſignify a ſyſtem, or body of the opinions, and other doctrines of the particular ſect of thoſe philoſophers, that make uſe of the word. As when an Ariſtotelian talks of philoſophy, he uſually means the Peripatetick, as an Epicurean does the Atomical, or a Platoniſt the Platonick.

BUT we may alſo, in a more general, and no leſs juſt acception of the term, underſtand by philoſophy, a comprehenſion of all thoſe truths or doctrines, which the natural reaſon of man, freed from prejudices and partiality, and aſſiſted by learning, attention, exerciſe, experiments, &c. can manifeſtly make out, or, by neceſſary conſequence deduce from clear and certain principles.

THIS being briefly premiſed, I muſt, in the next place, put you in mind of what I formerly obſerved to you, that many opinions are maintained by this, or that ſect of Chriſtians, or perhaps by the divinity-ſchools of more than one or two ſects, which either do not at all belong to the Chriſtian religion, or, at leaſt, ought not to be looked upon as parts of it, but upon ſuppoſition, that the philoſophical principles and ratiocinations, upon which, and not upon expreſs or mere revelation, they are preſumed to be founded, are agreeable to right reaſon.

AND having premiſed theſe two things, I now anſwer more directly to the objection; that, if philoſophy be taken in the firſt ſenſe above-mentioned, it's teaching things repugnant to theology, eſpecially taking this word in the more large and vulgar ſenſe of it, will not cogently conclude any thing againſt the Chriſtian religion. But, if philoſophy be taken in the latter ſenſe for true philoſophy, and divinity only for a ſyſtem of thoſe articles, that are clearly revealed as truths in the ſcriptures; I ſhall not allow any thing to be falſe in philoſophy ſo underſtood, that is true in divinity ſo explained, till I ſee ſome clearer proof of it, than I have yet met with. I have had occaſion, in the foregoing diſcourſe, to ſay ſomething, that may be applied to the point under debate; and in the following part of this letter, I ſhall have occaſion to touch upon it again: and therefore I ſhall now ſay but this in ſhort, that it is not likely, that God, being the author of reaſon as well as revelation, ſhould make it men's duty to believe as true, that, which there is juſt reaſon to reject as falſe.

There is indeed a ſenſe, wherein the phraſes, I diſapprove, may be tolerated. For if by ſaying, that ſuch a thing is true in divinity, but falſe in philoſophy, it were meant, that if the doctrine were propoſed to a mere philoſopher, to be judged of according to the principles of his ſect, or at moſt, according to what he, being ſuppoſed not to have heard of the Chriſtian religion, or had it duly propoſed to him, would reject it, the phraſe might be allowed, or at leaſt indulged. But then we muſt conſider, that the reaſon, why ſuch a philoſopher would reject the articles of Chriſtian faith, would not be, becauſe they could by no mediums be poſſibly proved, but becauſe theſe doctrines, being founded upon a revelation, which he is preſumed either not to have heard of, or not to have had ſufficiently propoſed to him, he muſt, as a rational man, refuſe to believe them upon the ſcore of their prooſleſneſs. And the ſame philoſopher, ſuppoſing him to be a true one, though he will be very wary, how he admits any thing as true, that is not proved, if it fall properly under the cognizance of philoſophy; yet he will be as wary, how he pronounces things to be falſe or impoſſible, in matters, which he diſcerns to be beyond the reach of mere natural reaſon, eſpecially if ſober and learned men do very confidently pretend to know ſomething of thoſe matters by divine revelation, which though he will not eaſily believe to be a true one, yet he will admit, in caſe it ſhould be proved true, to be a fit medium to evince truths, which, upon the account of mere natural light, he could not diſcover or embrace. To be ſhort, ſuch a philoſopher would indeed reject ſome of the articles of our faith hypothetically, i. e. upon ſuppoſition, that he need employ no other touchſtone to examine them by, than the principles and dictates of natural philoſophy, that he is acquainted with (upon which ſcore I ſhall hereafter ſhew, that divers ſtrange chemical experiments, and other diſcoveries would alſo be rejected;) but yet he would not pronounce them falſe, but upon ſuppoſition, that the arguments, by which they lay claim to divine revelation, are incompetent in their kind. For as he will not eaſily believe any thing within the ſphere of nature, that agrees not with the eſtabliſhed laws of it; ſo he will not eaſily adventure to pronounce one way or other in matters, that are beyond the ſphere of nature: he will indeed, as he juſtly may, expect as full a proof of the divine teſtimony, that is pretended, as the nature of the thing requires and allows; but he will not be backward to acknowledge, that God, to whom that teſtimony is aſcribed, is able to know and to do many more things, than we can explicate how he can diſcover, or imagine how any phyſical agent can perform.

[Since I propoſed to you this fifth conſideration, I happened to light on a paſſage in *Des Cartes*'s principles, which affords of what I have been diſcovering the ſuffrage of a philoſopher, that is wont to be accuſed of excluding theology too ſcrupulouſly out of his philoſophy. His words are ſo full to my preſent purpoſe, that I need not, to accommodate them to it, alter one of them, and therefore ſhall tranſcribe them juſt as they lie; *Si fortè nobis Deus de ſeipſo, vel aliis, aliquid revelet, quod naturales ingenii noſtri vires excedat, qualia ſunt myſteria incarnationis & trinitatis, non recuſabimus illa credere, quamvis non clarè intelligamus, nec ullo modo mirabimur, multa eſſe tum in immenſa ejus natura, tum etiam in rebus ab eo creatis, quæ captum noſtrum excedant.*]

Princip. Philoſ. part. prima. Artic. 25.

And let me add on this occaſion, that whereas the main ſcruples, that are ſaid to be ſuggeſted by philoſophy againſt ſome myſterious articles of religion, are grounded upon this, that the modus, as they ſpeak of thoſe things, is not clearly conceivable, or at leaſt, is very hardly explicable; theſe objections are not always ſo weighty, as perhaps, by the confidence, wherewith they are urged, you may think them. For, whereas I obſerved to you already, that there are divers things maintained by ſchool divines, which are not contained in the ſcripture, that obſervation is chiefly applicable to the things we are conſidering; ſince in ſeveral of theſe nice points, the ſcripture affirms only the thing, and the ſchoolmen are pleaſed to add the modus: and as by their unwarrantable boldneſs, the ſchool divines determine many things without book; ſo the ſcruples and objections, that are made againſt what the ſcripture really delivers, are uſually grounded upon the erroneous or precarious aſſertions of the ſchool philoſophers, who often give the title of metaphyſical truths to conceits, that do very little deſerve that name, and to which a rigid philoſopher would perhaps think, that of ſublime nonſenſe more proper. But of this I elſewhere ſay enough, and therefore ſhall now proceed to the conſideration I chiefly intended, viz. that from hence, that the modus of a revealed truth is either very hard, or not at all explicable, it will not neceſſarily follow, that the thing itſelf is irrational, provided the poſitive proofs of its truth be ſufficient in their kind. For even in natural things philoſophers themſelves do and muſt admit ſeveral things, whereof they cannot clearly explicate or perhaps conceive the modus. I will not here mention the origin of ſubſtantial forms as an inſtance in this kind, becauſe, though it may be a fit one as to the Peripatetick philoſophy, yet not admitting, that there are any ſuch beings, I will take no farther notice of them; eſpecially becauſe, for a clear inſtance to our preſent purpoſe, we need go no further than our ſelves, and conſider the union of the ſoul and body in man. For who can phyſically explain, both how an immaterial ſubſtance ſhould be able to guide or determine, and excite the motions of a body, and yet not be able to produce motion in it (as by dead palſies, great faintneſſes, &c. it appears the ſoul cannot,) and, which is far more difficult, how an incorporeal ſubſtance ſhould receive ſuch impreſſions from the motions of a body, as to be thereby affected with real pain and pleaſure; to which I elſewhere add ſome other properties of this union, which, though not taken notice of, are perhaps no leſs difficult to be conceived and accounted for. For how can we comprehend, that there ſhould be naturally ſuch an intimate union betwixt two ſuch diſtant ſubſtances, as an (incorporeal) ſpirit and a body, as that the former may not, when it pleaſes, quit the latter, which cannot poſſibly have any ſtrings or chains, that can tie, or faſten to it that, which has no body, on which they may take hold. And I there ſhew, that it is full as difficult, phyſically to explicate, how theſe ſo differing beings come to be united, as how they are kept from parting at pleaſure, both the one and the other being to be reſolved into the mere appointment of God. And if to avoid the abſtruſeneſs of the modus of this conjunction betwixt the rational ſoul and the human body, it be ſaid, as it is by the Epicureans, that the former is but a certain contexture of the finer and moſt ſubtle parts of the latter, the formerly propoſed abſtruſeneſs of the union betwixt the ſoul and the body will indeed be ſhifted off; but it will be by a doctrine, that will not much relieve us. For thoſe, that will allow no ſoul in man but what is corporeal, have a modus to explain, that I doubt they will always leave a riddle. For of ſuch I deſire, that they would explain to me, (who know no effects, that matter can produce, but by local motion and reſt, and the conſequences of it,) how mere matter, (let them ſuppoſe it as fine as they pleaſe, and contrive it as well as they can) can make ſyllogiſms, and have conceptions of univerſals, and invent ſpeculative ſciences and demonſtrations, and in a word do all thoſe things, which are done by man, and by no other animal; and he, that ſhall intelligibly explicate to me the modus of matters, framing theories and ratiocinations, will, I confeſs, not only inſtruct me, but ſurprize me too.

AND now give me leave to make this ſhort reflection on what has been ſaid in this ſection, compared with what formerly I ſaid in the firſt ſection; that if on the one hand we lay aſide all the irrational opinions, that the ſchoolmen and other bold writers have unwarrantably fathered on Chriſtian religion, and on the other hand all the erroneous conceits repugnant to Chriſtianity, which the ſchoolmen and others have proofleſly fathered upon philoſophy, the ſeeming contradictions betwixt ſolid divinity and true philoſophy will appear to be but few, as I think the real ones will be found to be none at all.

SECTION VI.

THE next conſideration I ſhall propoſe, is, that a thing may, if ſingly or preciſely conſidered, appear unreaſonable, which yet may be very credible, if conſidered as a part of, or a manifeſt conſequence from a doctrine, that is highly ſo.

OF this I could give you more inſtances in ſeveral arts and ſciences, than I think fit to be here ſpecified; and therefore I ſhall content my ſelf to mention three or four.

WHEN aſtronomers tell us, that the ſun, which ſeems not to us a foot broad, nor conſiderably bigger than the moon, is above a hundred and threeſcore times bigger than the whole globe of the earth, which yet is forty times greater than the moon; the thing thus nakedly propoſed ſeems very incredible. But yet, becauſe aſtronomers very ſkilful in their art have, by finding the ſemidiameter of the earth, and obſerving the parallaxes of the planets, concluded the proportion of theſe three bodies to be ſuch as has been mentioned, or thereabout, even learned and judicious men of all ſorts, (philoſophers, divines, and others,) think it not credulity to admit what they affirm.

So the relations of earthquakes, that have reached divers hundreds of miles; of eruptions of fire, that have at once overflown and burned vaſt ſcopes of land; of the blowing up of mountains by their own fires; of the caſting up of new iſlands in the ſea itſelf, and other prodigies of too unqueſtionable truth; (for I know what work ignorance and ſuperſtition have made about other prodigies:) if they were atteſted but by ſlight and ordinary witneſs, they would be judged incredible, but we ſcruple not to believe them, when the relations are atteſted with ſuch circumſtances, as make the teſtimony as ſtrong, as the things atteſted are ſtrange.

IF ever you have conſidered, what *Clavius*, and divers other geometricians teach upon the ſixteenth propoſition of the third book of *Euclid*, (which contains a theorem about the tangent, and the circumference of a circle,) you cannot but have taken notice, that there are ſcarce greater paradoxes delivered by philoſophers or divines, than you will find aſſerted by geometricians themſelves. And though of late the learned Jeſuit *Tacquet*, and ſome rigid mathematicians, have queſtioned divers of thoſe things, yet even what ſome of theſe ſevere examiners confeſs to be geometrically demonſtrable from that propoſition, contains things ſo ſtrange, that philoſophers themſelves, that are not well acquainted with that propoſition and its corollaries, can ſcarce look upon them as other than incomprehenſible, or at leaſt incredible, things; which yet, as improbable as they are conſidered in themſelves, even rigid demonſtrators refuſe not to admit, becauſe they are legitimately deducible from an acknowledged truth.

AND ſo alſo among the magnetical phænomena there are divers things, which, being nakedly propoſed, muſt ſeem altogether unfit to be believed, as indeed having nothing like them in all nature; whereas thoſe, that are verſed in magnetick philoſophy, even before they have made particular trials of them, will look upon them as credible, becauſe, how great paradoxes ſoever they may ſeem to others, they are conſonant and conſequent to the doctrine of magnetiſm, whoſe grand axioms (from what cauſe ſoever magnetiſms are to be deriv'd) are ſufficiently manifeſt; and therefore a magnetical philoſopher would not, though an ordinary philoſopher would, think it unreaſonable to believe, that one part of the ſame loadſtone ſhould draw a needle to it, and the other part drive the ſame needle from it; and that the needle in a ſeaman's compaſs, after having been carried many hundred leagues (through differing climates, and in ſtormy weather) without varying its declination, may, upon a ſudden, without any manifeſt cauſe, point at ſome part of the horizon ſeveral whole degrees diſtant from that, which it pointed to before. To which might here be added divers other ſcarce credible things, which either others or I have tried about magnetical bodies; but I ſhall hereafter have occaſion to take notice of ſome of them in a fitter place.

WHEREFORE, when ſomething delivered in, or clearly deduced from ſcripture, is objected againſt, as a thing, which it is not reaſonable to believe, we muſt not only conſider, whether, if it were not delivered in that book, we ſhould upon its own ſingle account think it fit or unworthy to be believed; but whether or no it is ſo improbable, that it is more fit to be believed, that all the proofs, that can be brought for the authority of the ſcripture, are to be rejected, than that this thing, which comes manifeſtly recommended to our belief by that authority, is worthy to be admitted: I ſay, "manifeſtly recommended by that authority," becauſe that, if the thing be not clearly delivered in ſcripture, or be not clearly and cogently deduced thence, ſo far as that clearneſs is wanting, ſo far the thing itſelf wants of the full authority of the ſcripture, to impoſe it on our aſſent.

[PERHAPS it will procure what I have ſaid the better reception, if I add a couple of teſtimonies not of any modern bigots, no, nor of any devout fathers of the church; but of two modern authors of ſects, and who in their kinds have been thought extremely ſubtle reaſoners, and no leſs rigid exacters of reaſon in whatever they admitted.

THE firſt paſſage I ſhall alledge, is the confeſſion of *Socinus*, who in his ſecond epiſtle to *Andreas Dudithius*, ſpeaks thus: *Jam verò ut rem in pauca conferam, quod ad meas aliorúmve opiniones, quæ novitatis præ ſe ferunt ſpeciem, attinet, mihi ita videtur; ſi detur, ſcripturam ſacram ejus eſſe authoritatis, ut nullo modo ei contradici poſſit, ac de interpretatione illius omnis duntaxat ſit ſcrupulus,* (which he allows) *nihil, utut veriſimile aut ratione concluſum videatur, afferri contra eas poſſit, quod ullarum ſit virium, quotieſcunque illæ ſententiis atque verbis illius libri aut rationibus liquidò inde deductis probatæ atque aſſertæ fuerint.* Which confeſſion of *Socinus* is ſurpaſſed by that of his champion *Smalcius*, to be produced elſewhere in this paper. Part II. Artic. 34, 35. The other paſſage I met with in the excellent Monſieur *Des Cartes*'s principles of philoſophy, where diſcourſing of the either infinite or indefinite diviſion of the particles of matter, which is neceſſary to make them fill exactly all the differingly figured ſpaces, through which various motions do ſometimes make them paſs; he confeſſes, as he well may, that the point is exceedingly abſtruſe, and yet concludes: *Et quamvis quomodo fiat indefinita iſta diviſio, cogitatione comprehendere nequeamus, non ideo tamen debemus dubitare, quin fiat, quia clarè percipimus illam neceſſario ſequi ex natura materiæ nobis evidentiſſimè cognitâ,* &c.]

AND in this place it may be ſeaſonable, as well as pertinent, to take notice of three or four particulars, which, though they be in ſome meaſure implied in the former general conſideration, yet deſerve to be diſtinctly inculcated here, both for their importance, and becauſe they may as well be deduced as corollaries from the foregoing diſcourſe, as be confirmed by the proofs I ſhall add to each of them. Of theſe the firſt ſhall be this, that we muſt not preſently conclude a thing to be contrary to reaſon, becauſe learned men profeſs or even complain, that they are not able clearly to comprehend it, provided there be competent proof, that it is true, and the thing be primary or heteroclite.

FOR it is not always neceſſary to the making the belief of a thing rational, that we have ſuch a comprehenſion of the thing believed as may be had, and juſtly required in ordinary caſes; ſince we may be ſure of the truth of a thing, not only by arguments ſuggeſted by the nature of the thing itſelf clearly underſtood by us, but by the external teſtimony of ſuch a witneſs, as we know will not deceive us, and cannot (at leaſt in our caſe) be reaſonably ſuſpected to be himſelf deceived. And therefore it may in ſome caſes ſuffice to make our belief rational, that we clearly diſcern ſufficient reaſon to believe, that a thing is true, whether that reaſon ſpring from the evidence and cogency of the extrinſick motives we have to believe, or from the proofs ſuggeſted to us by what we know of the thing believed, nay, though there be ſomething in the nature of that thing, which does puzzle and poſe our underſtanding.

THAT many things, that are very hard, and require a great attention, and a good judgment to be made out, may yet be true, will be manifeſt from what I ſhall, within a page or two, note about divers geometrical demonſtrations, which require, beſides a good ſtock of knowledge in thoſe matters, an almoſt invincible patience, to carry ſo many things along in one's mind, and go through with them. That alſo there are other things, which, though they be as manifeſtly exiſtent, as thoſe newly mentioned can be demonſtratively true, are yet of ſo abſtruſe a kind, that it is exceeding difficult to frame clear and ſatisfactory notions of their nature, we might learn, if we were inquiſitive enough, even from ſome of the moſt obvious things; ſuch as, for inſtance, matter and time: As to the former whereof, (matter,) though the world and our own bodies be made of it, yet the ideas, that are wont to be framed of it, even by the greateſt clerks, are incumbered with too great difficulties (ſome of which I elſewhere mention) to be eaſily acquieſced in by conſidering men. And as for the latter, (time,) though that juſtly celebrated ſaying of *Auguſtine*, *Si nemo ex me quærat, quid ſit tempus, ſcio; ſi quærenti explicare velim, neſcio*; ſeem in the firſt part of it to own a knowledge of what time is, yet by the latter part, (wherein he confeſſes he cannot declare what it is,) I am not only allowed to believe, that he could not propoſe an intelligible idea of it, but invited to think, that, in the firſt part of the ſentence, he only meant, that when he did not attentively conſider the nature of it, he thought he underſtood it, or that he knew, that there is ſuch a thing as time, though he could not explain what it is.

AND indeed, though time be that, which all men allow to be, yet, if *per impoſſibile* (as the ſchools ſpeak) a man could have no other notion or proof of time and eternity, (even ſuch eternity as muſt be conceded to ſomething,) than what he could collect from the beſt deſcriptions of its nature and properties, that are wont to be given; I ſcarce doubt, but he would look upon it as an unintelligible thing, and incumbered with too many difficulties to be fit to be admitted into a wiſe man's belief. And this perhaps you will grant me, if you have ever put yourſelf to the penance of peruſing thoſe confounding diſputes and ſpeculations about time and eternity, that partly in *Ariſtotle* and his commentators, and partly among the ſcoolmen, and others, are to be met with upon theſe abſtruſe ſubjects. And no wonder, ſince the learned *Gaſſendus* and his followers have very plauſibly (if not ſolidly) ſhewn, that duration (and time is but duration meaſured) is neither a ſubſtance nor an accident, which they alſo hold of ſpace; about which the altercations among philoſophers and ſchoolmen are but little, if at all, inferior to thoſe about time. And I the rather chooſe to mention theſe inſtances of time and ſpace, becauſe they agree very well with what I intimated by the expreſſion of primary or heteroclite things.

To which may be referred ſome of thoſe things, that are called ſpiritual or ſupernatural, about which the ſame conſiderations may have place, eſpecially by reaſon of this affinity between them, that when we treat of either, ſome proofs may in certain caſes be ſufficient, in ſpite of ſuch objections, as in other (and more ordinary caſes) would invalidate arguments ſeemingly as ſtrong as thoſe proofs.

IF it be here objected, that I am too bold in venturing, without the precedence or authority of learned men, to introduce ſo great a difference betwixt other things, and thoſe, which I call primary and heteroclite; I anſwer, that I ſhall not ſolicitouſly enquire, whether any others have had the ſame thoughts, that I propoſed; ſince, whether they be new or no, they ought not to be rejected, if they be rational.

AND I have this inducement to ſuppoſe, that there ought to be in ſome caſes a great difference between them and other things, and conſequently between the judgments we make of the ways of arguing about them, and about other things; ſo that they are exceeding difficult, to be clearly conceived and explicated by our imperfect faculties, and by that difficulty, apt to make what men ſay of them, though true, to be leſs ſatisfactory and acquieſced in, than things not more true or rational, ſuggeſted upon enquiries about ſubjects more familiar, or which are, at leaſt, more proportionate to our faculties: for thoſe abſtruſe things, of which we have been ſpeaking, being ſuch, as either have no proper and clear genus, by the help of which they may be comprehended, or have not any thing in nature, that is (ſufficiently, like them) by a reſemblance to which we may conceive them; or being perhaps, both primary and heteroclite too, as not being derived from the common phyſical cauſes of other things, and having a nature widely differing from the reſt of things; it is no wonder, that our limited and imperfect underſtandings ſhould not be able to reach to a full and clear comprehenſion of them; but ſhould be ſwallowed up with the ſcruples and difficulties, that may be ſuggeſted by a bold and nice enquiry into things, to which there ſeems to belong, in ſome reſpect or other, a kind of infinity.

UPON theſe, and other conſiderations of kin to them, I count it not irrational to think, that things primary and heteroclite, as alſo by a parity of reaſon, ſome things immaterial and ſupernatural, may be ſufficiently proved in their kind, if there be ſuch a poſitive proof of them, as would be competent and ſatisfactory, in caſe there were no conſiderable objections made againſt the thing proved (eſpecially ſuppoſing, that, the aſſerted doctrine be not incumbered with much greater inconveniencies than the contrary doctrine, or than any other, propoſed concerning that ſubject:) nay, I know not, why we may not, in judging of primary, and of immaterial things, ſafely enough prefer that opinion, which has the more cogent poſitive proofs, though it ſeem liable to ſomewhat the greater inconveniencies; becauſe, in ſuch caſes, our underſtanding is gratified with what it moſt requires in all caſes, that is, competent poſitive inducements to aſſent; and it is not confounded by the objections, becauſe a diſability to anſwer them directly, and fully, may very well proceed, either from the too abſtruſe nature of the thing, or the limitedneſs and weakneſs of our human intellects.

AND thus we may render a reaſon, why, when we diſcourſe of ſuch uncommon matters, we may ſometimes reaſonably acquieſce in proofs, in ſpight of ſuch objections, as in ordinary caſes would be prevailing ones. For the things, about which theſe proofs are converſant, being primary or heteroclite, or of as abſtruſe a nature, as if they were ſo, it too often happens, that, what opinion ſoever we chooſe about them, we muſt admit ſomething, that is incumbered with great difficulties, and therefore will be liable to great objections, that perhaps will never be directly and ſatisfactorily anſwered. And ſince it may fare thus with us, where two oppoſite opinions are contradictory, we may conclude, that thoſe difficulties will not cogently evince the falſity of a theological opinion, which are but ſuch, that the ſame, or as great, may be objected againſt another, that either is manifeſtly or confeſſedly a truth, or which muſt neceſſarily be admitted to be one, if the contrary theological tenet be ſuppoſed not to be one.

2. ANOTHER corollary, that may be drawn from the diſcourſe, that afforded us the former, may be this; that it may not be unreaſonable to believe a thing, though its proof be very difficult to be underſtood. To manifeſt this, I ſhall need no other argument, than what may be afforded by divers geometrical and other mathematical demonſtrations; ſome of which are fetched, by intermediate concluſions, from principles ſo very remote, and require ſo long a ſeries of mediums to be employed about them, that not only a man, that were of *Pilate*'s temper, who having aſked him, that could beſt tell him, what is truth? would not ſtay a while to be ſatisfied about his enquiry, would, before he reaches half way to the end of the demonſtration, or perhaps of the lemma's, be quite diſcouraged from proceeding any further; but even ſedulous and heedful peruſers do find themſelves oftentimes unable to carry along ſuch a chain of inferences in their minds, as clearly to diſcern, whether the whole ratiocination be coherent, and all the particulars have their due ſtrength and connection. And if you pleaſe to make a trial upon ſome of the demonſtrations of *Vitellio*, or even of *Clavius*, that I can direct you to, I doubt they will put you to the full exerciſe of your patience, and quite tire your attention: and though the modern algebriſts, by their excellent way of expreſſing quantities by ſymbols, have ſo abridged geometrical and arithmetical demonſtrations, that, by the help of ſpecies, it is ſometimes eaſy to demonſtrate, that in a line, which in the ordinary way would require a whole page, (as our moſt learned friend Dr. *Ward* has ingeniouſly ſhewn, by giving the demonſtrations of about twenty of Mr. *Hobbes*'s theorems, in leſs than ſo many lines;) yet ſome demonſtrable truths are ſo abſtruſe, that, even in the ſymbolical way, men need more attention to diſcern them, than moſt men would employ in any ſpeculation whatſoever. And *Des Cartes* himſelf, as famous and expert a maſter as he was in this way, confeſſes, in a letter to one of his friends, that the ſolution of a problem in *Pappus* coſt him no leſs than ſix weeks ſtudy; though now, moſt mathematical demonſtrations do indeed ſeem far ſhorter than they are, becauſe that *Euclid*'s elements being generally received among mathematicians, all his propoſitions are ſo many lemmata, which need be but referred to in the margin, being known and demonſtrated already. By all which it may appear, that, granting ſome theological truths to be complained of by many, as things ſo myſterious and abſtruſe, that they cannot readily diſcern the force of thoſe proofs, that *Des Cartes*, and other ſubtile ſpeculators, have propoſed to evince them; yet if other learned men, that are competent eſtimators, and are accuſtomed to bring much patience and attention to the diſcernment of difficult and important truths, profeſs themſelves ſatisfied with them, the probations may yet be cogent, notwithſtanding the difficulty to have their ſtrength apprehended. For, if ſuch a difficulty ought to paſs for a mark, that a ratiocination is not valid, no reaſonings will be found fitter to be rejected or diſtruſted, than many of thoſe, whoſe cogency has procured ſuch a repute to mathematical demonſtrations.

3. IT may alſo be deduced from the foregoing diſcourſe, that it is not always againſt reaſon to embrace an opinion, which may be incumbered with a great difficulty, or liable to an objection not eaſy to be ſolved; eſpecially if the ſubject be ſuch, that other opinions about it avoid not either the ſame inconveniencies, or as great ones. The firſt part of what is ſaid in this conſideration will often follow from the ſuppoſition made in the precedent diſcourſe. For thoſe things, that render a doctrine or aſſertion difficult to be conceived and explained, will eaſily ſupply the adverſaries of it with objections againſt it.

AND as for the latter, viz. the clauſe, which takes notice, that the conſideration, to which it is annexed, will chiefly take place in that ſort of opinions, that are ſpecified in it; it will need but little of diſtinct proof.

FOR it is manifeſt enough, that if the ſubject or object, about which the opinion propoſed is converſant, be ſuch, that not only the contradictory opinion, but others alſo, are obnoxious either to the ſame inconveniencies, or to others, that are equal or greater; the difficulties, that are urged againſt a theological doctrine, may (as hath been ſhewn already in the firſt corollary,) be rationally enough attributed, not to the unreaſonableneſs of the opinion, but to ſomewhat elſe.

THE laſt conſectary, that (as I intimated) may be deduced from the precedent diſcourſe, is, that it is not always unreaſonable to believe ſomething theological for a truth, which (I do not ſay, is truly inconſiſtent with, but) we do not clearly diſcern to comport very well with ſomething elſe, that we alſo take for a truth, or perhaps, that is one indeed; if the theological tenet be ſufficiently proved in its kind, and be of that ſort of things, that we have been of late, and are yet diſcourſing of.

THE generality of our philoſophers, as well as divines, believe, that God has a foreknowledge of all future contingencies; and yet how a certain preſcience can conſiſt with the freewill of man, (which yet is generally granted him, in things merely moral or civil,) is ſo difficult to diſcern, that the Socinians are wont to deny ſuch things, as depend upon the will of free agents, to be the proper objects of omniſcience; and the head of the Remonſtrants, though a very ſubtile writer, confeſſes, that he knows not, how clearly to make out the conſiſtency of God's preſcience, and man's freedom; both which he yet confeſſes to be truths, being compelled to acknowledge the former, (for the latter is evident,) as well by the infiniteneſs, that muſt be aſcribed to God's perfections, as by the prophetick predictions, whereby ſuch contingent events have been actually foretold. And the reconcilement of theſe truths is not a difficulty peculiar to the Chriſtian religion, but concerns ſpeculative men in all religions, who acknowledge the Deity to be infinitely perfect, and allow man, as they do, to be a free agent.

[But I have made this ſection ſo prolix already, that I muſt not enlarge on this third particular. And therefore I ſhall ſhut it up with an acknowledgment of *Des Cartes*, which may be applied not only to it, but to almoſt all, that has been diſcourſed in this ſection, and indeed to a great part of this letter. He then in an epiſtle, that came not forth, till ſome years after the writer's death, ſpeaks thus to the philoſophical adverſary, to whom it is addreſſed: "As I have often ſaid, when the "queſtion is about things, that relate to God, "or to what is infinite, we muſt not conſider, "what we can comprehend of them, (ſince "we know, that they ought not to be compre"hended by us) but only what we can con"ceive of them, or can attain to by any cer"tain reaſon or argument.

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SECTION VII.

AND now it is time to advance to one of the main conſiderations I had to propoſe to you, concerning the ſubject of this letter, and it is this; that when we are to judge, whether a thing be contrary to reaſon or not, there is a great deal of difference, whether we take reaſon for the faculty furniſhed only with its own innate principle, and ſuch notions, as are generally obvious, (nay, and if you pleaſe, with this or that philoſophical theory;) or for the faculty illuminated by divine revelation, eſpecially that, which is contained in the books commonly called the Scripture.

To clear and enforce this the better, I ſhall invite you to take notice with me of the two following particulars.

WE may then in the firſt place conſider, that even in things merely natural, men do not think it at all irrational, to believe divers ſuch things upon extrinſecal proofs, eſpecially the teſtimony of the ſkilful, as, if it were not for that teſtimony, a man, though born with good parts, and poſſibly very learned in the Peripatetick, or ſome other particular philoſophy, would look upon as irrational to be believed, and contrary to the laws of nature.

OF this I ſhall give you ſome inſtances in the phænomena of the loadſtone, and particularly ſuch as theſe; that the loadſtone, though (as was above intimated) with one part it will draw, yet with another the ſame ſtone will repel the ſame point of the ſame excited needle; and yet at the ſame time be fit to attract either point of another needle, that never came near a loadſtone before: that though it be the loadſtone, that imparts an attractive virtue to the iron, yet when the load-ſtone is capped, as they called it, and ſo a piece of iron (and conſequently a diſtance) is interpoſed betwixt the ſtone and the weight to be raiſed, it will take up by many times more, than if it be itſelf applied immediately thereunto, inſomuch that *Merſennus* relates, that (if there be no miſtake,) he had a load-ſtone, that of itſelf would take up but half an ounce of iron, which when armed (or capped) would lift up ten pounds, which, ſays he, exceeded the former weight three hundred and twenty times: that a mariner's needle, being once touched with a vigorous load-ſtone, will afterwards, when freely poiſed, turn it ſelf north and ſouth; and if it be by force made to regard the eaſt and weſt, or any other points of the compaſs, as ſoon as it is left at liberty, it will of itſelf return to its former poſition: that a load-ſtone floating on water will as well come to, and follow a piece of iron, that is kept from advancing towards it, as, when itſelf is fixed, and the iron at liberty, it will draw that metal to it: that without any ſenſible alteration in the agent or the patient, the load-ſtone will in a trice communicate all its virtues to a piece of ſteel, and enable that to communicate them to another piece of the ſame metal: that if a load-ſtone, having been marked at one end, be cut longwiſe according to its axis, and one ſegment be freely ſuſpended over the other, the halves of the marked end, that touched one another before, will not now lie together, but the lower will drive away the upper; and that, which regarded the north in the marked end of the intire load-ſtone, will join with that extreme of the lower half, which in the intire ſtone regarded the ſouth: that (as appears by this laſt named property) there are the ſame magnetical qualities in the ſeparated parts of a magnet, as in the intire ſtone; and if it be cut, or even rudely broken into a great many parts or fragments, every one of theſe portions, though perhaps not ſo big as a corn of wheat, will, if I may ſo ſpeak, ſet up for itſelf, and have its own northen and ſouthern poles, and become a little magnet, *ſui juris*, or independent upon the ſtone, from which it was ſevered, and from all its other parts: that if a loadſtone be ſkilfully made ſpherical, this little magnetick globe, very fitly by our *Gilbert* called a terrella, will not only, being freely placed, turn north and ſouth, and retain that poſition, but have its poles, its meridians, its æquator, &c. upon good grounds deſignable upon it, as they are upon the great globe of the earth. And this will hold, whether the terrella be great or ſmall.

In his lit. de tract. *De Magnetis Proprietatibus.* p. m. 350.

I might not only much encreaſe the number of theſe odd magnetical phænomena, but add others about other ſubjects; but theſe may ſuffice to ſuggeſt to us this reflection, that there is no doubt to be made, but that a man, who never had the opportunity to ſee or hear of magnetical experiments, would look upon theſe as contrary to the principles of nature, and therefore to the dictates of reaſon, as accordingly ſome learned Ariſtotelians, to whom I had occaſion to propoſe ſome of them, rejected them as incredible. And I doubt not, but I could frame as plauſible arguments from the mere axioms of philoſophers, and the doctrine of philoſophick ſchools againſt ſome magnetical phænomena, which experience hath ſatisfied me of, as are wont to be drawn from the ſame topicks againſt the myſterious articles of faith; ſince among the ſtrange properties of the load-ſtone there are ſome, which are not only admirable and ſtupendous, but ſeem repugnant to the dictates of the received philoſophy and the courſe of nature. For whereas natural bodies, how ſubtile ſoever, require ſome particular diſpoſitions in the medium, through which their corpuſcles are to be diffuſed, or their actions tranſmitted; ſo that light itſelf, whether it be a moſt ſubtile body, or a naked quality, is reſiſted by all opacous mediums, and the very effluvia of amber and other electricks will not permeate the thinneſt glaſs, or even a ſheet of fine paper; yet the load-ſtone readily performing his operations through all kind of mediums, without excepting glaſs itſelf.

If the poles of two magnetick needles do both of them regard the north, another philoſopher would conclude them to have a ſympathy, at leaſt to be unlikely to diſagree; and yet, if he bring theſe extremes of the ſame denomination within the reach of one another, one will preſently drive away the other, as if there were a powerful antipathy between them.

A ſomewhat long needle being placed horizontally, and exactly poiſed upon the point of a pin, if you gently touch one end with the pole of a vigorous magnet, that end ſhall manifeſtly dip or ſtoop, though you often take it off the pin, and put it on again. And this inclination of the needle will continue many years, and yet there is not only no other ſenſible change made in the metal by the contact of the load-ſtone; but one end has required a a durable preponderancy, though the other be not lighter, nor the whole needle heavier than before. And the inclination of the magnetick needle may be by another touch of the load-ſtone taken away, without leſſening the weight of the part, that is deprived of it.

The operation, that, in a trice, the load-ſtone has on a mariner's needle, though it makes no ſenſible change in it, or weakens the load-ſtone itſelf, will not be loſt, though you carry it as far as the ſouthern hemiſphere: but it will not be the ſame in all places, but in ſome, the magnetick needle will point directly at the north, in others, it will deviate or decline ſome degrees towards the eaſt or the weſt: and, which ſeems yet more ſtrange, the ſame needle in the ſame place will not always regard the ſame point of the compaſs, but, looked on at diſtant times, may vary from the true meridian, ſometimes to the weſt, and afterwards to the eaſt.

All the communicable virtues of the magnet may be imparted to iron, without any actual contact of the two bodies, but barely by approaching in a convenient way the iron to the loadſtone for a few moments. And the metal may likewiſe be deprived of thoſe virtues in a trice, without any immediate contact by the ſame, or another loadſtone.

If you mark one end of a rod, or other oblong piece of iron, that never came near a magnet, and hold it perpendicularly, you may at pleaſure, and in the hundredth part of a minute, make it become the north or ſouth pole of a magnetical body. For if, when it is held upright, you apply to the bottom of it the north-extreme of an excited and well-poiſed needle, the lower end of the iron will drive away that extreme, which yet will be drawn by the upper end of the ſame iron. And if, by inverting, you make this lower end the uppermoſt, it will not attract, but repel the ſame lilly or north point of the needle, juſt under which it is to be perpendicularly held.

Though *vis unita fortior* be a received rule among naturaliſts; yet oftentimes, if a magnet be cut into pieces, theſe will take up, and ſuſtain much more iron, than the entire ſtone was able to do.

If, of two good loadſtones, the former be much bigger, and on that account ſtronger than the other, the greater will draw a piece of iron, and retain it much more ſtrongly than the leſſer; and yet, when the iron ſticks faſt to the greater and ſtronger loadſtone, the leſſer and weaker may draw the iron from it, and take it quite away.

These phænomena, (to mention now no more,) are ſo repugnant to the common ſentiments of naturaliſts, and the ordinary courſe of things, that, if, antecedently to any teſtimony of experience, theſe magnetical properties had been propoſed to *Ariſtotle* himſelf, he would probably have judged them fictitious things, as repugnant to the laws of nature: nevertheleſs, though it ſeems incredible, that the bare touch of a loadſtone ſhould impart to the mariner's needle a property, which, (as far as we know) nothing in the whole world, that is not magnetical, can communicate or poſſeſs; and ſhould operate (as men ſuppoſe) upon it at three or four thouſand leagues diſtance; yet this is believed by the Peripateticks themſelves upon the teſtimony of thoſe navigators, that have ſailed to the *Eaſt* and *Weſt-Indies*; and divers even of the more rigid of the modern philoſophers believe more than this, upon the teſtimony of *Gilbert*, *Cabæus*, *Kircherus*, and other learned magnetical writers, who have affirmed theſe things; moſt of which I can alſo aver to you upon my own knowledge.

Thus the habitableneſs of the torrid zone, though (as I lately noted) upon probable grounds denied by *Ariſtotle*, and the generality of philoſophers for many ages; yet not only that, but its populouſneſs, is now confidently believed by the Peripatetick ſchoolmen themſelves, who never were there.

And though *Ptolomy*, and ſome other eminent aſtronomers, did with great care and ſkill, and by the help of geometry, as well as obſervations, frame a theory of the planets ſo plauſibly contrived, that moſt of the ſucceeding mathematicians for twelve or fourteen ages acquieſced in it; yet almoſt all the modern philoſophers and aſtronomers, that have ſearched into theſe matters, with a readineſs to believe their eyes, and allow their reaſon to act freely, have been forced, if not to reject the whole theory, yet, at leaſt to alter it quite, as to the number and order of the planets, though theſe laſt named innovations are ſometimes ſolely, and always mainly, built upon the phænomena, diſcovered to us by two or three pieces of glaſs placed in a long hollow cane, and honoured with the name of a teleſcope.

THE laſt of the two things, I invited you to conſider with me, is this, that when we are to judge, which of two diſagreeing opinions is moſt rational, i. e. to be judged moſt agreeable to right reaſon, we ought to give ſentence, not for that, which the faculty, furniſhed only with ſuch and ſuch notions, whether vulgar, or borrowed from this or that ſect of philoſophers, would prefer, but that, which is preferred by the faculty, furniſhed, either with all the evidence requiſite or advantageous to make it give a right judgment in the caſe lying before it, or, when that cannot be had, with the beſt and fulleſt Informations, that it can procure.

THIS is ſo evident by its own light, that your friend might look upon it as an affront to his judgment, if I ſhould go about ſolicitouſly to prove it. And therefore I ſhall only advertiſe you, that, provided the information be ſuch, as a man has juſt cauſe to believe, and perceives, that he clearly underſtands, it will not alter the caſe, whether he have it by reaſon, as that is taken for the faculty furniſhed but with its inbred notions, and the more common obſervations, or by ſome philoſophical theory, or by experiments purpoſely deviſed, or by teſtimony human or divine, which laſt we call revelation. For all theſe are but differing ways of informing the underſtanding, and of ſignifying to it the ſame thing; as the ſight and the touch may aſſure a man, that a body is ſmooth or rough, or in motion or at reſt; (and in ſome other inſtances, ſeveral ſenſes diſcover to us the ſame object, which is therefore called *objectum commune*;) and provided theſe informations have the conditions lately intimated, which way ſoever the underſtanding receives them, it may ſafely reaſon and build opinions upon them.

ASTRONOMERS have within theſe hundred years obſerved, that a ſtar hath appeared among the fixed ones for ſome time, and having afterwards diſappeared, has yet ſome years after that ſhewed itſelf again. And though, as to this ſurprizing phænomena, our experimental philoſophers could have contributed nothing to the producing it, and though it is quite out of all the received ſyſtems of the heavens, that aſtronomers have hitherto delivered; yet the ſtar itſelf may be a true celeſtial light, and may allow us to philoſophize upon it, and draw inferences from the diſcoveries it makes us; as well as we can from the phænomena of thoſe ſtars, that are not extraordinary, and of thoſe falling ſtars, that are within our own ken and region.

THAT the ſupernatural things, ſaid to be performed by witches and evil ſpirits, might, if true, ſupply us with hypotheſes and mediums, whereby to conſtitute and prove theories, as well as the phænomena of mere nature, ſeems tacitly indeed, but yet ſufficiently, to be acknowledged, by thoſe modern naturaliſts, that care not to take any other way to decline the conſequences, that may be drawn from ſuch relations, than ſolicitouſly to ſhew, that the relations themſelves are all, as I fear moſt of them are, falſe, and occaſioned by the credulity or impoſture of men.

BUT not to do any more than glance at theſe matters, let us proceed upon what is more unqueſtionable, and conſider, that, ſince even our moſt critical philoſophers do admit many of the aſtoniſhing attributes of magnetick bodies, which themſelves never had occaſion to ſee, upon the teſtimony of *Gilbert*, and others, who never were able to give the true cauſes of them; becauſe they look upon thoſe relators as honeſt men, and judicious enough not to be impoſed upon as to the matter of fact; ſince, I ſay, ſuch amazing things are believed by ſuch ſevere naturaliſts, upon the authority of men, who did not know the intimate nature of magnetick bodies; and ſince theſe ſtrange phænomena are not only aſſented to, as true, by the philoſophers we ſpeak of, but many philoſophical conſequences are without hæſitancy deduced from them, without any blemiſh to the judgment of thoſe, that give their aſſent both to the things and the inferences; why ſhould it be contrary to reaſon to believe the teſtimony of God, either about his nature, which he can beſt, and he alone can fully know, or about the things, which either he himſelf has done, as the creation of the world, and of man; or which he means to do, as the deſtroying the world, (whether the whole world, or our great vortex only, I diſpute not) and the raiſing both of good and bad men to life again, to receive rewards and puniſhments, according to their demerits. For methinks that apoſtle argues very well, who ſays, “if we receive the teſtimony of men, the teſtimony of God is greater;” eſpecially about ſuch things concerning his own nature, will, and purpoſes, as it is evident, that reaſon, by its own unaſſiſted light, cannot give us the knowledge of.

1 John. v. 9.

So that we Chriſtians, in aſſenting to doctrines upon the account of revelation, need not, nor do not, reject the authority of reaſon, but only appeal from reaſon to itſelf, i. e. from reaſon, as it is more ſlightly, to its dictates, as it is more fully informed. Of which two ſorts of dictates there is nothing more rational, than to prefer the latter to the former.

AND for my part, I am apt to think, that, if what has been repreſented in this ſection, were duly conſidered, this alone would very much contribute to prevent or anſwer moſt of the objections, that make ſuch of the queſtioners of religion, as are not reſolutely vicious, entertain ſuch hard thoughts of ſome articles of the Chriſtian faith, as if they were directly repugnant to reaſon. For, as we were obſerving, that is not to be looked on as the judgment of reaſon, that is pronounced even by a rational man, according to a ſet of notions, though the inferences from theſe would be rational, in caſe there were nothing elſe fit to be taken into conſideration by him, that judges; but that is rather to be looked upon as the judgment of reaſon, which takes in the moſt information procurable, that is pertinent to the things under conſideration.

And therefore men, though otherwiſe learned and witty, ſhew themſelves not equal eſtimators of the caſe of thoſe, that believe the articles we ſpeak of, when they pronounce them to aſſent irrationally, becauſe the things they aſſent to cannot be demonſtrated or maintained by mere natural reaſon, and would probably be rejected by *Democritus*, *Epicurus*, *Ariſtotle*, or any other of the ancient philoſophers, to whom they ſhould be nakedly propoſed, and whoſe judgment ſhould be deſired about them. For, although this allegation would ſignify much, if we pretended to prove what we believe only by arguments drawn from the nature of the thing aſſented to; yet it will not ſignify much in our caſe, wherein we pretend to prove what we believe, chiefly by divine teſtimony, and therefore ought not to be concluded guilty of an irrational aſſent, unleſs it can be ſhewn, either that divine teſtimony is not duly challenged by us for the main of our religion; or that in the particular articles we father ſomething on that teſtimony, which is not contained in it, or rightly deducible from it. And to put us upon the proving our particular articles of faith, ſufficiently delivered in the ſcriptures, and not knowable without revelation, by arguments merely natural, without taking notice of thoſe we can bring for the proof of that revelation, on whoſe account we embrace thoſe articles, is to challenge a man to a duel, upon condition he ſhall make no uſe of his beſt weapons; and is as unreaſonable, as if a ſchoolman ſhould challenge your friend to prove, that the torrid zone is inhabited, againſt the reaſons, that the Ariſtotelians are wont to give to prove it uninhabitable, without allowing him to make uſe of the teſtimony of navigators, who aſſure us of the conſtant breezes, that daily ventilate the air, and qualify that heat, which otherwiſe would not be ſupported, and who furniſh us with thoſe other circumſtances, whereon to build our proofs, which we, that were never there, can have but by relation.

AND indeed, the limitations, that Chriſtian religion puts to ſome of the dictates of philoſophy, which were wont to be admitted in a more general and unreſtrained ſenſe, and the doctrines about God and the ſoul, &c. that it ſuperadds to thoſe, which the light of nature might lead men to about the ſame ſubjects; though to ſome they may ſeem injurious to philoſophy and reaſon, are as little unkind to either, as is the gardener to a crab-ſtock, or ſome ſuch other wild plant, when by cutting off ſome of the branches, and by making a ſlit in the bark, that he may graft on it a pare-main, or ſome other choice apples, by this ſeemingly hard uſage he brings it to bear much nobler fruit, than, if left to its own natural condition, it ever would have done.

I know not, whether to all, that hath been ſaid in this ſection, I may not add thus much further, that it ſometimes happens, that thoſe very things, which at firſt were propoſed to the underſtanding, and believed upon the ſcore of revelation, are afterward aſſented to by it upon the account of mere reaſon. To which purpoſe I conſider, that not any of the ancient philoſophers, nay, as far as I have read, even of thoſe, that believed God to be the author of the world, dreamed, that he created matter of nothing, but only formed the world out of præ-exiſtent matter; whereas Chriſtian divines uſually teach, as an article of faith, that beſides what they call a mediate creation, as when fiſhes were made out of the water, or *Adam*'s body was made out of the earth, there was an immediate production of matter itſelf out of nothing.

SECTION VIII.

AFTER what has been hitherto diſcourſed, it may be ſeaſonable to conſider, what kind of probation, or what degree of evidence may reaſonably be thought ſufficient, to make the Chriſtian religion thought fit to be embraced.

PERHAPS I ſhall not need to tell you, that, beſides the demonſtrations wont to be treated of in vulgar logick, there are among philoſophers three diſtinct, whether kinds or degrees, of demonſtration. For there is a metaphyſical demonſtration, as we may call that, where the concluſion is manifeſtly built on thoſe general metaphyſical axioms, that can never be other than true; ſuch as *nihil poteſt ſimul eſſe & non eſſe; non entis nullæ ſunt proprietates reales, &c.* There are alſo phyſical demonſtrations, where the concluſion is evidently deduced from phyſical principles; ſuch as are *ex nihilo nihil fit: Nulla ſubſtantia in nihilum redigitur, &c.* which are not ſo abſolutely certain as the former, becauſe, if there be a God, he may (at leaſt for aught we know) be able to create and annihilate ſubſtances; and yet are held unqueſtionable by the ancient naturaliſts, who ſtill ſuppoſe them in their theories. And laſtly, there are moral demonſtrations, ſuch as thoſe, where the concluſion is built, either upon ſome one ſuch proof cogent in its kind, or ſome concurrence of probabilities, that it cannot be but allowed, ſuppoſing the truth of the moſt received rules of prudence and principles of practical philoſophy.

AND this third kind of probation, though it come behind the two others in certainty, yet it is the ſureſt guide, which the actions of men, though not their contemplations, have regularly allowed them to follow. And the concluſions of a moral demonſtration are the ſureſt, that men aſpire to, not only in the conduct of private men's affairs, but in the government of ſtates, and even of the greateſt monarchies and empires. And this is conſiderable in moral demonſtrations, that ſuch may conſiſt, and be, as it were, made up of particulars, that are each of them but probable; of which, the laws eſtabliſhed by God himſelf among his own people, as well as the practice of our courts of juſtice here in *England*, afford us a manifeſt inſtance in the caſe of murder, and ſome other criminal cauſes. For, though the teſtimony of a ſingle witneſs ſhall not ſuffice to prove the accuſed party guilty of murder; yet the teſtimony of two witneſſes, though but of equal credit, that is, a ſecond teſtimony added to the firſt, though of itſelf never a wit more credible than the former, ſhall ordinarily ſuffice to prove a man guilty; becauſe it is thought reaſonable to ſuppoſe, that, though each teſtimony ſingle be but probable, yet a concurrence of ſuch probabilities, (which ought in reaſon to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, *i. e.* ſuch a certainty, as may warrant the judge to proceed to the ſentence of death againſt the indicted party.

TO apply theſe things now to the Chriſtian religion: if you conſider, with how much approbation from diſcerning men, that judicious obſervation of *Ariſtotle* has been entertained, where he ſays, that it is as unſkilful and improper a thing, to require mathematical demonſtrations in moral affairs, as to take up with moral arguments in matters mathematical; you will not deny, but that thoſe articles of the Chriſtian religion, that can be proved by a moral, though not by a metaphyſical or phyſical demonſtration, may, without any blemiſh to a man's reaſon, be aſſented to; and that conſequently (by virtue of the foregoing conſiderations) thoſe other articles of the Chriſtian faith, that are clearly and legitimately deducible from the ſo demonſtrated truths, may likewiſe, without diſparagement, be aſſented to.

WE may alſo here conſider further, that the chuſing, or refuſing to embrace the Chriſtian religion, which is not propoſed to us only as a ſyſtem of ſpeculative doctrines, but alſo as a body of laws; according to which, it teaches us, that God commands us to worſhip him, and regulate our lives; the embracing, I ſay, or not embracing this religion, is an act of human choice, and therefore ought to be determined according to the dictates of prudence. Now, though in matters, that very much import us, we may wiſh for and endeavour after ſuch reaſons, whereby to determine our reſolves, as may amount to moral demonſtrations; yet prudence will not always require, that we ſhould refuſe to act upon arguments of a leſs cogency, than moral demonſtrations. For oftentimes, in human affairs, it ſo falls out, that divers hazards, or other inconveniencies, will attend whatever reſolution we take; and in that caſe, all that prudence requires, or can enable us to do, is, to take that reſolution, which upon the whole matter ſeems to be preferable to any other; though that, which is thus preferred, may perhaps be liable to ſome objection, that cannot be directly anſwered, but only obliquely, by the preponderancy of the arguments, that perſuade the choice, againſt which the objection is made.

BUT here perhaps you will tell me, that the ſafeſt way, in caſe of ſuch importance, is to ſuſpend an action, that is every way attended with difficulties, and to forbear either embracing or rejecting the Chriſtian religion, till the truth or falſeneſs of it come to appear evident and unqueſtionable.

TO which I anſwer, that indeed in matters of bare ſpeculation, about which our underſtandings only need to be converſant, the ſuſpenſion of aſſent is not only practicable, but uſually the ſafeſt way; but *Des Cartes* himſelf, who has been the greateſt example and inculcator of his ſuſpenſion, declares, that he would have it practiſed only about human ſpeculations, not about human actions; *ſed hæc interim dubitatio ad ſolam* contemplationem *veritatis reſtringenda; non quantum ad uſum vitæ: quia perſæpe rerum agendarum occaſio præteriret, antequam nos dubiis noſtris exolvere poſſemus. Non raro quod tantum eſt veriſimile cogimur amplecti, vel etiam interdum, etſi è duobus unum altero veriſimilius non appareat, alterutrum tamen eligere.* And in ſome of his other writings he ſpeaks ſo much to ſhew, that it is unreaſonable to expect in matters, where embracing or rejecting a courſe, that requires practice, is neceſſary, ſuch a certainty, as he judges neceſſary to make a true philoſopher acquieſce in reference to propoſitions about ſpeculative matters, that I find by one of his letters, that he was vehemently accuſed for having taught, that men need not have as ſure grounds for chuſing virtuous and avoiding vicious courſes, as for determining about things merely notional.

AND here let me obſerve to you the difference, that I take notice of in the caſes, where we are put upon deliberating, whether we will chuſe or refuſe a thing propoſed. For it may be propounded to us, either as a proffer, on whoſe acceptance an advantage may be hoped, or as a duty, which, beſides the advantage it promiſes to the performance, has a penalty annexed to the non-performance, or as an only expedient to avoid a great miſchief, or obtain a great good.

THUS, when in the *Theatrum Chymicum* ſome of its chief authors, as *Lully*, *Geber*, *Artephius*, who pretend to have been *adepti*, i. e. poſſeſſors of the elixir, very earneſtly exhort their readers to apply themſelves to ſo noble and uſeful a ſtudy as alchymy, (by the help of which, the laſt-named *Artephius* is ſaid to have lived 1000 years,) they make but a propoſition of the firſt ſort. For though a proſperous attempt to make the philoſophers ſtone (ſuppoſing there be ſuch a thing) would poſſeſs a man of an ineſtimable treaſure; yet, if he either refuſe to believe theſe writers, or, if he do believe them, refuſes to take the pains required of him, that would follow their counſel, he can only miſs of the wealth, &c. they would make him hope for, but is really never a whit the poorer, or in a worſe condition, than if they had not endeavoured to engage him.

BUT if an abſolute ſovereign commands ſomething to be done by his ſubjects; and to enforce his command, does not only propoſe great recompenſes to thoſe, that ſhall perform what it preſcribed, but threatens heavy penalties to the diſobedient; this will belong to the ſecond ſort of caſes above mentioned, in which, as it is evident, a man has not the ſame latitude allowed him as in the firſt.

But if we ſuppoſe, that a man by a tranſlation of very peccant matter has got a ſpreading gangrene in his arm, and a ſkilful ſurgeon tell him, that, if he will part with his arm, he may be recovered, and ſave his life, which elſe he will certainly loſe; this caſe will belong to the laſt ſort above-mentioned; the patient's parting with his arm being the only remedy of the gangrene, and expedient to ſave his life, and recover his health. And here alſo it is manifeſt, that there are far ſtronger motives, than thoſe mentioned in the firſt caſe, to make a poſitive and timely reſolution.

To bring this home to our ſubject, I need but mind you, that the Chriſtian doctrine does not only promiſe a heaven to ſincere believers, but threatens no leſs than a hell to the refractory.

The voice of *Moſes* to the *Jews* is this,
Deutr. xi 26,27,28. "Behold, I ſet before you this day a bleſſing "and a curſe; a bleſſing, if ye obey the commandments of the Lord your God, which "I command you this day; and a curſe, if "ye will not obey the commandment of the "Lord your God, but turn aſide out of the "way, which I command you this day."

And the commiſſion, that Chriſt gave his apoſtles, to preach the goſpel, runs thus:
Mark xvi. 15, 16. "Go ye into all the world, and preach the "goſpel to every creature," i. e. to all mankind; "he that believeth, and is baptized, "ſhall be ſaved; but he, that believeth not, "ſhall be damned."

By this you may perceive, that as far as there is either truth or probability in the Chriſtian religion, ſo far forth he, that refuſes to become a diſciple to it, runs a venture, not only to loſe the greateſt bleſſings, that men can hope, but to fall eternally into the greateſt miſeries that they can fear. And indeed our caſe, in reference to the Chriſtian religion may not only be referred to the ſecond ſort of caſes lately mentioned, but to the third ſort too. For as the language of the author of the Chriſtian religion was to his auditors,
John viii. 24. "If ye be-"lieve not, that I am he (the Meſſias) ye ſhall "die in your ſins; ſo of the two greateſt heralds of it, the one tells the Jews, that neither
Acts iv 12. is there ſalvation in any other: for, "there is "no other name under heaven given among "men, whereby we muſt be ſaved:" And the
2 Theſſ. i. 7, 8, 9. other tells the Theſſalonians, that the "Lord "Jeſus ſhall be revealed from heaven with his "mighty angels in flaming fire, taking ven-"geance on them, that know not God, and "obey not the goſpel of our Lord Jeſus "Chriſt; who ſhall be puniſhed with ever-"laſting deſtruction from the preſence of the "Lord, and from the glory of his power."

By all this it appears, that the Chriſtian religion is not propoſed barely as a proffer of heaven in caſe men embrace it, but as a law, that men ſhould embrace it upon the greateſt penalty, and as the only expedient and remedy to attain eternal happineſs, and eſcape endleſs miſery; ſo that the forbearing to ſubmit our necks to the yoke of Chriſt being as well a ruinous courſe, as to reject it, that, which reaſon here puts us upon, is, not ſo much to conſider, whether or no the arguments for the Chriſtian religion be demonſtrations, and will enable a man to anſwer directly all objections and ſcruples; (for there are divers courſes, that prudence may enjoin a man to ſteer, whilſt philoſophy ſuggeſts ſpeculative doubts about the grounds of ſuch reſolutions;) but whether it be more likely to be true, than not to be true, or rather, whether it be not more adviſeable to perform the conditions it requires upon a probable expectation of obtaining the bleſſings it promiſes, than by refuſing it to run a probable hazard of incurring ſuch great and endleſs miſeries, as it peremptorily threatens.

It will perhaps be ſaid, that this is a hard caſe. But that is an allegation I am not here to conſider; ſince it properly belongs to the doctrine about the providence of God, who being the only Author and abſolute Lord of the creatures, who can receive neither laws nor benefits from them, that can oblige him to them; has a right to preſcribe them what laws he thinks fit, that are not impoſſible for them to obey, and to puniſh their diſobedience to ſuch laws; and much more has a right to annex what conditions he pleaſes, to that ineſtimable felicity he holds forth; the proffer of it upon any terms being a free act of his mere goodneſs, and the value of it incomparably ſurpaſſing whatever we men can do or ſuffer to obtain it; eſpecially conſidering, that, as he might enforce his commands, as ſovereigns commonly do, by threatning penalties to the diſobedient, without propoſing rewards to the performers; ſo he has given men ſuch probable arguments to ground their expectations on, that they will be ſelf-condemned, if they reject the religion he propoſes, and yet maintain it to be decent (if I may ſo ſpeak) for him to crown their faith with unvaluable bleſſings. But, as I was ſaying, the direct and full anſwer to this allegation belongs not to this place, where it may ſuffice to ſay, that, whether the caſe be hard or no, yet this is the caſe. And therefore, though the proofs of the Chriſtian religion did not amount (which yet I do not grant) to moral demonſtrations, a man may act rationally in embracing that religion, if, all things conſidered, it appear more likely to be true, than not to be true.

And I ſhall by and by ſhew you, that this is not the only caſe, where prudence puts us upon making reſolutions, notwithſtanding contrary doubts.

I know the harſhneſs of the caſe is by moſt men made to conſiſt in this, that for a religion, whereof the truth ſuppoſed in its promiſes and threats is not demonſtratively proved, we muſt reſign up our pleaſures, and ſometimes undergo conſiderable hardſhips and loſſes, and conſequently we muſt quit what is certain, for what is uncertain. I have in another paper had occaſion to ſay ſomething elſe to this objection, than what (to avoid repetition) ſhall make up my preſent anſwer, which conſiſts of two parts.

The firſt whereof is, that what we are to give up to become Chriſtians, is not really ſo valuable in itſelf, as the objecters think, and that it is of ſcarce any value at all, if compared to the goods we may acquire by parting with them. For alas! what is it, that Chriſtianity requires us to forego, but ſmall petty enjoyments? which thoſe, that have had the moſt of, have found them, and pronounced them unſatisfactory, whilſt they poſſeſſed them, and which manifeſt experience ſhews to be no leſs tranſitory, than they have been declared empty, ſince a thouſand accidents may take them from us, and death will infallibly, after a ſhort time (which can be but a moment compared to eternity) take us from them. And if it be ſaid, that theſe enjoyments, ſuch as they are, are at leaſt the only happineſs, that we can make our ſelves ſure of; I muſt freely profeſs, that I think it therefore the more reaſonable to part with them, if it be neceſſary, upon the hopes, that Chriſtian religion gives us. For (eſpecially if a man behold thoſe things, not only with a philoſophical eye, that can look through them, but with a Chriſtian eye, that can look beyond them,) if there can be no greater happineſs, I do not think ſo poor a thing, as men call happineſs, worth being greedily deſired; and if there be ſuch a tranſcendent happineſs as Chriſtianity holds forth, I am ſure, that deſerves to be the object of my ambition. So that either the meanneſs of worldly happineſs will make me think it no great miſery to want it, or the excellency of heavenly felicity will make me think it great wiſdom to part with earthly for it.

And now, in the ſecond part of my anſwer, I muſt invite you to conſider with me, that Chriſtian religion requires not of us actions more imprudent, than divers others, that are generally looked upon as complying with the dictates of prudence, and ſome of them practiſed, by great politicians themſelves, in the weighty affairs of ſtate.

You know, what a common practice it is, in great ſtorms at ſea, for the merchants themſelves to throw over-board their goods, and, perhaps too, their victuals, (as in *Paul's* caſe) though they be ſure to loſe what they caſt away, and are not certain, either that this loſs will ſave the ſhip, or that the ſhip may not be ſaved without it. The wiſeſt, and even the worldlieſt men, whether princes or private perſons, think themſelves never more ſo, than when they toil, and lay out their care and time, and uſually deny themſelves many things, to provide advantagiouſly for children, which they have but a woman's word for, and conſequently a bare moral probability to aſſure them to be theirs.

In the ſmall pox many phyſicians are for bleeding, and others (as moſt of our Engliſh practitioners) are very much againſt it. Suppoſing then (which is no very rare caſe) that a perſon invaded by that diſeaſe be told by one of his phyſicians, that, unleſs nature be eaſed of part of her burthen by phlebotomy, ſhe will never be able to overcome the diſeaſe; and on the contrary, the other aſſures him, that, if by exhauſting the treaſure of life, (the blood) he further weakens nature, which is but too weak already, the diſeaſe muſt needs overcome her: what can a prudent man do in this caſe, where he can take no reſolution, againſt which probable arguments, that are not directly and fully to be anſwered, may not be oppoſed, and where yet the ſuſpenſion of his reſolution may be as ruinous, as the venturing to take either of thoſe he is invited to?

And in the formerly mentioned caſe, of a man, that has a ſpreading gangrene in his arm, if he conſents, that it be cut off, which prudence often requires that he ſhould do, he is certain to loſe one of his uſefulleſt limbs, and is not certain, but that he may ſave his life without that loſs, nor that he ſhall ſave it by that loſs.

And to give you an inſtance or two of a more publick nature; how many examples does hiſtory afford us of famous generals and other great commanders, who have ventured their forces and their lives to ſeize upon places promiſed to be betrayed to them by thoſe they had corrupted with money; though the ground, upon which they run this hazard, be the engagement of ſome, who, if they were not traytors, that could falſify their faith, would never have been bribed to make ſo criminal and ignominious an engagement? How often have the greateſt politicians either reſolved to enter into a war, or taken courſes, that they foreſee will end in a war, upon the informations they receive from thoſe they have corrupted in other prince's councils; though, to believe ſuch intelligencers, thoſe, who venture ſo much upon their informations, muſt ſuppoſe them faithleſs and perfidious men?

It were not difficult, to add other inſtances to the ſame purpoſe, by which, joined with what has been above diſcourſed, it may appear, that a man need not renounce or lay aſide his reaſon to reſolve to fulfil the conditions of the goſpel, though the arguments for it were none of them demonſtrative ones. For ſo much as a probability of obtaining by it ſuch ineſtimable bleſſings, as it propoſes, and little more than a bare probability of incurring, by rejecting it, ſuch unſpeakable miſeries, as it threatens, may rationally induce a man to reſolve upon fulfilling its reaſonable conditions, and his prudence may very well be juſtified, if it do but appear, that (1.) It is more probable, that ſome religion ſhould be true, than that ſo many well atteſted miracles alledged by the ancient Chriſtians ſhould be falſe; and that God, who is the author of the world, and of men, (for ſo much I think may be phyſically proved) ſhould leave man, whom he has ſo fitted, and by benefits and internal laws obliged to worſhip him, without any expreſs direction how to do it: and that (2.) If there be any true religion, the Chriſtian is the moſt likely to be that, in regard not only of the excellency of its doctrine and promiſes, but of the prophecies and miracles, that bore witneſs to it, the records of which were made by honeſt plain men, who taught and practiſed the ſtricteſt virtue, and who knew their religion condemned lying, freely joined their doctrine and narratives with their blood: the truth of which was ſo manifeſt in the times, when they were ſaid to be done, that the evidence ſeemed abundantly ſufficient to convert whole nations, and among them many conſiderable and prudent perſons, who had great opportunity, as well as concern, to examine the truth of them, and who were by their intereſt and education ſo indiſpoſed to embrace Chriſtianity, that, to make a ſincere profeſſion of it, they muſt neceſſarily relinquiſh both their former religion, and their former vices, and venturouſly expoſe for it not only their fortunes, but their lives.

IF it be here objected, that it is very harſh, if not unreaſonable, to exact, upon ſo great penalty as damnation, ſo firm an aſſent, as is requiſite to faith, to ſuch doctrines, as are either obſcurely delivered, or have not their truth demonſtratively made out: I anſwer, that whatever others may think, I don't believe, that there is any degree of faith abſolutely neceſſary to ſalvation, that is not ſuitable to the evidence, that men may have of it, if they be not wanting to themſelves through lazineſs, prejudices, vice, paſſion, intereſt, or ſome other culpable defect. For conſidering, that God is juſt, and gracious, and has been pleaſed to promulgate the goſpel, that men, whom it ſuppoſes to act as ſuch (that is, as rational creatures) ſhould be brought to ſalvation by it; I ſee no juſt cauſe to think, that he intends to make any thing abſolutely neceſſary to ſalvation, that they may not ſo far clearly underſtand as they are commanded diſtinctly and explicitely to believe it; and what is not ſo delivered, I ſhould, for that very reaſon, unwillingly admit to be neceſſary to ſalvation: and you may here remember, that I formerly told you, I was far from thinking all the tenents either of the ſchools, or of particular churches, to be ſo much as Chriſtian verities, and therefore am very unlike to allow them here to be fundamental and neceſſary ones; and I take it to be almoſt as great as common a miſtake, that all the doctrines, that concern fundamental articles, muſt be fundamental too; as if, becauſe the head is a noble part of the body, and eſſential to life, therefore all the hair, that grows upon it, muſt be thought ſuch too. But then, as to the abſolute firmneſs of aſſent, that is ſuppoſed to be exacted by Chriſtianity to the articles it delivers, I am not ſure, that it is ſo neceſſary in all caſes to true and ſaving faith, as very many take it to be. For firſt, the ſcripture itſelf tells us, that ſome of the truths it reveals, are unfathomable myſteries, and ſome other points are δυσνόητα, hard to be underſtood; and it is unreaſonable to ſuppoſe, that the higheſt firmneſs of aſſent is to be given to ſuch articles, or to thoſe parts of them, as their obſcurity keeps us from having ſo much reaſon to think, that we clearly underſtand them, as we have to ſuppoſe we underſtand thoſe, that are far more plainly revealed. And, ſecondly, to ſpeak more generally, it is harſh to ſay, that the ſame degree of faith is neceſſary to all perſons, ſince men's natural capacities and diſpoſitions, and their education, and the opportunities they have had of being informed, do very much, yet perhaps not culpably, diſpoſe ſome more than others to be diffident, and apt to heſitate, and frame doubts. And the ſame arguments may appear evident enough to one man, to make it his duty to believe firmly what they perſuade, which in another, naturally more ſceptical, or better acquainted with the difficulties and objections urged by the oppoſite party, may leave ſome doubts and ſcruples excuſable enough. And when either the doctrine itſelf is not clearly delivered, or the proofs of it, that a man could yet meet with, are not fully cogent; for that man, not to give ſuch truths the ſame degree of aſſent, that demonſtration may produce, is not, as many interpret it, an affront to the veracity of God, ſince he may be heartily diſpoſed and ready to believe all, that ſhall appear to him to be revealed by God, and only doubts, whether the thing propoſed be indeed revealed by him, or whether the diffident party rightly underſtands the ſenſe of theſe words, wherein the revelation is contained; which is not to diſtruſt God, but himſelf: and that in ſome caſes, a degree of faith, not exempt from doubts, may, through God's goodneſs, be accepted, we may learn from hence, that the apoſtles themſelves, who were ſo much in Chriſt's favour, made it their prayer to him, that he would increaſe their faith: and he, that begged, that if he could do any thing for his ſon, and cried out, "Lord, "I believe, help thou my unbelief," was ſo far accepted by that merciful high prieſt, who is apt to be touched with the ſenſe of our infirmities, that his requeſt was granted, though it could not be ſo but by having a miracle done in his favour. The diſciples diſtreſſed by a ſtorm, and crying to their maſter, as thinking themſelves upon the very point of periſhing, were ſaved by him, at the ſame time, when he gave them the epithet, "of men of "little faith:" and at another time, *Peter* walking upon the ſea, though he had loſt a degree of that faith, that made him firſt engage upon that adventure, and was reproved for it by Chriſt, was yet reſcued from that ſinking condition, which both he and his faith were in. And we are told, in the goſpel, of a faith, which, though no bigger than a grain of muſtard-ſeed, may enable a man to remove mountains: and though this paſſage ſpeaks not primarily of juſtifying faith, yet ſtill it may ſerve to ſhew, that degrees of aſſent, far ſhort of the greateſt, may be ſo far accepted by God, as to be owned by miraculous exertions of his power. For the faith then, that is made a neceſſary condition under the goſpel, as the genuine fruit and ſcope of it is obedience; ſo it is not indiſpenſably ſuch a faith, as excludes doubts, but refuſals. And though the aſſent be not ſo ſtrong, as may be produced by a demonſtration; yet it may be graciouſly accepted, if it be but ſtrong enough to produce obedience. And accordingly, whereas *Paul*, in one place declares, "that in Chriſt Jeſus neither "circumciſion availeth any thing, nor uncir- "cumciſion, but faith operative through "love;" we may learn his meaning from a parallel place, where varying the words, and not the ſenſe, of the latter part of the ſentence, he ſays, "that in Chriſt Jeſus, neither cir- "cumciſion availeth any thing, nor uncircum- "ciſion, but the keeping of the command- "ments of God." I readily grant, that attainment of a higher degree of faith is always a bleſſing, and cannot be ſufficiently prized, without being ſincerely aimed at; but there are in ſome virtues and graces degrees, which though to reach be a great happineſs, yet it is but the endeavouring after them, that is an indiſpenſable duty. Likewiſe it is true, that the firmneſs of aſſent to divine verities does, in ſome regard, bring much honour to God; as it is ſaid of the father of the faithful, (who in reference to the promiſe made him of *Iſaac*, did not conſider his own age, nor *Sarah*'s long barrenneſs, ſo as to entertain any diffidence of what God had told him) that being mighty in faith, he gave glory to God: but it is true too, that in another reſpect a practical aſſent built upon a leſs undoubted evidence may have its preheminence; for when Chriſt now riſen from the dead, had ſaid to the diſtruſtful *Didymus*, "*Thomas*, becauſe thou haſt ſeen "me, thou haſt believed;" he immediately adds, "But bleſſed (that is, peculiarly and "preferably bleſſed) are thoſe, that have not "ſeen, and yet have believed." And indeed he does not a little honour God (in that ſenſe, wherein mortals may be ſaid to honour him) who is ſo willing to obey and ſerve him, and ſo ambitious to be in an eſtate, where he may always do ſo, that upon what he yet diſcerns to be but a probability of the Chriſtian religion's being the moſt acceptable to God, he embraces it with all its difficulties, and dangers, and upon this ſcore venturouſly reſolves to ſubmit, if need be, to a preſent and actual dereliction of all his ſins and luſts, and perhaps his intereſt and his life too, upon a comparatively uncertain expectation of living with him hereafter.

The Concluſion of the FIRST PART.

AND here I will put a period to my anſwer to your friend's queſtion in one of the two ſenſes of it, and ſo to the firſt part of this diſcourſe. Againſt all which perhaps your friend will object, that at this rate of arguing for the Chriſtian religion, one may apologize for any opinion, and reconcile the moſt unreaſonable ones to right reaſon. But it is not difficult for me to reply, that this objection is grounded either upon a miſtake of the deſign of this letter, or upon the overlooking of what is ſuppoſed in it. For I do not pretend, that the conſiderations hitherto alledged ſhould paſs for demonſtrations of the truth of Chriſtianity, which is to be proved by the excellency of the doctrines it teaches, and that of the rewards it promiſes, (both which are worthy of God,) and by divers other arguments, eſpecially the divine miracles, that atteſt it: but that, which I was here to do, was, not to lay down the grounds, why I received the Chriſtian religion, but to return an anſwer, backed with reaſons, to the queſtion, that was propoſed: "whether I did not "think, that a Chriſtian, to continue ſuch, "muſt deny or lay aſide his reaſon?" The ſum of the anſwer is this, that the doctrines really propoſed by the Chriſtian religion, ſeeming to me to be by proper arguments ſufficiently proved in their kind, ſo as that the proofs of it, whether they be demonſtrative or no, are ſufficient, (the nature of the things to be proved, conſidered) to juſtify a rational and prudent man's embracing it; this religion, I ſay, ſeeming to me to have ſuch poſitive proofs for it, I do not think, that the objections, that are ſaid to be drawn from reaſon againſt it, do really prove the belief of it to be inconſiſtent with right reaſon, and do outweigh the arguments alledgable in that religion's behalf. To propoſe ſome of the general grounds of this anſwer of mine, was the deſign of the conſiderations hitherto diſcourſed of; which (as I hinted to you at the beginning) could be no other than general, unleſs you had mentioned to me ſome of your friend's particular objections, which when he tells you, you will perhaps find, that I have already given you the grounds of anſwering them. And though to propoſe arguments to evince poſitively the truth of our religion, after the example of the excellent *Grotius*, and ſome other very learned writers, be not, as you ſee, either my taſk or my deſign; yet if you attentively conſider, what I write in that ſhort diſcourſe, wherein I manage but that ſeemingly popular argument for Chriſtianity, that is drawn from the miracles, that are ſaid to atteſt it, you will perchance be invited to think, that when all the other proofs of it are taken in, a man may, without renouncing or affronting his reaſon, be a Chriſtian.

BUT to proceed to the more conſiderable part of what I preſumed your friend will object, I anſwer, that the conſiderations I have alledged in the behalf of ſome myſteries of the Chriſtian religion, will not be equally applicable to the moſt abſurd or unreaſonable opinions. For theſe conſideratians are offered as apologies for Chriſtian doctrines, but upon two or all of theſe three ſuppoſitions. The firſt, that the truth of the main religion, of which ſuch doctrines make a part, is ſo far poſitively proved by real and uncontrouled miracles, and other competent arguments, that nothing, but the manifeſt and irreconcileable repugnancy of it's doctrines to right reaſon, ought to hinder us from believing them. The ſecond, that divers of the things, at which reaſonable men are wont to take exception, are ſuch, as reaſon itſelf may diſcern to be very difficult, or perhaps impoſſible for us to underſtand perfectly by our own natural light. And the third, that ſome things in Chriſtianity, which many men think contrary to reaſon, are, at moſt, but contrary to it, as it is incompetently informed and aſſiſted, but not when it is more fully inſtructed, and particularly when it is enlightened and aſſiſted by divine revelation. And as I think theſe three ſuppoſitions are not juſtly applicable, (I ſay not, as the objection does, to the moſt abſurd, or unreaſonable opinions, but) to any other religion than the true, which is the Chriſtian; ſo the laſt of theſe ſuppoſitions prompts me to take notice to you, that, though we ought to be exceeding wary, how we admit what pretends to be ſupernaturally revealed; yet if it be attended with ſufficient evidence of its being ſo, we do very much wrong and prejudice our ſelves, if, out of an unreaſonable jealouſy, or to acquire or maintain the repute of being wiſer than others, we ſhut our eyes againſt the light it offers. For beſides that a man may as well err, by rejecting, or ignoring the truth, as by miſtaking a falſhood for it; I conſider, that thoſe men, that have an inſtrument of knowledge, which other men either have not, or, (which is as bad) refuſe to employ, have a very great advantage above others towards the acquiring of truth, and with far leſs parts than they, may diſcover divers things, which the others, with all their pride and induſtry, ſhall never attain to. As when *Galileo* alone among the modern aſtronomers was maſter of a teleſcope, it was eaſy for him to make noble diſcoveries in heaven of things, to which not only *Ptolomy*, *Alphonſus*, and *Tycho*, but even his maſters, *Ariſtarchus Samius* and *Copernicus* themſelves, never dreamed of, and which other aſtronomers cannot ſee but by making uſe of the ſame kind of inſtrument. And on this occaſion let me carry the compariſon, ſuggeſted by the teleſcope, a little further, and take notice, that if men having heard, that there were four planets moving about *Jupiter*, and that *Venus* is an opacous body, and ſometimes horned like the moon, had reſolved to examine theſe things by their naked eyes, as by the proper organs of ſight, without employing the teleſcope, by which they might ſuſpect, that *Galileo* might put ſome optical deluſion upon them; they would perhaps have aſſembled in great multitudes to gaze at *Venus* and *Jupiter*, that (ſince *plus vident oculi quam oculus*) the number of eyes might make amends for their dimneſs. This attempt not ſucceeding, they would perhaps chooſe out ſome of the youngeſt and ſharpeſt ſighted men, that by their piercing eyes that may be diſcovered, which ordinary ones could not reach. And this expedient not ſucceeding neither, they would perhaps diet their ſtar-gazers, and preſcribe them the inward uſe of fennel, and eye-bright, and externally apply collyriums and eye-waters, and thoſe to as little purpoſe as the reſt. With ſuch a pity, mixed with indignation, as *Galileo* would probably have looked on ſuch vain and fruitleſs attempts with, may a judicious Chriſtian, that upon a due examination admits the truth of the ſcriptures, look upon the preſumptuous and vain endeavours of thoſe men, who, by the goodneſs of their natural parts, or by the improvements of them, or by the number of thoſe, that conſpire in the ſame ſearch, think, with the bare eye of reaſon to make as great diſcoveries of heavenly truths, as a perſon aſſiſted by the revelations, contained in the ſcripture, can with great eaſe and ſatisfactorineſs attain. To which let me add this further improvement of the compariſon, that as a ſkilful aſtronomer will indeed, firſt ſeverely examine, whether the teleſcope be an inſtrument fit to be truſted and not likely to impoſe upon him; but being once reſolved of that, will confidently believe the diſcoveries it makes him, however contrary to the received theories of the celeſtial bodies, and to what he himſelf believed before, and would ſtill, if the teleſcope did not otherwiſe inform him, continue to believe; ſo a well qualified inquirer into religions, though he will be very wary, upon what terms he admits ſcripture, yet if he once be fully ſatisfied, that he ought to admit it, he will not ſcruple to receive upon its authority, whatever ſupernatural truths it clearly diſcloſes to him; though perhaps, contrary to the opinions he formerly held, and which, if the ſcripture did not teach him otherwiſe, he would yet aſſent to. And as the galaxy, and other whitiſh parts of the ſky, were by *Ariſtotle* and his followers, and many other philoſophers, who looked on them only with their naked eyes, for many ages reputed to be but meteors; but to thoſe, that look on them with an eye aſſiſted by the teleſcope, they plainly appear true conſtellations made up of a multitude of bright, though little, ſtars; ſo there are theological doctrines, which to philoſophers, and others, that look on them with the naked eye of natural reaſon, ſeem to be but light and fantaſtical things; which yet, when reaſon, aſſiſted and heightened by revelation, comes to contemplate, it manifeſtly ſees them to be true and celeſtial lights, which only their ſublimity keeps concealed from our weak, (naked) eyes.

SOME PHYSICO-THEOLOGICAL CONSIDERATIONS ABOUT THE POSSIBILITY OF THE RESURRECTION.

The PREFACE.

WHILST the Conſiderations about Religion and Reaſon, (to which the following eſſay is annexed) were not yet come from the preſs, the learned publiſher of them falling one day into diſcourſe with me about the deſign they aimed at, and ſome of the points they treated of, and particularly the reſurrection; our diſcourſe occaſioned my letting him know, that I had long ſince had thoughts, and perhaps imparted ſome of them to my friends, about ſuch ſubjects; and that in particular about the reſurrection I had yet by me a manuſcript, wherein divers years ago I had endeavoured to ſhew, that the philoſophical difficulties, urged againſt the poſſibility of the reſurrection, were nothing near ſo inſuperable, as they are by ſome pretended, and by others granted to be. Upon this notice, the curioſity he expreſſed to ſee this eſſay, engaged me quickly to bring it him; though my being ready to go from *London* made me do it without ſtaying to look it over my ſelf; much leſs, to add what ſince occurred to me about the things treated of in it. But notwithſtanding its imperfections, and my unwillingneſs to let it go abroad; eſpecially without ſome papers, that ſhould have preceded it, the learned peruſer would not be denied leave to ſend it, in my abſence, unaltered to the preſs, and join it to the tract he expected thence; poſitively affirming, that I ought no longer to ſtifle a diſcourſe, that he judged very ſeaſonable, and thought likely to do good. In which conjecture, if he do not prove miſtaken, I hope ſome more ingenious than religious men, ſeeing what can be eaſily ſaid by ſo incompetent a pen as mine, for one of the moſt oppoſed doctrines of Chriſtianity, will hereby be made leſs forward to condemn all thoſe for deſerters of reaſon, that ſubmit to revelation. And I ſhall hope too, on the other ſide, that ſome more religious, than, in this matter, well-informed men, will be induced to think, that what they call the new philoſophy may furniſh us with ſome new weapons for the defence of our ancienteſt creed; and that corpuſcularian principles may not only be admitted without Epicurean errors, but be employed againſt them.

SOME
Phyſico-Theological CONSIDERATIONS, &c.

THE queſtion about which my thoughts are deſired being this; "whether to "believe the reſurrection of the dead, "which the Chriſtian religion teaches, be not "to believe an impoſſibility? I ſhall, before I proceed any farther, crave leave to ſtate the queſtion ſomewhat more clearly and diſtinctly; that, being freed from ambiguities, you may the better know in what ſenſe I underſtand it in my anſwer; in the returning whereof, your friend need not deſire me to inſiſt but upon my own thoughts, unleſs he could do me the favour to direct me to ſome author, which I have not yet ſeen, that has expreſly treated, upon philoſophical grounds, of the queſtion he propoſes.

First then I take it for granted, that he does not mean, whether the reſurrection is a thing knowable, or directly provable by the mere light of nature. For, if God had not, in the ſcripture, poſitively revealed his purpoſe of raiſing the dead, I confeſs, I ſhould not have thought of any ſuch thing; neither do I know, how to prove that it will be, but by flying, not only to the veracity, but the power of God; who having declared, that he will raiſe the dead, and being an almighty agent, I have reaſon to believe, that he will not fail to perform what he has foretold.

Nor do I (ſecondly) underſtand the queſtion to be, whether the reſurrection be poſſible to be effected by merely phyſical agents and means. For that it is not to be brought to paſs according to the common courſe of nature, I preſume, after the univerſal experience of many ages, which have afforded us no inſtances of it. And though perhaps in ſpeculation it ſeems not abſolutely repugnant to reaſon, that the ſcattered parts of a dead body might be reconjoined, ſoon after the death of the man; yet I think you will eaſily grant it to be morally impoſſible, that this ſhould happen to any one perſon, and much more, that it may, nay, that it will happen to all the perſons of mankind at the world's end: ſo that when I treat of the poſſibility of the general reſurrection, I take it for granted, that God has been pleaſed to promiſe and declare, that there ſhall be one, and that it ſhall be effected, not by, or according to the ordinary courſe of nature, but by his own power. On which occaſion, I remember, that when our Saviour, treating of the reſurrection, ſilenced the Sadducees, that denied it, he conjoins, as the cauſes of their error, the two things I have pointed at in this obſervation, and in the firſt, that preceeded it: "You err, ſays he, "not knowing the ſcriptures, nor the power of "God." And when an angel would aſſure the bleſſed virgin, that ſhe ſhould bear a child without the intervention of a man, (which was a caſe ſomewhat akin to ours, ſince it was a production of a human body out of a ſmall portion of human ſubſtance in a ſupernatural way,) he concludes his ſpeech by telling her, "That nothing ſhall prove impoſſible to "God."

In the third place, I ſuppoſe, that the article of the reſurrection, taught by the Cbriſtian religion, is not here, meant by the propoſer in ſuch a latitude, as to comprize all, that any particular church or ſect of Chriſtians, much leſs any private doctor or other writer, hath taught about the reſurrection; but only what is plainly taught about it in the holy ſcriptures themſelves. And therefore if beſides what is there ſo delivered, the propoſer hath met with any thing, that he judges to be impoſſible in it's own nature, he hath my free conſent to deal with the authors and abettors of ſuch unreaſonable opinions, (which I declare my ſelf to be not only unconcerned to defend, but ſufficiently diſpoſed to reject,) as raſhneſſes unfriendly to the growth of Chriſtianity.

4. And now, that I may yet further clear the way for the diſcourſe, that is to follow, and obviate ſome objections and ſcruples, which I think it is better ſeaſonably to prevent, than ſolemnly to anſwer; I ſhall deſire your leave to lay down in this place a couple of conſiderations; of which I ſhall begin with this, that it is no ſuch eaſy way, as at firſt it ſeems, to determine, what is abſolutely neceſſary and but ſufficient to make a portion of matter, conſidered at differing times or places, to be fit to be reputed the ſame body.

5. That the generality of men do in vulgar ſpeech allow themſelves a great latitude about this affair, will be eaſily granted by him, that obſerves the received forms of ſpeaking. Thus *Rome* is ſaid to be the ſame city, though it hath been ſo often taken and ruined by the barbarians and others, that perhaps ſcarce any of the firſt houſes have been left ſtanding, and at leaſt very few remain in compariſon of thoſe, that have been demoliſhed, and have had others built in their ſtead. Thus an univerſity is ſaid to be the ſame, though ſome colleges fall to ruin, and new ones are built; and though once in an age all the perſons, that compoſed it, deceaſe, and are ſucceeded by others. Thus the *Thames* is ſaid to be the ſame river, that it was in the time of our forefathers, though indeed the water, that now runs under *London* Bridge, is not the ſame, that ran there an hour ago, and is quite other than that, which will run there an hour hence. And ſo the flame of a candle is ſaid to be the ſame for many hours together, though it indeed be every minute a new body, and the kindled particles, that compoſe it at any time aſſigned, are continually putting off the form of flame, and are repaired by a ſucceſſion of like ones.

NOR is it by the vulgar *only*, that the notion of identity has been uneaſy to be penetrated. For it ſeems, that even the ancient philoſophers have been puzzled about it; witneſs their diſputes, whether the ſhip of *Theſeus* were the ſame after it had (like that of Sir *Francis Drake*) been ſo patched up from time to time to preſerve it as a monument, that ſcarce any plank remained of the former ſhip, new timber having been ſubſtituted in the place of any part, that in length of time rotted. And even in metaphyſicks themſelves, I think it no eaſy taſk to eſtabliſh a true and adequate notion of identity, and clearly determine, what is the true principle of individuation. And at all this I do not much wonder; for almoſt every man, that thinks, conceives in his mind this or that quality or relation, or aggregate of qualites, to be that, which is eſſential to ſuch a body, and proper to give it ſuch a denomination; whereby it comes to paſs, that, as one man chiefly reſpects this thing, and another that in a body, that bears ſuch a name; ſo one man may eaſily look upon a body as the ſame, becauſe it retains what he chiefly conſidered in it, whilſt another thinks it to be changed into a new body, becauſe it has loſt that, which he thought was the denominating quality or attribute. Thus philoſophers and phyſicians diſagree about water and ice, ſome taking the latter to be but the former diſguiſed, becauſe they are both of them cold and ſimple bodies, and the latter eaſily reducible to the former, by being freed from the exceſſive and adventitious degree of coldneſs; whilſt others, looking upon fluidity as eſſential to water, think ice upon the ſcore of its ſolidity to be a diſtinct ſpecies of bodies. And ſo Peripateticks and chemiſts often diſagree about the aſhes and calces of burnt bodies; the firſt referring them to earth, becauſe of their permanency and fixedneſs, and divers of the Spagyriſts, taking them to be bodies *ſui generis*, becauſe common aſhes uſually contain a cauſtick ſalt, whereas earth ought to be inſipid: and the like may be ſaid of ſome wood-aſhes and lime-ſtone, and even coral, which, when well calcined and recent, have a biting taſte, beſides that ſome of them, that are inſipid, may be reduced into metals, as may be eaſily enough tried in the calces of lead and copper.

THESE difficulties about the notion of identity I have therefore taken notice of, that we may not think it ſtrange, that among the ancient Hebrews and Greeks, whoſe languages were ſo remote in ſeveral regards from ours, the familiar expreſſions employed about the ſameneſs of a body ſhould not be ſo preciſe as were requiſite for their turn, who maintain the reſurrection in the moſt rigid ſenſe. And this leads me from the firſt of my two conſiderations to the ſecond.

THAT, then, it is not repugnant or unconſonant to the holy ſcripture, to ſuppoſe, that a comparatively ſmall quantity of the matter of a body, being encreaſed either by aſſimilation or other convenient appoſition of aptly diſpoſed matter, may bear the name of the former body, I think I may reaſonably gather from the three following expreſſions, I meet with in the Old and New Teſtament.

FOR firſt, St. *Paul* in the 15th chapter of his firſt epiſtle to the *Corinthians*, where he profeſſedly treats of the reſurrection, and anſwers this queſtion; "But ſome men will ſay, "how are the dead raiſed up? and with what bo-"dy do they come?" ver 35: he more than once explains the matter by the ſimilitude of ſowing, and tells them, ver 37. "That which "thou ſoweſt, thou ſoweſt not that body, that "ſhall be: but bare grain, it may chance of "wheat, or of ſome other grain. Adding, that "God gives this ſeed a body as he thought fit, "to each ſeed its own body, ver. 38." Now, if we conſider the multitude of grains of corn, that may in a good ſoil grow out of one; inſomuch, that our Saviour ſpeaking, in the parable *de Agro Dominico*, of a whole field, tells us, that the grain may well bear a hundred for one; we cannot but think, that the portion of the matter of the ſeed, that is in each of the grains (not to reckon what may be contained in the roots, ſtalk, and chaff,) muſt be very ſmall.

I will not now conſider, whether this text juſtifies the ſuppoſition of a plaſtick power in ſome part of the matter of a deceaſed body; whereby, being divinely excited, it may be enabled to take to its ſelf freſh matter, and ſo ſubdue and faſhion it, as thence ſufficiently to repair or augment itſelf; though the compariſon ſeveral times employed by St. *Paul* ſeems to favour ſuch an hypotheſis. Nor will I examine, what may be argued from conſidering, that leaven, though at firſt not differing from other dough, is by a light change of qualities, that it acquires by time, enabled to work upon and ferment a great proportion of other dough. Nor yet will I here debate, what may be ſaid in favour of this conjecture from thoſe chemical experiments, by which *Kircherus*, a *Polonian* phyſician in *Quercetanus* and others, are affirmed to have, by a gentle heat, been able to re-produce, in well-cloſed vials, the perfect ideas of plants deſtroyed by the fire: I will not, I ſay, in this place enter upon a diſquiſition of any of theſe things, both becauſe I want time to go thorough with it, and becauſe, the reſuſcitation, ſuppoſing the matter of fact, may give no ſmall countenance to our cauſe; yet I do not either abſolutely need it, or perhaps fully acquieſce in all the circumſtances and inferences, that ſeem to belong to it. But one thing there is, that I muſt not leave unmentioned in this place; becauſe I received it, ſoon after the trial was made, from two eminent perſons of my acquaintance, men of great veracity, as well as judgment; whereof one made the experiment, and the other ſaw it made in his own garden, where the trier of the experiment (for he was ſo modeſt, that he would not confeſs himſelf to be the author of it,) took ſome aſhes of a plant, juſt like our Engliſh red poppy; and having ſowed theſe alkaliſate aſhes in my friend's garden, they did, ſooner than was expected, produce certain plants larger and fairer than any of that kind, that had been ſeen in thoſe parts. Which ſeems to argue, that, in the ſaline and earthy, *i. e.* the fixed particles of a vegetable, that has been diſſipated and deſtroyed by the violence of the fire, there may remain a plaſtick power, inabling them to contrive diſpoſed matter, ſo as to re-produce ſuch a body, as was formerly deſtroyed. But to this plaſtick power, reſiding in any portion of the deſtroyed body itſelf, it will not perhaps be neceſſary to have recourſe; ſince an external and omnipotent agent can, without it, perform all that I need contend for: as I think I might gather from that other expreſſion of holy ſcripture, that I meet with in the ſecond chapter of *Geneſis*, where it is ſaid, “That the Lord God cauſed a deep ſleep to fall upon *Adam*, and he ſlept; and he took one of his ribs, and cloſed up the fleſh inſtead thereof. And the rib, which the Lord God had taken from man, made he a woman, and brought her unto the man, ver. 21, 22.” For, ſince it cannot be pretended, that either the whole, or any conſiderable portion of *Eve*'s body was taken out of *Adam's*, which was deprived but of a rib; and ſince it cannot be probably affirmed, that this rib had any ſpermatick faculty, both becauſe the text aſſigns the formation of the woman to God, and becauſe the ſeminal principles in animals require the commixture of male and female, the latter of which the text ſuppoſes not to have been then made; why may I not conclude, that, if it pleaſe God, by his immediate operation, to take a portion of the matter of a human body, and add to it a far greater quantity, either of newly created, or of pre-exiſtent matter; the new body ſo framed may, congruouſly enough to ſcripture-expreſſions, be reputed to be made of the former body. And accordingly *Adam* (ver. 23.) gives the reaſon why, he called his wife *Iſha*, which our tranſlation renders woman; becauſe ſhe was taken out of *Iſh*, which in our verſion is rendered man.

The other text, that I conſider, to my preſent purpoſe, is the myſtical reſurrection deſcribed in *Ezekiel*'s viſion, where all, that remain of the dead men, that were to riſe up an army of living men, was a valley full of dry bones, which being by the divine Power approached to one another, and made to join together in a convenient manner, were afterwards by the ſupernatural appoſition of either newly created, or extrinſecally ſupplied matter, Ezekiel xxxvii. furniſhed with ſinews, (by which I ſuppoſe is meant not only nerves, but veſſels, tendons, Ver. 7, 8. ligaments, &c.) and fleſh covered with ſkins; and laſt of all, a vivifying ſpirit was conveyed Ver. 9, 10. into them, that made them ſtand upon their feet alive, an exceeding great army. Whence I gather, that it is not unconſonant to the expreſſions of ſcripture, to ſay, that a portion of the matter of a dead body, being united with a far greater portion of matter furniſhed from without by God himſelf, and completed into a human body, may be reputed the ſame man, that was dead before. Which may appear, both by the tenor of the viſion, and particularly from the expreſſion ſet down in the tenth verſe, where God, calling for the enlivening ſpirit, names the compleated, but not yet revived bodies, *theſe ſlain*, as if he now counted them the ſame, that had formerly been killed.

These preliminary conſiderations being thus laid down, we may now proceed to examine more cloſely thoſe difficulties, which are ſaid to demonſtrate the impoſſibility of the reſurrection; the ſubſtance of which difficulties may be compriſed in this objection.

When a man is once really dead, divers of the parts of the body will, according to the courſe of nature, reſolve themſelves into multitudes of ſteams, that wander to and fro in the air; and the remaining parts, that are either liquid or ſoft, undergo ſo great a corruption and change, that it is not poſſible ſo many ſcattered parts ſhould be again brought together, and re-united after the ſame manner wherein they exiſted in a human body, whilſt it was yet alive. And much more impoſſible it is to effect this re-union, if the body have been, as it often happens, devoured by wild beaſts or fiſhes; ſince in this caſe, though the ſcattered particles of the cadaver might be recovered as particles of matter, yet having already paſſed into the ſubſtance of other animals, they are quite tranſmuted, as being informed by the new form of the beaſt or fiſh that devoured them, and of which they now make a ſubſtantial part.

And yet far more impoſſible will this redintegration be, if we put the caſe, that the dead body be devoured by Canibals; for then the ſame fleſh belonging ſucceſſively to two differing perſons, it is impoſſible that both ſhould have it reſtored to them at once, or that any footſteps ſhould remain of the relation it had to the firſt poſſeſſor.

In anſwer to this (indeed weighty) objection, I have ſeveral things to offer.

And firſt, I conſider, that a human body is not as a ſtatue of braſs or marble, that may continue, as to ſenſe, whole ages in a permanent ſtate; but is in a perpetual flux, or changing condition, ſince it grows in all its parts, and all its dimenſions, from a corpuſculum, no bigger than an inſect, to the full ſtature of man; which in many perſons, that are tall and fat, may amount to a vaſt bulk, which could not happen but by a conſtant appoſition and aſſimilation of new parts to the primitive ones of the little embryo; and ſince men, as other animals, grow but to a certain pitch, and till a certain age, (unleſs perhaps it be the crocodile, which ſome affirm to grow always till death,) and therefore muſt diſcharge a great part of what they eat and drink by inſenſible tranſpiration, which *Sanctorius*'s Statical Experiments, as well as mine, aſſure me to be ſcarce credibly great, as to men and ſome other animals, both hot and cold; it will follow, that, in no very great compaſs of time, a great part of the ſubſtance of a human body muſt be changed: and yet it is conſiderable, that the bones are of a ſtable and laſting texture, as I found not only by ſome chemical trials, but by the ſculls and bones of men, whom hiſtory records to have been killed an exceeding long time ago, of which note we may hereafter make uſe.

SECONDLY, I conſider, that there is no determinate bulk or ſize, that is neceſſary to make a human body paſs for the ſame, and that a very ſmall portion of matter will ſometimes ſerve the turn; as an embryo, for inſtance, in the womb, a new-born babe, a man at his full ſtature, and a decrepit man of perhaps an hundred years old, notwithſtanding the vaſt difference of their ſizes, are ſtill reputed to be the ſame perſon; as is evident, by the cuſtom of crowning kings and emperors in the mother's belly, and by putting murderers, &c. to death in their old age, for crimes committed in their youth; and if a very tall and unweildy fat man ſhould, as it ſometimes happens, be reduced by a conſumption to a ſkeleton, as they ſpeak, yet none would deny, that this waſted man were the ſame with him, that had once ſo enormouſly big a body.

I conſider alſo, that a body may either conſiſt of, or abound with ſuch corpuſcles, as may be variouſly aſſociated with thoſe of other bodies, and exceedingly diſguiſed with thoſe mixtures, and yet retain their own nature; of this we have divers inſtances in metalline bodies: thus gold, for example, when diſſolved in aqua regis, paſſes for a liquor, and when dexterouſly coagulated, it appears a ſalt or vitriol: by another operation, I have taken pleaſure to make it part of the fuel of a flame: being dexterouſly conjoined to another mineral, it may be reduced to glaſs: being well precipitated with mercury, it makes a glorious tranſparent powder: being precipitated with ſpirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very eaſily, yet is far ſtronger than gun-powder: being precipitated with a certain other alkali, the fire turns it to a fixed and purple calx. And yet in ſpite of all theſe, and divers other diſguiſes, the gold retains its nature; as may be evinced by chemical operations, eſpecially by reductions. Mercury alſo is a greater Proteus than gold, ſometimes putting on the form of a vapour; ſometimes appearing in that of an almoſt inſipid water; ſometimes aſſuming, in that condition, the form of a red powder; ſometimes that of a white one, and of a yellow one, or of a chryſtalline ſalt, of a malleable metal; of what not? And yet all theſe are various dreſſes of the ſame quickſilver, which a ſkilful artiſt may eaſily make it put off, and re-appear in its native ſhape.

AND though it be true, that inſtances of the permanence of corpuſcles, that paſs under ſucceſſive diſguiſes, may be much eaſier found among metals and minerals, than vegetables and animals; yet there are ſome to be met with among theſe: for, not to mention *Hippocrates* his affirmation about purging a child with the milk of an animal, that had taken *Elaterium*, (if I miſ-remember not the drug,) not to mention this, I ſay; I remember, that when I once paſſed a ſpring in *Savoy*, I obſerved, that all the butter, that was made in ſome places, taſted ſo rank of a certain weed, that at that time of the year abounds there in the fields, that it made ſtrangers much nauſeate the butter, which otherwiſe was very good. If it be conſidered, how many, if I may ſo call them, elaborate alterations the rank corpuſcles of this weed muſt have undergone in the various digeſtions of the cow's ſtomach, heart, breaſts, &c. and that afterwards, two ſeparations, at leaſt, were ſuperadded, the one of the cream from the reſt of the milk, and the other of the unctuous parts of the cream from the ſerum or butter-milk; it will ſcarce be denied, but that vegetable corpuſcles may, by aſſociation, paſs through divers diſguiſes, without loſing their nature; eſpecially conſidering, that the eſſential attributes of ſuch corpuſcles may remain undeſtroyed, though no ſenſible quality ſurvive to make proof of it; as in our newly mentioned example the offenſive taſte did. And beſides what we commonly obſerve on the ſea-coaſt, of the fiſhy taſte of thoſe ſea-birds, that feed only upon ſea-fiſh, I have purpoſely enquired of an obſerving man, that lived upon a part of the Iriſh coaſt, where the cuſtom is to fatten their hogs with a ſhell-fiſh, which that place very much abounds with, about the taſte of their pork: to which he anſwered me, that the fleſh had ſo ſtrong and rank a taſte of the fiſh, that ſtrangers could not endure to eat it. There is a certain fruit in *America*, very well known to our Engliſh planters, which many of them call the prickle-pear, whoſe very red juice being eaten with the pulp of the fruit, whereof it is a part, doth ſo well make its way through the divers ſtrainers and digeſtions of the body, that it makes the urine red enough to perſuade thoſe, that are unacquainted with this property, that they piſs blood; as I have been ſeveral times aſſured by unſuſpected eye-witneſſes. But more odd is that, which is related by a learned man, that ſpent ſeveral years upon the Dutch and Engliſh plantations in the *Charibbe-Iſlands*, who ſpeaking of a fruit, (which I remember I have ſeen, but had not the liberty to make trial of it,) called janipa, or junipa, growing in ſeveral of thoſe iſlands, he tells us, among other things, that *au temps*, &c. which is, at the ſeaſon, when this fruit falls from the tree, the hogs, that feed on it, have both their fleſh and fat of a violent colour, as experience witneſſeth, (which colour is the ſame, that the juice dies;) and the like happens to the fleſh of parrots and other birds, that feed upon it. I ſhall by and by give you an inſtance of a vegetable ſubſtance, which, though torn in pieces by very corroſive liquors, and ſo diſguiſed as to leave no ſuſpicion of what it was, does thereby not only loſe its nature, but is in an immediate capacity of re-appearing cloathed even with the ſenſible qualities of it, as colour, taſte, and ſmell.

HAVING thus ſhewn, that the particles of a body may retain their nature under various diſguiſes, I now proceed to add, that they may be ſtripped of thoſe diſguiſes, or, to ſpeak without a metaphor, be extricated from thoſe compoſitions, wherein they are diſguiſed, and that ſometimes by ſuch ways, as thoſe, that are ſtrangers to the nicer operations of nature, would never have thought upon, nor will not perhaps judge probable, when propoſed. It is not unknown to expert chemiſts, that, in deſpite of all the various ſhapes, which that Proteus, mercury, may be made to appear in, as of a criſtalline ſublimate, a red precipitate, a yellow turbith, a vapor, a clear water, a cinaber, &c. a ſkilful method of reduction will quickly free it from all, that made it impoſe upon our ſenſes, and re-appear in the form of plain running mercury. And though vitrification be looked upon by chemiſts, as the ultimate action of the fire, and powerfuleſt way of making inſeparable conjunctions of bodies; yet even out of glaſs of lead, for inſtance, (made of ſand, and the aſhes of a metal,) though the tranſmutation ſeems ſo great, that the dark and flexible metal is turned into a very traſparent and brittle maſs; yet even from this have we recovered opacous and malleable lead. And though there be ſeveral ways, beſides precipitations, of divorcing ſubſtances, that ſeem very ſtrictly, if not unſeparably united; (which though I may, perhaps, have practiſed, it is not now convenient I ſhould diſcourſe of;) yet, by precipitation alone, if a man have the ſkill to chooſe proper precipitants, ſeveral ſeparations may not only be made, but be eaſily and thoroughly made, that every one would not think of: for, it is not neceſſary, that in all precipitations, as is obſerved in moſt of the vulgar ones, the precipitant body ſhould indeed make a ſeparation of the diſſolved body from the maſs, or bulk of that liquor, or other adjunct, whereto it was before united, but ſhould not be able to perform this without aſſociating its own corpuſcles with thoſe of the body it ſhould reſcue, and ſo make in ſome ſenſe a new and further compoſition. For, that ſome bodies may precipitate others, without uniting themſelves with them, is eaſily proved by the experiment of refiners, ſeparating ſilver from copper; for, the mixture being diſſolved in aqua fortis, if the ſolution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you ſhall quickly ſee the ſilver begin to adhere to the plate, not in the form of a calx, as when gold is precipitated to make *aurum fulminans*, or tin-glaſs to make a fine white powder for a *Fucus*; but in the form of a ſhining metalline ſubſtance, that needs no farther reduction to be employed as good ſilver. And by a proper precipitant, I remember, I have alſo in a trice (perhaps in a minute of an hour) reduced a pretty quantity of well diſguiſed mercury into running quickſilver. And if one can well appropriate the precipitants to the bodies they are to recover, very ſlight and unpromiſing agents may perform great matters in a ſhort time; as you may gueſs by the experiment I lately promiſed you: which is this, that, if you take a piece of camphire, and let it lie a-while upon oil of vitriol, ſhaking them now and then, it will be ſo corroded by the oil, as totally to diſappear therein, without retaining ſo much as its ſmell, or any manifeſt quality, whereby one may ſuſpect there is camphire in that mixture; and yet, that a vegetable ſubſtance, thus ſwallowed up, and changed by one of the moſt fretting and deſtroying ſubſtances, that is yet known in the world, ſhould not only retain the eſſential qualities of its nature, but be reſtorable to its obvious and ſenſible ones, in a minute, and that by ſo unpromiſing a medium as common water, you will readily grant, if you pour the diſſolved camphire into a large proportion of that liquor, to whoſe upper parts it will immediately emerge white, brittle, ſtrong-ſcented, and inflammable camphire, as before.

ONE main conſideration I muſt add to the foregoing ones, namely, that body and body being but a parcel, and a parcel of univerſal matter mechanically different; either parcel may ſucceſſively put on forms in a way of circulation, if I may ſo ſpeak, till it return to the form, whence the reckoning was begun, having only its mechanical affections altered.

THAT all bodies agree in one common matter, the ſchools themſelves teach, making what they call the *materia prima* to be the common baſis of them all, and their ſpecifick differences to ſpring from their particular forms: and ſince the true notion of body conſiſts either alone in its extenſion, or in that, and impenetrability together, it will follow, that the differences, which make the varieties of bodies we ſee, muſt not proceed from the nature of matter, of which, as ſuch, we have but one uniform conception; but from certain attributes; ſuch as motion, ſize, poſition, &c. that we are wont to call mechanical affections. To this it will be congruous, that a determinate portion of matter being given, if we ſuppoſe, that an intelligent and otherwiſe duly qualified agent do watch this portion of matter in its whole progreſs, through the various forms it is made to put on, till it come to the end of its courſe, or ſeries of changes; if, I ſay, we ſuppoſe this, and withal, that this intelligent agent lay hold of this portion of matter cloathed in its ultimate form, and extricating it from any other parcels of matter, wherewith it may be mingled, make it exchange its laſt mechanical affections for thoſe, which it had, when the agent firſt began to watch it; in ſuch caſe, I ſay, this portion of matter, how many changes and diſguiſes ſoever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be re-produced, it will become capable of having the ſame relation to it, that formerly it had.

To explain my meaning by a groſs example; ſuppoſe a man cut a large globe, or ſphere, of ſoft wax, in two equal parts or hemiſpheres, and of the one make cones, cylinders, rings, ſcrews, &c. and kneading the other with dough, make an appearance of pie-cruſt, cakes, vermicelli, (as the Italians call paſte, ſqueezed through a perforated plate into the form of little worms,) wafers, biſkets, &c. it is plain, that a man may by diſſolution, and other ways, ſeparate the wax from the dough or paſte, and reduce it in a mould to the ſelf-ſame hemiſphere of wax it was before, and ſo he may deſtroy all, that made the other part of the wax paſs for ſeveral bodies, as cones, or cylinders, or rings, &c. and may reduce it in a mould to one diſtinct ſemi-globe, fit to be re-conjoined to the other, and ſo to re-compoſe ſuch a ſphere of wax, as they conſtituted, before the biſection was made. And to give you an example to the ſame purpoſe, in a caſe, that ſeems much more difficult; if you look upon precipitate, carefully made *per ſe*, you would think, that art has made a body extremely different from the common mercury; this being conſiſtent like a powder, very red in colour, and purgative, and for the moſt part vomitive in operation, though you give but four or five grains of it; and yet, if you but preſs this powder with a due heat, by putting the component particles into a new and fit motion, you may re-unite them together, ſo as to re-obtain, or re-produce the ſame running mercury you had, before the precipitate *per ſe* was made of it.

HERE I muſt beg your leave to recommend more fully to your thoughts that, which, ſoon after the beginning of this diſcourſe, I did (purpoſely) but touch upon, and invite you to conſider with me, that the Chriſtian doctrine doth not aſcribe the reſurrection to nature, or any created agent, but to the peculiar and immediate operation of God, who has declared, that, before the very laſt judgment, he will raiſe the dead. Wherefore, when I lately mentioned ſome chemical ways of recovering bodies from their various diſguiſes, I was far from any deſire it ſhould be imagined, that ſuch ways were the only, or the beſt, that can poſſibly be employed to ſuch an end. For, as the generality of men, without excepting philoſophers themſelves, would not have believed or thought, that, by eaſy chemical ways, bodies, that are reputed to have paſſed into a quite other nature, ſhould be reduced or reſtored to their former condition; ſo, till chemiſtry, and other parts of true natural philoſophy, be more throughly underſtood, and farther promoted, it is probable, that we can ſcarce now imagine, what expedients to re-produce bodies a further diſcovery of the myſteries of art and nature may lead us mortals to. And much leſs can our dim and narrow knowledge determine, what means, even phyſical ones, the moſt wiſe author of nature, and abſolute governor of the world, is able to employ to bring the reſurrection to paſs, ſince it is a part of the imperfection of inferior natures to have but an imperfect apprehenſion of the powers of one, that is incomparably ſuperior to them. And even among us, a child, though endowed with a reaſonable ſoul, cannot conceive, how a geometrician can meaſure inacceſſible heights and diſtances, and much leſs how a coſinographer can determine the whole compaſs of the earth and ſea, or an aſtronomer inveſtigate, how far it is from hence to the moon, and tell many years before, what day and hour, and to what degree, ſhe will be eclipſed. And indeed in the *Indies*, not only children, but rational illiterate men, could not perceive, how it was poſſible for the Europeans to converſe with one another by the help of a piece of paper, at an hundred miles diſtance, and in a moment produce thunder and lightening, and kill men a great way off, as they ſaw gunners and muſketeers do, and much leſs foretel an eclipſe of the moon, as *Columbus* did to his great advantage; which things made the Indians, even the chiefeſt of them, look upon the Spaniards, as perſons of a more than human nature. Now among thoſe, that have a true notion of a Deity, which is a Being both omnipotent and omniſcient; that he can do all, and more than all, that is poſſible to be performed by any way of diſpoſing of matter and motion, is a truth, that will be readily acknowledged, ſince he was able at firſt to produce the world, and contrive ſome part of the univerſal matter of it into the bodies of the firſt man and woman. And that his power extends to the re-union of a ſoul and body, that have been ſeparated by death, we may learn from the experiments God has been pleaſed to give of it both in the Old Teſtament and the New, eſpecially in the raiſing again to life *Lazarus* and Chriſt; of the latter of which particularly, we have proofs cogent enough to ſatisfy any unprejudiced perſon, that deſires but competent arguments to convince him. And that the miraculous power of God will be, as well as his veracity is, engaged in raiſing up the dead, and may ſuffice, if it be ſo, we may not difficultly gather from that excellent admonition of our Saviour to the Sadducees, where he tells them, (as I elſewhere noted) that the two cauſes of their errors are, their not knowing the ſcriptures, wherein God hath declared, he will raiſe the dead; nor the power of God, by which he is able to effect it. But the engagement of God's omnipotence is alſo in that place clearly intimated by St. *Paul*, *Acts* xxvi. 8. where he aſks king *Agrippa*, and his other auditors, why they ſhould think it a thing not to be believed, (ἄπιστον,) that God ſhould raiſe the dead. And the ſame truth is yet more fully expreſſed by the ſame apoſtle, where ſpeaking of Chriſt returning in the glory and power of his father, to judge all mankind, after he has ſaid, that this divine judge ſhall transform, or transfigure (μετασχηματίζειν) our vile bodies (ſpeaking of his own, and thoſe of other ſaints,) to ſubjoin the account on which this ſhall be done, he adds, "That it will be according to the "powerful working, (ἐνέργειαν) whereby he is "able even to ſubdue all things to himſelf, "*Phil.* iii. 21."

AND now, it will be ſeaſonable to apply, what has been delivered in the whole paſt diſcourſe, to our preſent purpoſe.

SINCE then a human body is not ſo confined to a determinate bulk, but that the ſame ſoul, being united to a portion of duly organized matter, is ſaid to conſtitute the ſame man, notwithſtanding the vaſt differences of bigneſs, that there may be, at ſeveral times, between the portions of matter, whereto the human ſoul is united:

Since a conſiderable part of the human body conſiſts of bones, which are bodies of a very determinate nature, and not apt to be deſtroyed by the operation, either of earth or fire:

Since, of the leſs ſtable, and eſpecially the fluid parts of a human body, there is a far greater expence made by inſenſible tranſpiration, than even philoſophers would imagine:

Since the ſmall particles of a reſolved body may retain their own nature, under various alterations and diſguiſes, of which it is poſſible they may be afterwards ſtripped:

Since, without making a human body ceaſe to be the ſame, it may be repaired and augmented by the adaptation of congruouſly diſpoſed matter to that, which pre-exiſted in it:

Since, I ſay, theſe things are ſo, why ſhould it be impoſſible, that a moſt intelligent agent, whoſe omnipotency extends to all that is not truly contradictory to the nature of things, or to his own, ſhould be able ſo to order and watch the particles of a human body, as, that partly of thoſe, that remain in the bones, and partly of thoſe, that copiouſly fly away by inſenſible tranſpiration, and partly of thoſe, that are otherwiſe diſpoſed of upon their reſolution, a competent number may be preſerved or retrieved? ſo, that ſtripping them of their diſguiſes, or extricating them from other parts of matter, to which they may happen to be conjoined, he may re-unite them betwixt themſelves, and, if need be, with particles of matter fit to be contexed with them, and thereby reſtore or reproduce a body; which, being united with the former ſoul, may, in a ſenſe conſonant to the expreſſions of ſcripture, recompoſe the ſame man, whoſe ſoul and body were formerly disjoined by death.

What has been hitherto diſcourſed, ſuppoſes the doctrine of the reſurrection to be taken in a more ſtrict and litteral ſenſe, becauſe I would ſhew, that, even according to that, the difficulties of anſwering what is mentioned againſt the poſſibility of it, are not inſuperable; though I am not ignorant, that it would much facilitate the defence and explication of ſo abſtruſe a thing, if their opinion be admitted, that allow themſelves a greater latitude, in expounding the article of the reſurrection, as if the ſubſtance of it were, that, in regard the human ſoul is the form of man, ſo that whatever duly organized portion of matter it is united to, it therewith conſtitutes the ſame man, the import of the reſurrection is fulfilled in this; that, after death there ſhall be another ſtate, wherein the ſoul ſhall no longer perſevere in its ſeparate condition, or, as it were, widowhood, but ſhall be again united, not to an aetherial, or the like fluid matter, but to ſuch a ſubſtance as may, with tolerable propriety of ſpech, notwithſtanding its differences from our houſes of clay, (as the ſcripture ſpeaks) be called a human body. Job iv. 19.

They, that aſſent to what has been hitherto diſcourſed of the poſſibility of the reſurrection of the ſame bodies, will, I preſume, be much more eaſily induced to admit the poſſibility of the qualifications the Chriſtian religion aſcribes to the glorified bodies of the raiſed ſaints. For, ſuppoſing the truth of the hiſtory of the ſcriptures, we may obſerve, that the power of God has already extended itſelf to the performance of ſuch things, as import as much as we need infer, ſometimes by ſuſpending the natural actings of bodies upon one another, and ſometimes by endowing human and other bodies with preternatural qualities. And indeed lightneſs, or rather agility, indifferent to gravity and levity, incorruption, tranſparency and opacity, figure, colour, &c. being but mechanical affections of matter, it cannot be incredible, that the moſt free and powerful author of thoſe laws of nature, according to which, all the phænomena of qualities are regulated, may (as he thinks fit) introduce, eſtabliſh, or change them in any aſſigned portion of matter, and conſequently in that, whereof a human body conſiſts. Thus, though iron be a body above eight times heavier, bulk for bulk, than water, yet, in the caſe of *Eliſha*'s helve, its native gravity was rendered ineffectual, and it emerged from the bottom to the top of the water: and the gravitation of St. *Peter*'s body was ſuſpended, whilſt his maſter commanded him, and by that command enabled him to come to him walking on the ſea. Thus the operation of the activeſt body in nature, flame, was ſuſpended in *Nebuchadnezzar*'s fiery furnace, whilſt *Daniel*'s three companions walked unharmed in thoſe flames, that, in a trice, conſumed the kindlers of them. Thus did the Iſraelites manna, which was of ſo periſhable a nature, that it would corrupt in little above a day, when gathered in any day of the week but that, which preceded the ſabbath, keep good twice as long, and, when laid up before the ark for a memorial, would laſt whole ages uncorrupted. And to add a proof, that comes more directly home to our purpoſe, the body of our Saviour, after his reſurrection, though it retained the very impreſſions, that the nails of the croſs had made in his hands and feet, and the wound, that the ſpear had made in his ſide, and was ſtill called in the ſcripture his body, as indeed it was, and more ſo, than, according to our paſt diſcourſe, it is neceſſary, that every body ſhould be, that is rejoined to the ſoul in the reſurrection: and yet this glorified body had the ſame qualifications, that are promiſed to the ſaints in their ſtate of glory; St. *Paul* informing us, "That our vile "bodies ſhall be transformed into the like-"neſs of his glorious body," which the hiſtory of the goſpel aſſures us, was endowed with far nobler qualities than before its death. And whereas the apoſtle adds, as we formerly noted, that this great change of ſchematiſm, in the ſaints bodies, will be effected by the irreſiſtible power of Chriſt, we ſhall not much ſcruple at the admiſſion of ſuch an effect from ſuch an agent, if we conſider, how much the bare, ſlight, mechanical alteration of the texture of a body, may change its ſenſible qualities for the better. For, without any viſible additament, I have ſeveral times changed dark and opacous lead into finely coloured tranſparent and ſpecifically lighter glaſs. And there is another inſtance, which, though becauſe of its obviouſneſs it is leſs heeded, is yet more conſiderable: for who will diſtruſt, what advantageous changes ſuch an agent as God can work, by changing the texture of a portion of matter, if he but obſerve, what happens merely upon the account of ſuch a mechanical change in the lighting of a candle, that is newly blown out, by the applying another to the aſcending ſmoke. For, in the twinkling of an eye, an opacous, dark, languid and ſtinking ſmoke loſes all its ſtink, and is changed into a moſt active penetrant and ſhining body.

A
CONJECTURE
CONCERNING THE
BLADDERS of AIR, that are found in FISHES,

Communicated by *A. J.*

And illuſtrated by an EXPERIMENT.

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REFLECTING on that queſtion, whether liquids gravitate upon bodies immerſed or not? I came to a reſolution in my own thoughts, that they do gravitate; and one of the greateſt inſtances, that did occur to me was, that a bubble of air, riſing from the bottom, does dilate itſelf all the way to the top; which is cauſed by the leſſening of the weight or preſſure of the incumbent water, the nearer it is to the top. Upon conſideration of that inſtance, the following conjecture preſented itſelf to my thoughts; that fiſhes, by reaſon of the bladder of air, that is within them, can ſuſtain or keep themſelves in any depth of water. For the air in that bladder is like the bubble, more or leſs compreſſed, according to the depth the fiſh ſwims at, and takes up more or leſs ſpace; and conſequently the body of the fiſh, part of whoſe bulk this bladder is, is greater or leſs according to the ſeveral depths, and yet retains the ſame weight. The rule *de inſidentibus humido* is, that a body, that is heavier than ſo much water, as is equal in quantity to the bulk of it, will ſink; a body, that is lighter, will ſwim; a body of equal weight will reſt in any part of the water.

Now by this rule, if the fiſh in the middle region of the water be of equal weight to the water, that is, commenſurate to the bulk of it, the fiſh will reſt there without any tendency upwards or downwards: and if the fiſh be deeper in the water, the bulk of the fiſh becoming leſs by the compreſſion of the bladder, and yet retaining the ſame weight, it will ſink and reſt at the bottom: and on the other ſide, if the fiſh be higher than that middle region, the air dilating it ſelf, and the bulk of the fiſh conſequently encreaſing, but not the weight, the fiſh will riſe upwards, and reſt at the top of the water.

Perhaps the fiſh by ſome action can emit air out of this bladder, and afterwards out of its body, and alſo, when there is not enough, take in air and convey it to this bladder; and then it will not be wondered, that there ſhould be always a fit proportion of air in the bodies of all fiſhes, to ſerve their uſe, according to the depth of water they are bred and live in: perhaps by ſome muſcle the fiſh can contract this bladder beyond the preſſure of the weight of water: perhaps the fiſh can by its ſides, or ſome other defence, keep off the preſſure of the water, and give the air leave to dilate itſelf. In theſe caſes the fiſh will be helped in all intermediate diſtances, and may riſe or ſink from any region of the water without moving one finn.

It were worth obſerving, what fiſhes want bladders, and if the bladders of ſeveral fiſhes are not of different ſhapes or bigneſs, and how they are in ſea-fiſhes, that live in great depths, and whether any amphibious creatures have them, or any thing analogous; as the lungs may be, or other cavities. By an inquiry into theſe, and other particulars, this conjecture may be either fortified or refuted.

[So far this conjecture: in reference to which, when it was propounded to the honourable *Robert Boyle*, he, reflecting upon the manner how a fiſh comes to riſe or ſink in water, ſoon bethought himſelf of an experiment probably to determine, whether a fiſh makes thoſe motions by conſtricting or expanding himſelf? the experiment by him ſuggeſted was; to take a bolthead with a wide neck, and having filled it almoſt full with water, to put into it ſome live fiſh of a convenient ſize, that is, the biggeſt, that can be got in, as a roch, perch, or the like; and then to draw out the neck of the bolthead as ſlender as you can; and to fill that alſo with water: whereupon the fiſh lying at a certain depth in the water of the glaſs, if upon his ſinking you perceive the water at the ſlender top does ſubſide, you may infer, he contracts himſelf, and if, upon his riſing, the water be alſo raiſed, you may conclude, he dilates himſelf.]

A NEW
ESSAY INSTRUMENT,
Together with the USES thereof.

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SECION. I.

Shewing the occaſion of making this new eſſay-inſtrument, together with the hydroſtatical principles it is founded on.

TO give you now a more explicite and particular account, than I had then time to do, of the inſtrument, which you ſaw tried at the Royal Society, I ſhall inform you, on what grounds I deviſed it, and then annex ſome obſervations about the fabrick and the uſes of it.

You may remember, that many years ago I ſhewed you a little glaſs-inſtrument, conſiſting of a bubble, furniſhed with a long and ſlender ſtem, which was to be put into ſeveral liquors, to compare and eſtimate their ſpecifick gravities, and which I made uſe of to ſome purpoſes, for which it is not, that I know, as yet employed. But afterwards conſidering this little inſtrument ſomewhat more attentively, I thought the application of it might eaſily be, as it were, inverted, and that, whereas it was employed but to diſcover the differing gravities of ſeveral liquors, by its various degrees of immerſion in them, it might be employed to diſcover the ſpecifick gravities of ſeveral appended ſolids, by its being more or leſs depreſſed by them in the ſame liquor. For it is clearly deducible from the grounds of the hydroſtaticks, that any ſolid body, heavier than water, looſes in the water as much of the weight it had in the air, as water of equal bulk to the immerſed ſolid would weigh in the air; and conſequently, ſince gold is by far the moſt ponderous of metals, a piece of gold, and one of equal weight of copper, braſs, or any other metal, being propoſed, the gold muſt be leſs in bulk, than the copper or braſs. And by this means, if both of them be weighed in the water, the gold muſt looſe in that liquor leſs of its former weight than the braſs or copper; becauſe the baſer metal, as well as the gold, grows higher by the weight of a bulk of water equal to it; and the baſer metal being the more voluminous, the correſpondent water muſt weigh more than that, which is equal to the gold.

THIS hydroſtatical principle may be evidently proved from what has been demonſtrated in a mathematical way, by the moſt ſubtile Archimedes *de inſidentibus humido*, and his commentators; and thoſe, that are either unacquainted with, or diſtruſtful of ſuch ratiocinations, may find the principle made out in a phyſical and experimental way in another paper. Whence I concluded, that I might ſafely infer, that the floating inſtrument abovementioned would be made to ſink deeper by an ounce, for inſtance, of gold hanging at it under water, than by an ounce of braſs or any other metal, which by reaſon of its greater bulk than gold, looſing more of its weight by the immerſion, muſt needs retain leſs, and ſo have leſs power to depreſs the inſtrument it was faſtened to. Which concluſion you will eaſily believe the event did upon trial exactly juſtify; and I preſume you will as little doubt, that the concluſion will alſo hold (though the diſparity be not ſo great and conſpicuous) in reference to other metals, as lead and tin, that differ in ſpecifick gravity.

V. Hydroſt. Paradox.

TO give at once an inſtance of the truth and uſe of this notion, I was included to fit the inſtrument, that was grounded on it, for the examination of guineas, which are by far the moſt uſual gold-coins, that paſs in *England*. And though the exactneſs and diligence of our ingenious friend Mr. *Slingſby* allows us to expect, that no injury, that care and ſkill can prevent, ſhall be done to that coin; yet becauſe ſome goldſmiths and others retain fears of being deceived by the fradulent and ſubtile artifices of falſe coiners, I thought it might not be amiſs to furniſh them with an eaſy and practical way of diſtinguiſhing a true guinea from a counterfeit. And though I hope I need not tell you, that I look not upon the inſtrument I ſhewed you at *Greſham-College*, as capable of examining gold and other metals with as much nicety, as by other methods one may hydroſtatically do; yet this little trifle may on ſome occaſions be preferable, ſince the inſtrument, which is not dear, being once fitted, there is no need to have either exact ſcales, or ſkill in hydroſtaticks, or any knowledge of arithmetick, and yet the difference of a true guinea from a counterfeit will not only be ſufficiently, but conſpicuouſly made to appear, and the operation will be much ſooner performed than in the other way, and very much ſooner and cheaper than by the methods commonly employed by goldſmiths and refiners. For, in our way the coin is not defaced or injured by cutting, punching, &c. nor is there any need of touch-ſtones, or aqua-fortis, and yet the trial is ſo quickly made, that perhaps near twenty guineas may be examined, one by one, in about a quarter of an hour: I ſay, one by one, becauſe, that if the inſtrument be deſigned and fitted for ſuch a purpoſe, many guineas may be tried at once. But whether the goldſmiths will make uſe of this way, I leave them to determine; it being ſufficient for me, to have gratified ſuch virtuoſi, eſpecially the diſciples of *Vulcan*, as have given occaſion to expect this trifle will be acceptable unto them; and to add this inſtance to thoſe I have elſewhere given by way of proof, that by the knowledge of cauſes men may employ exceedingly differing means to produce the ſame effects (as in our caſe, gold, that chemiſts and ſay-maſters are fain to examine by the fire, we examine by water) and, that philoſophical truths, and particularly hydroſtatical ones, are not lightly to be deſpiſed, as airy and empty ſpeculations, ſince they may be ſometimes applied to practical uſes, to which at firſt ſight they ſeem to have no relation at all.

SECTION II.

Deſcribing the conſtruction of this inſtrument.

I PROCEED now to the conſtruction of the inſtrument itſelf, in which are to be conſidered the matter and the form.

THE matter may be glaſs, copper, ſilver, or almoſt any other ſolid body, that is, or may be made, fit to float in the water, with a guinea hanging at it, and of a texture cloſe enough to keep out the water. For, if any of that ſhould, by ſoaking or otherwiſe, get in, it may alter the gravity of the inſtrument, and render it deceitful.

MY firſt trials were made with bubbles of glaſs, furniſhed with ſlender ſtems, hermetically ſealed at the top; and theſe, when one can procure an artificer, that can blow them well, are both the gentileſt and the cheapeſt, and for ſome of the uſes, that may hereafter be mentioned, they are almoſt the only ones, that can be fitly employed. But beſides that it is not eaſy to meet with artificers, that can give glaſs the right bigneſs and ſhape, thoſe, as all other inſtruments of glaſs, being very frail and ſubject to be broken; the ſafeſt way and more durable is, to make them of ſome metal, eſpecially either copper or ſilver, (of which the former is far more cheap, and the other more gentile, but either will ſerve well;) in regard they are leſs heavy, and, being more ſtiff, will maintain their figure better than gold or lead. Copper and ſilver will alſo ſuffer themſelves to be beaten into plates thin and yet ſtrong enough, and are not ſo ſubject to ruſt, as iron and ſteel. But in ſome caſes, eſpecially in want of metal in inſtruments, we may make uſe of well ſeaſoned wood, laid over with ſome china varniſh, or ſome other, that is very cloſe.

As to the form of the inſtrument, it conſiſts of three parts; the ball or globulous part; the ſtem or pipe; and that, which holds the coin.

THE ball or round part conſiſts of two thin concave plates of copper, or other metal, exactly ſodered together in the middle; and at the diſtanteſt parts from the commiſſure there ought to be left two oppoſite holes, one in each plate, for the two other parts of the inſtrument. This middle part, though for brevity ſake we name it the ball, ſhould not be exactly round, but, for the conveniency of ſwimming, of an almoſt eliptical or oval form, or rather ſomewhat inclining to that of a very deep double convex glaſs; or it may be of any other ſhape, that ſhall be found fitteſt to make the inſtrument keep its erect poſture ſteadily in the water. The bigneſs of it muſt be ſomewhat greater or leſs, as the plate is made thicker or thinner. But the general rule for its capacity is, that it ſhould contain as much air, as may ſerve to keep the whole inſtrument, when furniſhed, if need be, with its ballaſt and clogged with a guinea, from ſinking beneath the top of the ſtem, which ſtem is the next part to be taken notice of.

IF the inſtrument be to have its ballaſt (if I may ſo call it) within its cavity, it will be convenient, if not neceſſary, that it ſhould be hollow, like a pipe, exactly cloſed at the upper end; but where the ballaſt is to be placed without, the pipe ſhould be made ſolid, as of a piece of wire, or a little cylinder of ſome lighter matter, that will not ſoak in water: but, whether it be hollow or no, it ought to be made very ſlender, that the different depreſſions of the inſtrument in the water may be the more notable. And for the ſame reaſon it ought not to be too ſhort, eſpecially if it be to be applied to other uſes than the examining of guineas.

THE inſtrument, I moſt uſe merely for guineas, hath its ball about the bigneſs of a ſmall hen-egg, or rather leſs, and the pipe between four and five inches long, being ſodered on to the ball at the uppermoſt of the two holes abovementioned; at the undermoſt of which is inſerted and ſodered the undermoſt part of the inſtrument, which I call the ſcrew, or the ſtirrup, becauſe ſometimes it is made of a piece of wire, that a little beneath the bottom of the ball is bent round, ſo as to ſtand horizontally, that the guinea being laid on it, it may be ſupported by it, as the foot is by a ſtirrup; and in this way a piece of coin is the moſt readily put on and taken off. But the more ſecure way is, inſtead of the bent wire, to employ a very ſhort piece of braſs with a broad ſlit in it, capable of receiving the edge of the guinea, which with one turn or two of a ſmall and ſlight lateral ſcrew may be kept faſt in it, and readily, the operation being ended, taken out again.

IF you deſire to examine not only guineas, but greater gold coins and metalline mixtures, it would be convenient, that the undermoſt ſtem and the ſcrew be made by itſelf, that it may be at pleaſure thruſt upon the ſtem and taken off again. For by this means, if the ball of the inſtrument be made large enough, you may have room to put on, as occaſion ſhall require, one, two, or three flat and round pieces of copper, lead, &c. with each of them a hole in the middle, fitted to the ſize of the ſtem, ſo that they may be put on as near the lower part of the ball, as you think fit, and then the ſcrew may be thruſt on after them, not only to take hold of the coin or metalline mixture to be examined, but to ſupport the plate, if need be; and by a variety of ſuch plates, which which may be taken off and put on at pleaſure, the ſame inſtrument if (as I was ſaying) the ball be competently large, may be adjuſted, ſometimes to a guinea, ſometimes to a coin of gold or ſilver, or to a metalline mixture twice or thrice as heavy as a guinea in the air.

THE inſtrument being made of a convenient bigneſs and ſhape; to adjuſt it for the uſe of examining guineas, you muſt by the help of the ſtirrup or ſcrew, hang, at the bottom of it, a piece of that coin, which you know to be genuine, and having carefully ſtopped the orifice of the ſtem, if it be a pipe, (that no water may get in at it,) immerſe the inſtrument leiſurely and perpendicularly into a veſſel full of clean water, until it be depreſſed almoſt to the top of the ſtem, and then letting it alone, if being ſettled it continue in the ſame ſtation and poſture, your work is done, but if it ſink quite under water, you muſt lighten it either with a file, or by ſcraping or grating off a little of the ballaſt-plate above mentioned; or, if you have put any weight into the cavity to poiſe it, by taking out ſome of that, until you have made it light enough: but if, when you leave the inſtrument to itſelf, it emerge, you muſt then add a little weight to it, either by putting into the ſtem, if it be hollow, ſome duſt-ſhot, filings of lead, or ſome other minute and heavy body, or elſe by putting on the ſhort ſtem abovementioned, that comes out beneath the ball, a flat, round, and perforated piece of lead, of weight, ſufficient to enable the guinea to depreſs the weight, as low as it is deſired: which being done, a mark is to be made juſt at the place, where the ſurface of the water touches the ſtem, and then taking out your inſtrument, ſubſtitute in the place of your guinea a little round plate of braſs, of the ſame weight, or a grain or two heavier, in the air; and putting the inſtrument into the water, as before, ſuffer it to ſettle, and make another mark at the interſection of the ſtem and the horizontal ſurface of the water.

ABOUT this way of adjuſting our inſtrument, the following particulars may be noted:

IF a ſcrew be employed to ſuſtain the guinea, the coin ought to be ſo placed, that one half, according to the eſtimate of the eye, may be on the right hand, and the other on the left hand of the ſcrew; that the inſtrument being depreſſed may continue in an erected poſture, and not ſwerve to an inclined.

THOUGH, when the ſtem is hollow, and the inſtrument too light, it may ſeem the better to add quick-ſilver than any other weight, becauſe of its fluidneſs, and great ſpecifick gravity; yet, unleſs the inſtrument be of glaſs, it is not ſafe to employ mercury, becauſe it is apt to diſſolve the ſoder.

IF the marks be made of a white colour, they will be ſo much the more conſpicuous: and theſe marks may be made, if the pipe be hollow, by making round impreſſions with a ſmall file, and encompaſſing them with little circles of fine wire of ſilver, gold, &c. And, if the ſtem be ſolid, it may then be either quite perforated at the requiſite places, and have the holes filled with chawed maſtic, or ſome ſuch white ſubſtance, that diſſolves not in water, or elſe have little holes, that pierce not quite through, ſtuck into it, and theſe may likewiſe be filled with the ſame ſubſtance, which, if further diſtinction be deſired, may have ſome parts of it differingly coloured, before they be employed.

IT will be requiſite to employ in adjuſting the inſtrument one of the heavieſt guineas you can get, to depreſs the inſtrument as low as it is like to be by any piece of that coin, leſt otherwiſe meeting with one conſiderably heavier than that you made uſe of, the inſtrument may be thereby made to ſink to the very bottom of the water.

THE reaſon why it is above preſcribed, that the inſtrument be immerſed almoſt, not quite, to the apex of the ſtem, is, becauſe I have found, that guineas are not all preciſely of the ſame weight, nor all waters neither; and therefore it is ſafeſt, to leave a ſmall part of the ſtem, as an eighth, or, in longer inſtruments, a quarter of an inch, extant above the water; that we may ſecure the inſtrument from being by a heavier guinea made quite to ſink.

I foreſee, it may be hence objected, that theſe contingencies may make our inſtrument uſeleſs: to which it is not difficult to anſwer, that, though ſome guineas weigh a grain or two more than others, it is not that will fruſtrate the uſe of our inſtrument, and leſs will the difference of our waters do it, ſince (as I have obſerved in another paper, where I mention ſome trials of this kind) having examined and compared together the ſpecifick gravities of (common) pump-water, Thames-water, and rain-water, I found the difference far more inconſiderable, than one would have thought, and conſequently unable to keep hydroſtatical trials of metals from being accurate enough for practice, and more exact than thoſe troubleſome and chargeable ones, that are commonly relied on.

THESE anſwers to the recited objections will be made good by this, that it is not a doubtful or inconſiderable difference, that appears upon the differing depreſſions of the inſtrument, that are made by a true guinea, and by a piece of braſs or of copper, of the ſame weight with it in the air. For, in the inſtrument lately deſcribed, though ſmaller than moſt, that I have employed, the diſtance betwixt the mark, to which the gold, and that, to which the other metal, though copper, depreſſed it, was, by meaſure, about an inch and three quarters; ſo, that it is not every ſmall variation of circumſtances, that can make it doubtful to him, that employs our inſtrument, whether a guinea be true or counterfeit.

BUT philoſophical candor forbids me to conceal, that there may, (though it is like there very ſeldom will,) happen a caſe, wherein, though the principle, our inſtrument is framed on, will hold good, yet the practical application may be unſecure. For, if a falſifier of money have the ſkill, by waſhing or otherwiſe, to take off much of the quantity or ſubſtance of the guinea, without altering or impairing either the figure or the ſtamp, the piece of coin will not be able to depreſs our inſtrument to the uſual mark, and may thereby make it be judged counterfeit, when it is indeed but too light.

But on this occaſion it is to be conſidered, that neither the touch-ſtone, nor aqua-fortis, nor antimony, nor the cupel, can ſhew us, whether a piece of coin propoſed have its juſt weight, but only, whether the metal be true gold; and therefore our inſtrument need not pretend to do more, than diſcover the genuineſs of the metal: but whether the coin have the juſt weight the law requires, is to be judged by the balance; as each ſingle piece is wont to be in moſt of the gold coins of *Europe*, and is in *England*, in reference to angels and twenty-ſhilling pieces, and all the other coins of broad gold, as they are now called. And yet it may be further conſidered, that our inſtrument does more than it need pretend to: for, without a pair of ſcales, it preſently ſhews, that the propoſed guinea, if it be not counterfeit, is otherwiſe abuſed; and though it does not clearly determine, whether that likewiſe proceed from the want of ſpecifick gravity in the metal, or from the coins having been waſhed, or otherwiſe fraudulently leſſened; yet it probably reſolves the doubt, becauſe, if the want of weight appear by the inſtrument to be very great, as it uſually does, where the piece has been robbed of ſome of its ſubſtance, (eſpecially if it be ſo much, as is reported, of ſome guineas, that of late are ſaid to have been found wanting to the value of near four ſhillings;) it is a ſtrong preſumption, that it is rather waſhed, &c. than counterfeited. For men will ſcarce venture their lives to ſteal but three or four grains from a true guinea, and much leſs from a falſe one. And they, that counterfeit, are not wont to be ſo ſparing as to make their coins too light. However, our inſtrument will in theſe caſes be ſure to prompt him, that uſes it, to employ the balance, which will preſently aſſiſt him to reſolve his doubt. For, if the ſuſpected coin have in the air its due weight, it will argue, that the greateſt lightneſs of it in the water proceeds from the metal's not being true gold, or, at leaſt, of its not being of the requiſite fineneſs; and, if it want much of its due weight in the air, it is very probable, for the reaſon above-intimated, that it is waſhed, &c. rather, than of another metal than gold; and however may be lawfully refuſed to be taken in payments, and perhaps afford a juſt ground of queſtioning him, that utters it. And if one would, for curioſity, be further ſatisfied, whether the metal be gold or no, one may add to the coin (as will be hereafter taught) as much ſterling-gold, as will make it, in the air, of the weight of a guinea, and then examining it by the weight in the water, he will preſently diſcover, whether it be gold or not.

There comes into my thoughts another poſſible way of counterfeiting guineas; but becauſe it is very likely, that coiners will not light upon it, and it cannot be practiſed on any of the guineas already coined, the fear of teaching bad men a ſkill, that probably they will not otherwiſe acquire, makes me forbear to mention it, though the fraud may be quickly diſcovered, ſometimes by the bare eye, and always by our inſtruments and the balance; whereof publick advertiſements may be given, if there ſhall appear need of it.

And now I have this to add about the conſtruction of this inſtrument, that perhaps it would not be very difficult to propoſe a much more accurate and elaborate contrivance, if it were thought fit to propound any, that would require an extraordinary ſkill in the artificer to make it, and ſome conſiderable ſkill or dexterity in the perſon, that is to uſe it: but the ſlight conſtruction, hitherto diſcribed, ſeemed to ſuit better with my principal aim, which was, to propoſe at preſent an inſtrument, as ſimple, cheap, and eaſy, to be employed and kept in order, as I could well examine guineas with; little doubting, but that the principle, upon which this is framed, being well underſtood and conſidered, will, if it be found uſeful, be further improved by new applications and more artificial contrivances.

Explications of the figures.

In fig. 1. A B, the ſtem or pipe.

C E, the two parts of the ball ſodered together.

B C D E, the ball itſelf.

F, the ſcrew.

G, the ſtirrup, ſomewhat repreſented out of its place.

H, the mark to which a copper-plate, of equal weight in the air with the guinea, depreſſes the inſtrument.

I, the mark to which a true guinea ſinks it.

Fig. 2. is the ſcrew by itſelf, to be put upon, or taken from the (ſhort) undermoſt ſtem of the inſtrument.

Fig. 3. the perforated plates of lead or other metal, to be put on as ballaſt upon the undermoſt ſtem.

Fig. 4. the undermoſt ſtem, with a perforated ballaſt-plate put upon it.

Fig. 5. the ſtirrup, that may be employed inſtead of a ſcrew.

Fig. 6. A B C, the glaſs-inſtrument.

D D D, the coin hanging at the bottom of it, and ſupported by four horſe-hairs, or ſlender ſtrings of ſilk.

Fig. 7. the undermoſt ſtem of the glaſs-inſtrument, to which, being ſtreight and ſolid, a ſcrew is faſtened on with horſe-hair or otherwiſe.

Fig. 8. A B C D, the ſmall glaſs-inſtrument for eſtimating the ſpecifique gravity of liquors, (of which an account may be expected in our next.)

E E, the quick-ſilver and water, that is employed as ballaſt to ſink it in an erected poſture.

SECT.

SECTION III.

Repreſenting the uſes of this inſtrument, as relating to metals.

THERE is in the nature of the thing ſuch a connection between the fabrick and uſe of our inſtrument, that I could not well deſcribe it without plainly intimating the principal uſes of it. Wherefore I ſhall here but ſummarily repeat thoſe, that are delivered already, and make a more explicit mention of thoſe few, that have been either omitted, or but lightly touched.

USE I.

THE firſt uſe, and that, which was mainly intended, is, eaſily and cheaply to diſcriminate true guineas from counterfeit, without defacing, or any ways injuring the coin. But of this uſe I have ſpoken largely enough already, and therefore ſhall advance to the next.

USE II.

ANY other kind of gold-coin, that is near about the weight of a guinea, may be examined by our inſtrument after the manner above delivered; but more eaſily, if it want of the weight of a guinea, than if it exceed it. For in caſe it be heavier, as is a twenty-ſhilling piece of broad gold, the ballaſt, whether internal or external, of the inſtrument muſt be taken off, that ſo heavy a coin may not quite ſink it; whereas, if the coin propoſed be lighter than a guinea, one may add as much gold (of the ſame alloy) beaten into thin plates, as, with the coin propoſed, will make up in the air the weight of a guinea. For then this aggregate, being examined, as if it were a guinea, will diſcover in the water, whether the coin be right or counterfeit. I ſhall add, that if the piece, to be examined, be not much heavier than a guinea, it may be convenient to paſs a very ſmall perforated plate of copper or lead over the upper ſtem (or pipe,) ſo as to make it reſt upon the ball before the inſtrument is adjuſted. For, by this means, nothing need be altered beneath the ball; and ſuch pieces of metal (of which ſeveral differingly heavy may be eaſily provided) being thin and light, will not (as trial has ſhewn) make the inſtrument top heavy, though one of them be placed above the center of gravity, and may be very readily taken off, and (if need be) ſcraped or filed to lighten the inſtrument, when an extraordinarily heavy guinea, or a coin ſomewhat more weighty than a guinea, is to be examined.

BUT to return to what I was ſaying about adding a weight of gold to a piece of propoſed coin; in order to this uſe it will be neceſſary, that the ſlit or aperture at the bottom of the inſtrument, which is to be ſhut and opened by the lateral ſcrew, be made (as it eaſily may without inconvenience) wide enough to receive double the thickneſs of a guinea, that ſo different coins, as *Engliſh*, *French*, *Spaniſh*, &c. and the grain-weights, neceſſary to bring them to the weight required (in the air,) may be ſecurely faſtened to the inſtrument by the ſcrew.

IF the ball be large, and the pipe well proportioned to it, coins, that do not much exceed the weight of a guinea, may be examined without much altering the weight of our inſtrument, provided it be at firſt adjuſted ſo, as that a guinea will not depreſs it ſo far as not to leave a conſiderable part of the pipe above water, that the coin heavier than a guinea may not be able to draw it quite under water.

ACCORDING to the method above deſcribed, may half guineas be examined. For, if the inſtrument be good, it will ſhew a manifeſt difference, if inſtead of an entire guinea, you faſten in the ſcrew a half guinea, that you know to be true, and that, which is ſuſpected to be counterfeit; adding a grain-weight or two of gold, in caſe the propoſed coin needs it; I ſay, a grain-weight of gold, becauſe, if it be of braſs, of which the grain-weights, commonly uſed, are made, it will looſe in water more than it ſhould of the weight it had in the air; and therefore it will be uſeful to ſuch, as intend to try ſeveral ſorts of Engliſh coins, as angels, two and twenty ſhilling pieces, double guineas, &c. to have by them a numerous ſet of grains, (about whoſe ſhape, by the way, one need not be curious, that not being material) made of a thin plate of ſterling gold.

USE III.

IF the inſtrument be ſkilfully fitted for ſuch a purpoſe, it may be made to ſerve to examine ſome ſorts of white money leſs heavy than half crowns. And becauſe it may be uſeful to know in general, what coins may, and what may not, be examined by this or that particular inſtrument propoſed, I ſhall here add a general way, that is not difficult for finding this out; namely, firſt by weighing the piece of gold or ſilver in the air, and afterwards in the water, and ſubſtracting the latter from the former, to obtain the difference of the two weights: and next, by weighing alſo in the air and in water a piece of copper, or braſs, if this be the likelieſt to be employed in counterfeiting the coin, and obſerving likewiſe the difference between thoſe weights. For, the leſſer of theſe differences being ſubſtracted from the greater, the remains will ſhew, how much the true piece of coin will outweigh the other in the water; and conſequently if ſo many grains, as this reſidue amounts to, being added to the weight of the lighter metal, do make a ſufficiently manifeſt depreſſion of it below the mark it would ſtay at without that addition, one may probable conclude, that the difference between a true and counterfeit piece of coin propoſed will be diſcoverable by the inſtrument.

THE cheapneſs of theſe ſlight inſtruments being conſidered, it may be expedient for goldſmiths and others, that have frequent occaſions to examine various ſorts of coin, to have a ſeveral inſtrument adjuſted for each of them, to ſave themſelves ſome pain and trouble. But if the ball be made large, and fitted with a ſtem ſlender and long enough, one may quickly by changing the ballaſt-plates, as occaſion requires, fit the ſame inſtrument to examine coins of differing metals, and of very differing weights. For one of theſe, made of copper, ſerves me to examine both guineas and crown-pieces of ſilver, and half crowns too; and it may be eaſily made to ſerve alſo for divers foreign coins.

Use IV.

It is a great complaint of pewterers, that the tin they buy of the miners or merchants, is often adulterated with lead, as they find to their prejudice, when they have made veſſels of it. And many others, that are buyers, complain much more of divers pewterers for putting too much lead into their pewter, becauſe lead is by many times cheaper than tin. On theſe accounts, I ſhall add, to the other uſe of our inſtrument, ſomething, that relates to tin and pewter. Though I muſt take notice, that ſome tin may perhaps be found a little heavier in ſpecie than ordinary, although no fraud intervene; becauſe I have obſerved ſome tin (as I elſewhere relate) to contain ſome, though but a very little, proportion of gold or ſilver. But this being no uſual caſe, I ſhall proceed to ſay, that the pewterer may judge, whether the miner or merchant have deceived him; if, taking a piece of tin, that he knows to be pure, and is of a convenient weight, he obſerves, how much it depreſſes the pipe, and then makes the like obſervation with an equal piece of the tin ſuſpected to have lead or ſome other metal in it. For if this depreſſes the inſtrument much lower than the other, it will juſtify the ſuſpicion; ſince as gold, being the heavieſt of metals, cannot be allayed by any other, that will not depreſs our inſtrument leſs than gold can do; ſo tin, being the lighteſt of metals, cannot be mixed with any other, that will not ſink it lower than unmixed tin, (ſtill ſuppoſing the weights to be the ſame in the air.)

And as for the buyers of pewter, it will be eaſy for them (if they think it worth while) to find by our inſtrument, if there be too much, or but enough of lead mixed with the tin in an aſſigned portion of pewter of convenient weight to be examined by it. For, having once obſerved, how much the inſtrument is depreſſed by a piece of two, three, or four drams, or even an ounce weight of pewter, which is known to be good, and to contain ſuch a proportion of lead in reference to the tin, if you load the inſtrument with an equally heavy piece of any other maſs of pewter propounded, if the inſtrument ſink deeper, it will be a ſign, that the former proportion of lead may be very probably argued to exceed in the mixture; I ſay, probably, becauſe perhaps it is poſſible to embaſe pewter by mixing not only lead, but other mineral ſubſtances, whoſe ſpecifick gravity is not well known: but yet I ſay very probably, becauſe the addition of too much lead is the moſt gainful way of adulterating pewter. And the other things, that ſome employ, as regulus of antimony, tin-glaſs, copper, and ſpeltar, are ſeldom uſed in great quantities; and if I thought it worth the while, I could facilitate the diſcovery even of theſe by adding, what I have obſerved of their differing ſpecifick gravities, and ſome other things, that I think fitter to be here omitted than to have time and words ſpent upon them.

Use V.

The laſt uſe, I ſhall now mention of our inſtrument, in reference to metals, is, that it may aſſiſt us to eſtimate the quality of metalline mixtures, whether in coins or other maſſes, and to gueſs at the proportion of the ingredients, that compoſe them. For, ſince we have formerly ſeen, that the ſame inſtrument, employed to examine guineas, ſerved alſo for crown-pieces of ſilver, that wanted of an ounce leſs than a twentieth part of that weight, it will be eaſily granted, that the ſame inſtrument, and more eaſily, that a larger one, may be ſo fitted, as to help goldſmiths, chemiſts, and others, that are not acquainted with hydroſtaticks, to make ſuch an eſtimate, as will not much deceive them, of the fineneſs of gold and its differing allays with ſilver, or ſome its other determinate metal.

In order to this, the inſtrument may be be fitted to ſink to the tip of the pipe, with ſome determinate weight of the fineſt gold, as of 24 carats, as they call that, which is moſt pure and fine. But it will be convenient, that this metal in the air be juſt an ounce, or half an ounce, or ſome ſuch determinate weight, that is commodiouſly diviſible into many aliquot parts. Then you may make a mixture, that contains a known proportion of the metal wherewith you allay the gold; as if it hold 19 or 15 parts of gold, and one of ſilver; and, letting the inſtrument ſettle in the water, mark the place, where the ſurface of the water cuts the ſtem or pipe. And then putting in another mixture, wherein the ſilver has a new and greater proportion to the gold; as if the former be an eighteenth or a fourteenth part of the latter, you may obſerve, how much leſs than before this depreſſes the inſtrument, and ſo you may proceed with as many mixtures or degrees of allays, as you think fit, or can be diſtinguiſhed conveniently on the ſtem; being always careful, that, whatever be the proportion of the two ingredients, the weight of the maſs in the air be juſt the ſame with that of the pure gold, which we have lately ſuppoſed to be one ounce, or half an ounce.

By the ſame method may be examined the differing alloys of pure ſilver upon the admixture of ſuch and ſuch determinate proportions of copper, or any other metal, lighter in ſpecie than ſilver; and by the ſame way, with a ſlight variation, it will not be difficult to eſtimate, how much divers coins, whether of ſilver or gold, are more or leſs embaſed by the known ignobler metal, that is mixed in the piece propoſed.

And though this way of determining the alloys of metals be not ſo exact, as is poſſible to be propoſed by the help of hydroſtaticks and calculation; yet it may be very uſeful to chemiſts, goldſmiths, refiners, and others, that are unacquainted with hydroſtatical matters, to make without trouble or ſupputation eſtimates, that will not much deceive them, and perhaps will come nearer the truth, not only than the eſtimates wont to be made by the touchſtone, but perhaps too than ſome of thoſe, that divers make with trouble, and inconvenience, and charge. And indeed I was chiefly invited to communicate this trifle, and ſpend ſo many words about it, by the requeſt of ſome ingenious diſciples of *Vulcan*, who thought they perceived, that by this way they could oftentimes make better eſtimates of the ſucceſs of their graduating, and ſome other operations upon metals, than otherwiſe they ſhould be able; this way greatly accommodating them by this particular advantage, that they may from time to time try the degrees of purity, and ſome other conſiderable alterations of their mixtures, without at all deſtroying or injuring them, though they have not yet attained the pitch they aim at and expect; whereas, if they happen to be too forward, as often they are, in examining the productions of their labours by the cupel or ſevere cementations, what they would try may be deſtroyed or ſpoiled in its way to a perfection, which otherwiſe, in their opinion, it might in due time be brought to.

Perhaps it may not be amiſs, on this occaſion, to add, as an improvement of this fifth uſe of our inſtrument, that it may be employed to examine other mixtures beſides allayed coins, and that if the inſtrument be adjuſted to an ounce, for inſtance, of pure copper, it may help men to make an eſtimate of the alloy of tin, or the quantity of it, that is oftentimes added to copper, to make differing ſorts of bell-metal, and of thoſe metalline ſpecula, whether plain or concave, that are called ſteel glaſſes, as alſo of ſoders conſiſting of certain proportions of ſilver and braſs, or copper; in all which, and divers others, the diſcovery of the proportion of the ingredients may, on ſome occaſions, be uſeful to tradeſmen, as well as deſirable by virtuoſi. And though I have obſerved, that by mixture, tin and copper acquire a ſpecifick gravity ſomewhat differing from what their ingredients promiſe; yet, ſince the inſtrument is to be fitted for ſuch eſtimates, not by calculation, but by trials, the eſtimates may be made near enough to the truth.

NEW EXPERIMENTS

ABOUT THE

Weakened SPRING, and ſome unobſerved Effects of the AIR.

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As for the not yet communicated trials, that I made in proſecuting my deſign of diſcovering or obſerving ſome latent qualities of the air, I will not deny you ſome of them, as imperfect as yet they are, but will venture to ſend them you, as my notes or my memory ſuggeſts them to me; not only, becauſe without being compleated they may be fit enough to countenance ſuſpicions (for you know, that I do not call them ſo much as opinions,) but for a weightier inducement, to be told you at the end of this paper.

The two chief things aimed at in the imperfect attempts I now ſend you, were to diſcover; firſt, whether, as ſome corroſions of bodies do in cloſe veſſels increaſe the ſpring of the air (as I long ſince noted them to do,) ſo ſome other corroſions may not, by a contrary, or ſome other way, weaken the ſpring of the air; and next, whether in ſome ſolutions and precipitations the air on the account of ſome unobſerved quality may not be found to produce ſome phænomena not yet taken notice of.

In order to each of theſe inquiries, I ſhall mention a few trials, though without curiouſly ſorting them, becauſe ſometimes in the ſame experiment both thoſe attempts were jointly proſecuted.

You may remember, that in ſome of my formerly publiſhed trials I acquainted you with an odd phænomenon of the change of colour producible in ſolutions of copper by the operation of the air: I ſhall now add, what further phænomena my memory or notes ſupply me with, about the ſubject of that and the like experiments.

EXPERIMENT I.

WE took filings of crude copper, and put them into a chryſtalline glaſs of a conical ſhape, into which we poured ſome ſtrong ſpirit of ſalt, (that was fitted for our peculiar purpoſe) to the height of about a finger's breadth above the filings; and then cloſing the veſſel with a glaſs-ſtopple exquiſitely fitted to it, we ſuffered it to continue unmoved in a window for ſome days, untill the liquor had both obtained a high and darkiſh brown colour by the ſolution of ſome of the copper, and loſt that colour again, growing clear like common water, (which is itſelf a ſomewhat odd phænomenon;) and then taking out the ſtopple, (without ſhaking the liquor) and thereby giving acceſs to the outward air, we perceived, (as we had conjectured) that the upper ſurface of the liquor did in a few minutes re-acquire a darkiſh brown colour, which penetrating deeper and deeper, at the end of about a quarter of an hour the whole body of the liquor appeared to be likewiſe tinged. The conical glaſs being again well ſtopped, the menſtruum did again in very few days let fall, or otherwiſe looſe its tincture; which, the ſtopple being taken out, it re-gained as before. Nor were theſe two the only trials I made with the like ſucceſs for the main; but afterwards being deſirous by a further trial to reſolve a doubt I had, I kept the glaſs yet longer in the ſame place with the ſame filings and menſtruum in it for (if I miſ-remember not) a month or two together; but obſerved not, that the liquor would any more grow clear.

EXPERIMENT II.

HAVING taken ſuch a glaſs, as is mentioned in the firſt experiment, wherein the liquor was grown clearer than is uſual, and had probably been ſo a good while before (for the veſſel, having been hid by others, which ſtood before it, had been for ſome weeks forgotten;) we took out the ſtopple, and left it open for about half an hour, but did not perceive the liquor to have acquired any colour ſo much as at the top. Whether this proceeded from the long debarring of commerce with the freſh air, or from ſome other cauſe, being unable to wait the event as long as would perhaps be requiſite, I thought fit to try, whether the air had already had ſome operation upon the liquor, though it did not yet appear; and accordingly putting in the ſtopple, I left the veſſel cloſed for two or three hours, and at my return to viſit it, I perceived, that it had acquired a faint colour tending to a green; wherefore taking out the ſtopple again, I opened its commerce with the outward air, leaving the glaſs unſtopped for 20 or 24 hours, but found, that in all that time it had not regained its wonted dark colour, but was only arrived at a green, deep enough, but not true, nor very tranſparent.

THIS obſervation being made in the ſame veſſel, that had been formerly employed, ſuggeſted to us an enquiry, whether the advanced time of the year, which was the middle of *October*, might not have an intereſt in the ſlow and imperfect ſucceſs of this trial.

EXPERIMENT III.

SOME ſtrong ſpirit of ſalt having been kept upon filings of copper, until the ſolution was come to be of a dark brown colour, about three ſpoonfuls of it by gueſs was put into a receiver, that might hold eight or ten times as much: being kept *in vacuo* (if the time be rightly remembered) about half a year, it retained its colour, but the veſſel being opened and the external air permitted a free acceſs to it, the ſolution in about an hour was turned into a fine tranſparent green, though no precipitation of any muddy ſubſtance appeared by any ſediment to be made.

EXPERIMENT IV.

IN one of that ſort of conical glaſſes, that has been already more than once deſcribed, we had put upon ſome filings of copper, a convenient quantity of our ſpirit of ſalt; and though we obſerved, that for a great while it would not part with its deep and ſomewhat muddy tincture; yet we left it in the window for many weeks longer, and at length, towards the latter end of *December*, we found it to have loſt its tincture, ſo much, that the liquor appeared like common water. Upon which obſervation, though the time of the year were unpromiſing, I thought fit to try, whether the air in that ſeaſon would not have ſome, though perhaps but a ſlow operation on the ſaline ſpirit, and accordingly taking out the glaſs-ſtopple to give free acceſs to the outward air, we obſerved, that in ſome hours its operation on the liquor was ſcarce ſenſible, but within about 24 hours the menſtruum had acquired not juſt its former colour, but a ſomewhat faint and moderately tranſparent green: ſo that this tincted menſtruum, as it had been very ſlow in loſing its colour, ſo it did but ſlowly and imperfectly re-acquire it.

"I HAVE not in the foregoing experiments "made mention of any phænemena of "them relating to the Spring of the included "Air, becauſe I do not remember, that they "were ſuch, as invited me to draw any poſitive "concluſion from them, and my ſilence on "this occaſion may be the more allowable, "becauſe the way of further making ſuch obſervations "may be ſufficiently deduced from "the enſuing trials; in reciting of which I "alter very little, and in ſome of them not at "all, the expreſſions I find them regiſtered "in, though more than once the phænomena, "that relate to the air's elaſtic power, be "mingled in the ſame experiment with the "mention of its operations upon colours.

"THE ſpring of the air and its variations, "by the ways now known to many of the curious, "being things, that manifeſtly appear "to have a notable intereſt in divers phæno-

“ mena of nature, whoſe cauſes, if not them-
“ ſelves alſo, were unknown to former philo-
“ ſophers: it ſeemed an attempt, though not
“ very promiſing, yet worth the making, to
“ try, whether the ſpring of the air, which may
“ divers ways, as by heat, compreſſion, &c.
“ be increaſed, may not by ſome other way
“ than cold and dilatation be weakened: and
“ having often found menſtruums, that corrode
“ metals, ſo as to produce bubbles to invi-
“ gorate the ſtrength of the ſpring of the air
“ included in the veſſels, wherein the ſolution
“ was made, I thought fit to try, whether in
“ ſome metalline diſſolutions, wherein I had
“ obſerved, that few or no viſible bubbles at
“ all were produced, the ſpring of the neigh-
“ bouring and included air would not be de-
“ bilitated; and in order to this were made
“ the following trials.”

EXPERIMENT V.

[WE took ſome filings of copper, and putting them together with a mercurial gage * in a conical glaſs fitted with an exactly ground-ſtopple of the ſame matter, (which was chryſtalline) we poured on the filings, as much rectified ſpirit of fermented urine made *per ſe*, as ſufficed to ſwim an inch or better above them; then carefully ſtopping the glaſs, coming to look on it many hours after, we perceived, that the mercury in the ſealed leg was conſiderably depreſſed, and gently drawing out the ſtopple to let in the outward air, we perceived that acceſs to have a manifeſt effect upon the mercury.]

BUT this will be better underſtood by the more circumſtantial experiment, that enſues.

EXPERIMENT VI.

[WE took a cryſtal glaſs of an almoſt conical ſhape, and capable of containing between five and ſix ounces of water, and furniſhed with a ſtopple of the ſame matter, that by grinding was exactly fitted to it. Into this we put a convenient quantity of clean filings of good copper, on which we poured as much ſtrong ſpirit of (fermented, or rather, putrified urine, as ſerved to ſwim about an inch above the copper, and having let down a mercurial gage, ſo that it leaned upon the bottom and ſide of the glaſs, we cloſed it very well with a ſtopple, and ſet it in a quiet and well enlightened place, having taken good notice at what mark the quickſilver reſted in the open leg of the gage. This done, we let the menſtruum alone to work upon the filings; which it did, as we foreſaw, ſomewhat ſlowly and very calmly, without producing any noiſe or ſenſible bubbles, acquiring by degrees a very pleaſant blue colour, and the glaſs being kept quiet in the ſame place for two or three days longer, the liquor, as I conjectured would happen, began to loſe of the intenſneſs of its colour, which by degrees grew fainter and fainter, until at the end of three or four days the liquor was grown very pale, and left me little doubt but that, if I would have ſtaid ſome days longer, it would have loſt the remaining eye of blue, and have looked almoſt like common water. But being unwilling to tarry ſo long, I took out the ſtopple, that the air without the glaſs might have acceſs to that within; and leaving the vial in the ſame place and poſture, my expectation was ſomewhat anſwered by finding, that within four or five minutes, if not leſs, the upper part of the liquor, that was contiguous to the air, had acquired a fine blue colour, which deſcending deeper and deeper, before the end of the tenth minute had diffuſed itſelf, but ſomewhat weakened, through the liquor, whoſe colour was ſuffered to deepen for a while longer; ſo that in leſs than a quarter of an hour from the firſt unſtopping of the vial, the liquor was grown to be throughout of a rich ceruleous colour, which grew almoſt too opacous within a few minutes longer: when carefully cloſing the vial again with the ſame ſtopple as before, we ſet it aſide in the ſame place, where, the included air being denied all commerce with the external, the liquor began again, within two or three days, to loſe of its colour, and, to be ſhort, afforded me the opportunity of making a ſecond experiment, much like the former. And the like ſucceſs I had, for the main, in a trial or two made in another glaſs with another portion of the ſame ſpirit of urine, put upon the filings of copper; ſo that the experiment was, in all, made divers times, as well when I was not, as when I was alone: and particularly, once to be ſure, that the diurnal air as ſuch had not any great intereſt in the phænomenon, I made the trial ſucceſsfully about nine a clock at night, in the preſence of ſo well known a witneſs, as the learned ſecretary of the Royal-Society.

See the reference in the foregoing experiment.

ONE circumſtance I forgot to take notice of, which was, that in moſt of theſe experiments I forbore to ſhake the glaſs, leſt it ſhould be ſuſpected, that the agitation of the liquor might have raiſed ſome little fine powder, that might have been ſuppoſed to have been precipitated out of the tincture, and, being thus mingled with the liquor again, reſtore it to its former colour; but in truth, I did not perceive any ſuch powder to be precipitated. And though, to obviate the objection, I forbore to ſhake the vial; yet I juſtly ſuppoſed, that if, by the agitation of the liquor, more parts of it ſhould be quickly expoſed to the action of the air, the coloration would be haſtened, which upon trial appeared to be true.]

EXPERIMENT VII.

[EXPERIENCE have made me think it likely, that ſtrong ſpirit of ſal armoniac, made without quick-lime, would operate more nimbly and more powerfully on that metal than our ſpirit of urine had done; we took ſuch a conical glaſs, as has been lately deſcribed, and covering the bottom of it with a convenient quantity of filings of good copper, we poured on them as much ſtrong ſpirit of ſal armoniac, as ſerved to ſwim about a finger's breadth above them; and, having let down ſuch a mercurial gage, as is formerly mentioned, ſo that it leaned upon the bottom and ſide of the glaſs, we cloſed it very well with a ſtopple, and ſet it in a quiet and well enlightened place, having taken good notice at what mark the quick-ſilver reſted in the open leg of the gage: this done, we let the menſtruum alone to work upon the filings, which it did, as we foreſaw, ſomewhat ſlowly and very calmly, without producing any noiſe or ſenſible bubbles, acquiring by degrees a very pleaſant blue colour, and afforded us alſo the phænomenon we chiefly looked after; which was, that repairing from time to time to the window to ſee what paſſed, we perceived, that for two or three days together the mercury in the ſealed leg of the gage did, though very ſlowly, deſcend, until it appeared to be near a quarter of an inch lower than at firſt; and probably the depreſſion might have been greater, if ſome indiſcreet body or other had not, by tampering with the glaſs, diſturbed the experiment; whoſe event yet ſeemed ſufficiently to argue, that the ſpring of the air, contained in the cavity of the glaſs, and communicating with that in the open leg of the gage or ſyphon, was weakened in compariſon of that in the cloſed leg, which by the hermetic ſeal on one ſide, and the quickſilver on the other ſide, was kept from ſuch communication.

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* About ſuch glaſſes, ſee Experiment XVII. in the continuation of our New Phyſico Mechanical Experiments.

AND becauſe I thought it might be ſuſpected, that the phænomenon might be referrable to ſome inequality in the preſſure of the air, occaſioned by the greater operation of the heat of the day on the more impriſoned air of the gage, than on that more immediately included in the cavity of the vial; I was careful to obſerve, whether the depreſſion did not continue at differing times of the day, and found it to do ſo, as well at night, as at noon, though at this laſt named time, the ſun ſhined hot upon the place and veſſels too.

THIS experiment was made, in all, four or five times, though not always with equal, yet ſtill with ſome ſucceſs, the mercury in the ſealed leg of the gage being ſometimes more and ſometimes leſs, but always manifeſtly depreſſed; which phænomenon was confirmed by the obſervation we more than once made of the ſudden return of the quickſilver to its former ſtation, upon the unſtopping of the glaſs, to give free admiſſion to the outward air.

EXPERIMENT VIII.

CONSIDERING, whilſt I was about theſe trials, that ſpirit of vinegar, though in working upon coral and ſome other bodies, it not only produces ſtore of bubbles, but alſo, as I have elſewhere delivered*, a ſomewhat odd kind of elaſtical ſubſtance, yet being put upon minium it was wont, in my obſervation, to work calmly and without producing froth; I thought fit to make trial, whether this calm and ſilent ſolution of minium would be accompanied with a permanent change of the air's ſpring: the event I find thus ſet down:

[A pretty quantity of ſpirit of vinegar being put upon minium in a conical glaſs, furniſhed with a glaſs ſtopple and a mercurial gage, continued divers days without any ſenſible depreſſion of the mercury in either leg, nor did any change appear in the gage, upon the removal of the ſtopple, though it was evident by the great ſweetneſs acquired, that it had made a ſolution of a great portion of the minium.] But to return to our trials upon copper.

EXPERIMENT IX.

[WE took ſome filings of copper, and in a vial capable of holding ſome two or three ounces of water, we poured on them ſtrong ſpirit of ſal armoniac, made without quicklime, till the liquor reached near an inch above them. This was done about the twentieth of *Auguſt*, on the *Friday* before noon, and the following *Monday*, preſently after dinner, it had acquired a deep blue tincture, and loſt again ſo much of it, that it was pale, almoſt like common water: then, to ſatisfy a virtuoſo, I unſtopped the vial, deſiring him to place his eye level with the ſurface of the liquor, which, in a minute of an hour, or leſs, appeared, to his ſurpriſe and wonder, to have acquired a deep blue tincture, that reached downwards to the thickneſs of the back of a knife, the whole liquor becoming of the like colour in four or five minutes more, and the glaſs being preſently ſtopped again, and left where it was before, appeared not at the end of nine days, to have loſt its tincture; though now and then, within that time, it ſeemed manifeſtly paler than when the vial was ſtopped.]

“NONE of the former trials with ſpirit of “ſal armoniac having been made in an her“metically ſealed glaſs, it will not be amiſs “both to diverſify and to confirm our experi“ments, by ſetting down the ſucceſs of one “made in ſuch a veſſel.”

EXPERIMENT X.

[WE took a round vial, holding about eight ounces of water, and having put into it filings of copper and a mercurial gage, we poured on the metal ſtrong ſpirit of ſal armoniac, till it reached to a good height in the vial, which then being hermetically ſealed up, was ſet by in a ſouth-window, where it quickly acquired a deep blue tincture: there it ſtood about twelve days, before that tincture, which decayed but ſlowly, did, little by little, grow ſo diluted, that the liquor was pale and almoſt like water: during this ſtay of the glaſs in the window, the mercury in the open leg appeared to be impelled up, and when after nine o' clock at night, (which time I choſe to try, whether the nocturnal air, as nocturnal, would have any thing to do with the phænomenon,) the hermetic ſeal was broken off; immediately upon which there was produced a noiſe, and the mercury in the ſhorter and cloſed leg was briſkly impelled up, by our gueſs, near three eighths of an inch, and though the orifice, at which the air had acceſs, was ſcarce wide enough to admit a middle-ſized pea, yet, within a minute and half, the ſurface of the liquor being held between the eye and the candle, appeared to have acquired a very lovely and fair colour, which reached downwards a quarter of an inch; ſo that the vial ſeemed to contain two very differing liquors ſwimming on one another, and the coloration piercing deeper and deeper within five minutes in all, the whole liquor had attained a rich blue colour.]

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* To the better underſtanding of this, the enſuing trial may much conduce; and therefore is tranſcribed out of another paper, to which it properly belongs.

A mercurial gage having been put into a conical glaſs whoſe bottom was covered with beaten coral, ſome ſpirit of vinegar was poured in, and then the glaſs ſtopple, which was very well ground, cloſing the neck exactly, we obſerved, that upon the working of the menſtruum on the coral, ſtore of bubbles were for a good while produced, which ſucceſſively broke in the cavity of the veſſel, and their acceſſion ſo conſtipated the air, that they compreſſed the air impriſoned in the cloſed leg of the gage three marks or diviſions, which I gueſſed to amount to about the third part of the extent it had before: but ſome hours after the corroſion had ceaſed, the compreſſion made by this newly generated air grew manifeſtly fainter, and the impriſoned gage air drove down the mercury again, till it was depreſſed within one diviſion of its firſt ſtation; and thereabouts, or little lower, continued five or ſix days; ſo that in this operation there ſeemed to have been a double compreſſive power exerciſed; the one tranſient, by the briſk agitation of vapours or exhalations, and the other durable, from the aerial and ſpringy particles, either produced or extricated by the action of the ſpirit of vinegar upon the coral.

"With this experiment I ſhall conclude this paper: for though I made ſeveral other trials, with the ſame deſign, that I made the foregoing ones, as with ſpirit of nitre, and minium, ſpirit of vinegar and copper; yet a preſent want of time hinders me from troubling you with them, which I the rather forbear to do, becauſe I fear, they would prove leſs ſatisfactory than thoſe I have ſet down, which themſelves muſt, to a leſs diſcerning eye than yours, appear very imperfect, notwithſtanding that prolixity in reciting ſome of them, which I was obliged to by my not yet knowing, in ſuch odd attempts, what circumſtances might ſafely be omitted. But ſuch as they are, I ſend them you, who, by your diffuſed correſpondency, have great opportunity to get them made, if you think them worth it, by curious perſons in ſeveral countries, various manners, and differing ſeaſons of the year: and however the things I ſend you be but trifles, yet their novelty may, perhaps, excite the induſtry of others, and give riſe to further enquiries."

AN
EXPERIMENTAL DISCOURSE
OF
QUICKSILVER growing hot with GOLD.

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The INTRODUCTION of the Publisher.

Though the following diſcourſe was by the author of it made part of a ſhort Examen of the ſuppoſed ſympathy between gold and quickſilver, (which itſelf belongs to another treatiſe;) yet the worthineſs of the ſubject, and the great curioſity, that is obſerved among many virtuoſi, (not only chemiſts, but others,) about mercurial preparations and experiment, made me think I might do them an acceptable piece of ſervice, if I could prevail with the author to ſever them from the papers, whereto he had annexed them, (but to which they ſeemed not abſolutely neceſſary) though upon the conditions he judged requiſite to inſiſt on.

And ſince I venture to impart before the time theſe things unto the curious, I hope and deſire, they will be ſo equitable, as to indemnify me to the author, and not fruitleſly endeavour to put a perſon, that has already given ſo many proofs of his propenſity to gratify ingenious men, upon making unſeaſonable anſwers to any verbal or epiſtolary queſtions about things, wherein ſome conſiderations, that he thinks are not to be diſpenſed with by him, do as yet injoin him ſilence.

Now, to gratify the curious among ſtrangers, as well as thoſe of our own nation, the publiſher was not unwilling to give this diſcourſe in Latin, as the author hath been pleaſed to impart it in Engliſh.

Follows the DISCOURSE itſelf.

Of mercury growing hot with gold.

1. —BUT that what I have hitherto ſaid, may not be drawn to the diſparagement or diſcouragement of thoſe Spagyriſts, that poſſeſs or aſpire to the nobler arcana of gold and mercury, I muſt mind you to take notice, that what I have objected againſt the ſuppoſed ſympathy of gold and quickſilver, is ſpoken only of common mercury, that being it, whoſe ſympathy with gold is wont to be celebrated. And though perhaps, a good part of the things I have alledged will be found applicable even to true running mercuries; yet I would not be thought to deny, that there may be a quickſilver more ſubtile and penetrant than that which is common, and that thoſe chemiſts, that ground the ſympathy of gold and mercury upon the operations of a more philoſophical mercury, may likewiſe argue for it more ſpeciouſly, than vulgar mercury will enable them to do. And to let you ſee on this occaſion, that I am not unkind to the chemiſts, I will annex part of a paper, written to a friend to give him my opinion about mercury's incaleſcence with gold.

2. —AND now I ſhall abruptly begin this ſection with the conſideration of a problem much agitated among the curious, eſpecially thoſe, that pretend, whether truly or vainly, to have more than ordinary inſight into chemiſtry: among whom I find it hotly diſputed, whether or no there be any ſuch thing, as a mercury, that will heat with gold, that is, which by being barely mingled with that metal reduced to fine parts, will, without the help of external heat, produce upon the commixture of thoſe two bodies very ſenſible heat.

3. THE affirmative of this queſtion is poſitively aſſerted by ſome writers and others, that pretend to the tranſmutation of metals: for, among theſe, I have met with ſome, that aſcribe this virtue of incaleſcence with gold to the mercuries extracted, as they ſuppoſe, from ſome complete metals, which are therefore in their phraſe ſtiled *mercurii corporum*, or the mercuries of the metalline bodies.

4. BUT the negative part of the queſtion is more generally maintained, being not only embraced by far the greateſt number of philoſophers and phyſicians, but aſſented to by many of the more learned Spagyriſts themſelves, eſpecially the modern, divers of whom have reckoned this ſort of mercuries among the chimæra's and *non-entia* of bragging chemiſts. And I have the leſs wondered to find many learned men ſo averſe from believing this incalſcence of mercury and gold, becauſe, having purpoſely enquired of ſeveral prying alchemiſts, that have ſpent much labour, and many trials, to find out things of this kind, and have,

De Mercurio cum auro incaleſcente.

—VERÙM enimverò, ne quæ hactenus diſſerui eò torqueantur, ac ſi laudes animoſque viris illis Spagyricis demere velimus, qui nobiliora auri & argenti vivi arcana poſſident ambiuntve, monendus es mihi, ut advertas, me quod contra ſuppoſitam auri & mercurii ſympathiam objeci, de vulgari duntaxat mercurio dictum velle, cum ille ſit, cujus cum auro ſympathia celebrari ſueverit. Et quamvis fortè magna à me dictorum pars, conſultâ experientiâ, ad nativum etiam mercurium currentem extendi poſſit, non tamen cenſere lectorem velim, negare me, dari argentum vivum poſſe vulgari ſubtilius & penetrantius, iſtoſque chymicos, qui auri & mercurii ſympathium niti voluut mercurii magis philoſophici operationibus, contendere etiam pro ea multò ſpecioſius poſſe, quàm ſi vulgaris duntaxat mercurius adhibeatur. Atque ut hac occaſione teſtatum faciam, me viros chymiæ addictos neutiquam averſari; ſubjungam hic ſcripti mei partem, ad amicum quendam idcircò exarati, ut meam ipſi de mercurii cum auro incaleſcentia opinionem depromerem.

—NUNC verò abruptè ſectionem hanc ordiar problematis cujuſdam diſcuſſione, quod diu multumque inter curioſos fuit agitatum, eos imprimis, qui, ſive verè ſive falsò, obtendunt, ſe intimiores, quàm vulgo conceſſum eſt, chymiæ receſſus adiiſſe: inter quos id calidè diſceptari reperio, utrum ejuſmodi detur mercurius, qui incaleſcat cum auro, id eſt, qui, dum nudè metallo iſti, ad minutas admodùm partes redacto, commiſcetur, citra externi caloris adminiculum, factâ ſolummodò duorum illorum corporum cramate, ſenſibilem valdè calorem pariat.

HUJUS quæſtionis affirmativam mordicùs tenent nonnulli authores, aliique, qui metallorum tranſmutationem ſibi vendicant; inter hos quippe nonnullos videre mihi licuit, qui hanc incaleſcendi cum auro virtutem mercuriis adſcribunt, ex perfectis quibuſdam corporibus, ut autumant, elicitis; quos idcircò mercurios corporum, *ſive mercurios corporum metallicorum, nuncupare ſolent.*

AT negativam tuentur multò plures, iique non modò philoſophi & medici, ſed & ex ipſis Spagyricis doctrinâ clariores, imprimis, ex neotericis & modernis, quorum non pauci hanc mercuriorum familiam chimæris & non-entibus grandiloquentium chemiſtarum accenſent. Atque eò minùs mirabar, complures viros doctos adeò eſſe ab hoc mercurii cum auro incaleſcentiæ aſſenſu alienos, quia, conſultò quæſiti à me plures ex alchymiſtis ſagacioribus, qui multum impenderant operæ, plurimaque experimenta peregerant ad hujus generis arcana depromenda, quique per aliquot annos noviſſimos varias Europæ partes permearant, ut aliorum, qui tranſmutationes

of late years, travelled into many parts of *Europe*, to pry into the ſecrets of other ſeekers of metalline tranſmutations, they have apart ingenuouſly confeſſed to me, that they never actually ſaw any incaleſcent mercury, though they ſometimes heard it boaſted of by all alchemiſts, whoſe bold pretenſions had the leſs weight with me in this matter, becauſe I had long taken notice, how great a confidence, fraud, or ignorance (for I would not think all thoſe cheats, that are miſtaken,) can give to ſome of that ſort of men, that I am ſpeaking of. Inſomuch that one of them having impoſed upon an honeſt chemiſt, well known, and much employed, with a pretended incaleſcent mercury, they had the confidence to bring it me to convince me of the experiment; but upon due trial, I found not any ſenſible degree of that great heat, that was promiſed. Which miſcarriage was vainly pretended to be ſalved by I know not what unſatisfactory excuſes.

5. **But**, notwithſtanding all this, having, for the reaſons I have long ſince expreſſed in other papers (and for ſome other conſiderations, that I have not judged fit to mention) looked upon mercury as a body, which is not neceſſarily ſo homogeneous, as it is ſuppoſed, the opinion I moſt liked of was that of a poſſibility of an incaleſcent mercury. For, notwithſtanding the vulgarly ſuppoſed ſimular nature of quickſilver, which I willingly confeſs to be great enough to be admirable, it was yet congruous to my principles, that a liquor, which in weight, colour, total volatility, &c. was anſwerable to all the eſſential properties for which a body is called mercury, might yet have an internal conſtitution of parts, that might make it in ſome unobſerved things conſiderably differing from common mercury. And, among theſe differing qualites, I did not know but one might well be, that of growing hot with gold. And this opinion I judged the more reaſonable, becauſe, having deviſed two ways (unpractiſed, that I know of, by any chymiſt) the one, to diſcover, whether a clean and carefully diſtilled mercury might not be a compounded body, and have in it parts, that were not mercurial; and the other, out of ſuch a fine diſtilled mercury to ſeparate parts, and that in no deſpicable number, that are plainly heterogeneous; I found, upon trial, that both the methods I had thought on would ſucceed, which warranted me to think it poſſible, that a mercury very fine and clean, and even purged by ſublimations and diſtillations, may, by art, have been made to aſſume and incorporate with it a multitude of heterogeneous corpuſcles, not to be diſcovered, much leſs ſeparated, (as thoſe of tin, lead, &c. may be) but by a ſkilful artiſt.

6. **This**, in the general, may ſuffice to make me ſuſpend my judgment about the problem formerly propoſed, and to engage me to make trials, whether ſome of theſe heterogeneous particles, that I found reducible with mercury into a laſting mercurial flux, might not ſo alter it, as to diſpoſe it to heat with gold. But this was not ſufficient to determine me to an

metallicas veſtigant, ſecreta rimarentur, illi, inquam, ſinguli ſeorſim à me rogati ingenuè apud me faſſi ſunt, ſe revera nunquam incaleſcentem ullum mercurium vidiſſe, licèt id quandoque jactatum ab alchymiſtis audiviſſent; quorum jactabundi obtentus eò minùs apud me in hoc negotio valebant, quòd à longo jam tempore notaveram, quantam fraus vel ignorantia (non enim omnes illos haberi impoſtores velim, qui hallucinantur) in nonnullis hujuſmodi, de quibus loquor, viris fiduciam parere poſſint; quæ ſanè tanta erat, ut illi, cùm eorum unus, bonæ frugis chymiſtam, multis notum multiſque operam ſuam locantem, ſuppoſito mercurio incaleſcente fefellerat, eò fiduciæ abriperentur, ut apud memet ſe ſiſterent, de experimento illo me convicturi. At, re, ut par erat, exploratâ, nullum percepi ſenſibilem illius caloris gradum, quem promiſerant.

Verùm *enimverò, his omnibus nequicquam obſtantibus, cùm ex rationibus dudum in alio ſcripto à me expoſitis, aliiſque de cauſis hic non memorandis, argentum vivum corpus reputem, quod non neceſſariò tam ſit homogeneum, ac paſſim habetur; illa mihi opinio præ cæteris allubuit, quæ mercurii incaleſcentis poſſibilitatem adſtruit. Etenim, non obſtante vulgò ſuppoſitâ mercurii (ut ſic dicam) ſimilaritate, quàm adeò eximiam eſſe puto, ut parere admirationem poſſit, meis tamen principiis conſonum erat, liquorem quendam, qui pondere, colore, totali volatilitate, &c. omnes referebat proprietates eſſentiales, quarum reſpectu corpus aliquod mercurii nomine venit, habere tamen poſſe internam ejuſmodi partium diatheſin, quæ in nonnullis hactenus non obſervatis inſignem illi à mercurio vulgari diſcrepantiam conciliare queat: atque has inter qualitates differentes neſciebam, annon ea recenſeri meritò poſſet, quâ incaleſcit cum auro commixtus. Atque hanc opinionem rationi eò magis conſentaneam arbitrabar, quòd, excogitatâ à me duplici methodo (hactenus à chymicorum nullo, quòd ſciam, in praxin verſâ,) unâ quidem, ut manifeſtum redderem, eſſetne purus curatéque diſtillatus mercurius corpus compoſitum, parteſque contineret non mercuriales; alterâ verò, ex purificato ejuſmodi & diſtillato mercurio partes ſeparandi non paucas manifeſtò heterogeneas; experiundo comperi, utramque illam methodum à me inventam ſucceſſu gaudere: id quod auctoramentum mihi haud leve erat, ut poſſibile exiſtimarem, mercurium valdè defæcatum, quin & per ſublimationes & diſtillationes repurgatum, arte poſſe eò redigi, ut aſſumat ſecúmque conflet heterogeneorum corpuſculorum multitudinem, quæ nonniſi à perito artis filio detegi, multò minus ſegregari queant (ut fieri de ſtanneis, plumbeis, &c. corpuſculis poteſt.)*

Hoc *generatim ſuffecerit, ut meum de problemate ſuprà propoſito judicium ſuſpendam, & ad experimenta ſumenda properem, quibus palàm reddatur, annon aliquæ ex particulis illis heterogeneis, quas cum mercurio in durabilem fluxum mercurialem reduci poſſe deprehendi, ita alterare eum poſſint, ut ad incaleſcendum cum auro ipſum diſponant. At non erat hoc ſatis ad eliciendum à me aſſenſum;*

an aſſent; for to oblige me to admit incaleſcent mercuries, it ought not to ſuffice, that it is poſſible, or even probable, that there may be ſuch, but there was neceſſary ſome poſitive proof, that there are ſuch; and that alſo, through God's bleſſing, my trials afforded me about the year 1652.

7. SOME years after I was in poſſeſſion of this mercury, I found in ſome of their books, that chemiſts call philoſophers, ſome dark paſſages, whence I then gueſſed their knowledge of it, or of ſome other very like it; and in one of them I found, though not all in the very ſame place, an allegorical deſcription of it, the greateſt part of which was not very difficult for me to underſtand; but not finding there any notice taken of the property of this mercury to grow hot with gold, I was induced to ſuſpect, that either they had not the knowledge of it, or judged it unfit to be ſpoken of. But you will, I ſuppoſe, expect from me rather narratives than conjectures. And, indeed, it is but reaſonable, that, having but mentioned to you a phænomenon, whoſe credibility is by many denied, I ſhould take notice of ſome circumſtances fit to bring credit to it. And I ſhall the leſs grudge the pains of ſetting down ſeveral particular phænomena, becauſe I preſume you have not met with them, and becauſe alſo it may gratify ſome of your chemical friends, who may have or diſcover ſome noble mercury, by helping them to examine it, and to try, whether it reſembles ours.

8. THAT I might not then be impoſed on by others, I ſeveral times made trial of our mercury, when I was all alone. For when no body was by me, nor probably dreamed of what I was doing, I took to one part of the mercury, ſometimes half the weight and ſometimes an equal weight of refined gold reduced to a calx or ſubtle powder. This I put into the palm of my left hand, and putting the mercury upon it, ſtirred it and preſſed it a little with the finger of my right hand, by which the two ingredients were eaſily mingled, and grew not only ſenſibly, but conſiderably hot, and that ſo nimbly, that the incaleſcence did ſometimes come to its height in about a minute of an hour by a minute-clock. I found the experiment ſucceed, whether I took altogether, or but half as much gold as mercury; but the effect ſeemed to be much greater when they were employed in equal weight. And, to obviate a ſuſpicion, which, though improbable, might poſſibly ariſe, as if the immediate contact of the ingredients and the ſkin produced a ſenſe of heat, which was not due to the action of the metals upon one another; I had the curioſity to keep the mixture in a paper, and found not its interpoſition to hinder me from feeling the incaleſcence, though it much abated the degree of my ſenſe of it.

9. I tried alſo the ſame mercury with refined ſilver reduced to a very fine powder; but I could not perceive any heat or warmth at all; though, I am apt to think, that if I had had a ſufficient quantity of leaf-ſilver to have made the experiment with, I ſhould, after ſome time,

ſum; ut enim ad mercurios incaleſcentes admittendum adducerer, ſufficere non debebat, poſſibiles eos eſſe, vel etiam probabiles, ſed reverâ tales dari manifeſtâ probatione erat evincendum: & hoc ipſum quoque, favente Deo, experimenta mea, anno 1652. circiter, comprobarunt.

POST *aliquot ab eo tempore annos, quo mercurium hujuſmodi jam poſſidebam, in quibuſdam ex eorum, quos turba chymica philoſophos nuncupat, libris abſcura quædam loca inveniebam, unde tunc eorum de ipſo, vel alio aliquo perquam ei ſimili, cognitionem conjectabam; atque in ipſorum uno reperiebam (non tamen rem totam in uno planè eodemque loco) deſcriptionem ejus allegoricam, cujus pars maxima adeò difficilis intellectu mihi non erat: at cùm nihil ibi notatum viderem de illa mercurii hujus proprietatê, quâ calorem cum auro acquirit, in ſuſpicionem incidi, eos vel cognitione illius fuiſſe deſtitutos, vel eam ſilentio premendam cenſuiſſe. At tu ſine dubio facti potiùs narrationes, quàm conjecturas à me exſpectas. Et ſanè æquum omninò fuerit, ut, cùm mentionem duntaxat fecerim phænomeni, cujus à multis negatur credibilitas, circumſtantias nonnullas annotem, quæ fidem ei conciliare valeant. Atque eò minùs laborem detrectabo particularia aliquot phænomena hìc tradendi, tum quòd ea tibi non occurriſſe autumem, tum quòd ea grata fore putem quibuſdam amicis tuis chymicis, nobilem quendam vel jam poſſidentibus vel paraturis mercurium, ut ſcil. hoc qualicunque ſcripto noſtro ad eum examinandum, &, an referat noſtrum, experiundum, juventur.*

ITAQUE, *ne mihi imponerent alii, pluries mercurium noſtrum, quando ſolus eram, explorabam. Etenim quando nemo mihi aderat, neque quiſquam per ſomnium quid agerem conjiceret, ſumebam unam partem illius mercurii, ad auri, in calcem vel pollinem redacti, pondus quandoque dimidium, quandoque æquale. Hoc polline volæ manus ſiniſtræ immiſſo, & mercurio ſuperinfuſo, utrumque ſimul agitabam, premebamque nonnihil digito manûs dextræ; quâ ratione duo hæc ingredientia facilè commixta, non modò ad ſenſum ſed inſigniter incaleſcebant, idque adeò properè, ut incaleſcentia interdum unius horæ circiter minuto, indicante idipſum horologio minutis inſtructo, ad ἀκμὴν perveniret. Succedebat hoc experimentum, ſive æqualem ſumerem ſive dimidiam auri quantitatem; effectus tamen multò videbatur inſignior, quando æquali pondere adhibebantur. Atque, ut ſuſpicioni, quæ, licèt improbabilis, ſubnaſci tamen poſſet, occurrerem, immediatum ſcil. ingredientium & cutis contactum producere poſſe ſenſum caloris, qui non debeatur metallorum in ſe invicem actioni, curioſitate ducebar mixturam hanc in charta ſervandi; quo facto, interpoſitionem ejus nequaquam impedire incaleſcentiæ ſenſum comperiebam, quanquam, ex natura rei, intenſiorem illius gradum remitteret.*

PORRÒ *mercurium eundem cum repurgato argento, ad ſubtilem valdè pulverem redacto, exploravi; at nullum omnino calorem percipere potui; quanquam eò ferar, ut exiſtimem, ſi ſufficiens argenti foliati quantitas, ad peragendum experimentum, mihi ſuppetiiſſet, me poſt aliquot tem-*

have produced an incaleſcence, though much inferior to what the ſame quantity of mercury would produce with gold; but this only upon the by. I ſhall now add, that to the end I might not be thought to impoſe upon myſelf, I did not make trial in my own hand, when it was in different tempers, as to heat and cold, but I did it in the hands of others, who were not a little ſurpriſed and pleaſed at the event. And this I did more than once or twice; by which means I had, and ſtill have, divers witneſſes of the truth of the experiment, whereof ſome are noted perſons, and eſpecially him, to whom I laſt ſhewed it, which you will eaſily believe, when I tell you it is the learned ſecretary of the Royal Society; to whom having given the ingredients, I deſired him to make the experiment in and with his own hands, in which it proved ſucceſsful within ſomewhat leſs than a minute of an hour*.

10. And that, which makes this incaleſcence the more conſiderable is, that being willing to huſband my mercury, a great part of which had been, as I gueſſed, ſtolen from me before I employed it, I made theſe trials but with a drachm at a time, which ſcarce amounts in quantity to the bigneſs of half a middle-ſized bean; whereas, if I could have made the experiment with a ſpoonful or two of quickſilver, and a due proportion of gold, it is probable the heat would have been intenſe enough, not only to burn one's hand, but perchance to crack a glaſs vial; ſince I have ſometimes had of this mercury ſo ſubtle, that when I employed but a drachm at a time, the heat made me willing to put it haſtily out of my hand.

11. These things being matters of fact, I ſcruple not to deliver them; but I would much ſcruple to determine thence, whether thoſe, that are *mercurii corporum*, and were made, as chemiſts preſume, by extraction only from metals and minerals, will each of them grow hot with gold, as, if I much miſtake not, I found antimonial mercury to do. And much leſs would I affirm, that every metalline mercury (though never ſo diſpoſed to incaleſcence) or even that of ſilver or gold itſelf, is the ſame with that, which the chryſopæan writers mean by their philoſophic mercury, or is near ſo noble as this. Nay, I would not ſo much as affirm, that every mercury, obtained by extraction even from the perfect metals themſelves, muſt needs be more noble and fit (as alchemiſts ſpeak) for the philoſophic work, than that, which may with ſkill and pains be at length obtained from common mercury ſkilfully freed from its recrementitious and heterogeneous parts, and richly impregnated with the ſubtle and active ones of congruous metals or minerals. Theſe and the like points I ſhould, as I was ſaying, much ſcruple at offering to determine in this place, where what I deſigned to deliver was hiſtorical, though I have not thought it impertinent to glance at the points lately mentioned, becauſe thoſe glances may intimate things conducive to the better underſtanding of what I have ſaid, and have to ſay in this paper.

poris ſpatium incaleſcentiam ſuſcitaturum fuiſſe, quamvis multò inferiorem eo, quem eadem mercurii quantitas cum auro produceret: at hoc nonniſi in tranſitu. Adjiciam nunc, me, ne mihimet impoſuiſſe cenſerer, non tantùm rem hanc exploraſſe in manu mea, quando variè erat pro caloris & frigoris ratione temperata, ſed & in manibus aliorum, quos non parùm attonitos habebat, juvabatque eventus. Atque hoc ipſum pluries quàm ſemel biſve feci; unde mihi teſtes ſuppetunt experimenti veritatis aſſertores, probatæ fidei viri, quorum unus erat eruditus Societatis Regiæ ſecretarius, quem, exhibitis ei ingredientibus, rogabam, ut ſuiſmet manibus experimentum caperet; in quibus & optatum ſucceſſum minori quàm unius minuti ſpatio ſortiebatur †.

Atque, *quod incaleſcentiam hanc inſigniorem reddit, eſt, quòd, cùm parcè uti mercurio meo cuperem, quippe cujus magna pars (ut conjicio) ſurrepta mihi fuerat, priuſquam eum adhiberem, experimenta ſingula nonniſi cum una drachma peragebam, quæ vix fabæ mediocris dimidiæ magnitudinem æquat, cùm, ſi copia mihi fuiſſet capiendi experimentum cum cochleari uno alteróve mercurii pleno, ſupparique quantitate auri, probabile ſit, calorem inde oriturum fuiſſe ſatis intenſum, ut non modò ureret manum, ſed forſan & in phiala vitrea rimas ageret; quandoquidem interdum hujus generis mercurium habui adeò ſubtilem, ut, adhibente me ſingulis vicibus nonniſi drachmam unam, calor me adegerit, ut properè è manibus mixturam deponerem.*

Hæc, *cùm ſint res facti, tradere non dubito; at valdè ambigerem exinde determinare, num, qui appellantur mercurii corporum, paranturque, ut jactant chymici, ſola extractione ex metallis & foſſilibus, eorum quilibet calorem acquirat cum auro, quemadmodum, ni multum fallor, mercurium antimonialem acquirere comperi. Multóque minus affirmarem, quemvis mercurium metallicum (quantumcumque ad incaleſcentiam diſpoſitum,) quin & mercurium argenti aurive ipſius eundem eſſe cum eo, quem ſcriptores chryſopæi per mercurium ſuum philoſophicum intelligunt, vel præſtantiâ ſuâ ad hunc accedere. Quin imò, ne quidem aſſererem, quemlibet mercurium, extractione etiam ab ipſis perfectis metallis impetratum, nobiliorem eſſe oportere, & (ut loquuntur alchymiſtæ) ad philoſophicam operatiorem magis idoneum, quàm illum, qui, peritiâ & induſtriâ comite, obtineri tandem poteſt à mercurio vulgari, à partibus ſuis recrementitiis heterogeneiſq; purgato, ſubtilibuſque & efficacibus metallorum mineraliumve congruorum partibus uberrime fæto: hæc, inquam, & ſimilia hoc loco affirmare admodum vererer; cùm hîc nonniſi ea tradere inſtituerim, quæ ad rei hiſtoriam faciunt; quanquam præter rem non exiſtimaverim, jamjam indigitatos rei hujus apices innuere, quòd ſtricturæ iſtæ ea poſſint lectori ingerere, quæ ad meliorem tum dictorum tum dicendorum intelligentiam conducere queant.*

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* Since this was written, the noble and judicious preſident of the Royal Society, the lord viſcount *Brouncker*, made the ſame experiment with ſome of the ſame mercury, in his own hand with good ſucceſs.

† *Ex quo tempore hoc literis fuit conſignatum, illuſtriſſimus & judicioſiſſimus Regiæ Societatis præſes, Dom. Vicecomes* Brouncker, *idem experimentum ſuâ cum ejuſdem mercurii portione manu cum ſucceſſu peregit.*

12. I doubt not but what I have related and hinted has given you a curioſity to know ſomewhat further of this mercury: and I confeſs, that if there be any truth in what ſome of the moſt approved Spagyriſts have delivered about a ſolvent of gold, that ſeems of kin, and perhaps is not much nobler than one, that I had; it ſeems allowable to expect, that even ours ſhould be of more than ordinary uſe, both in phyſick and alchemy. But the misfortune I had to have loſt a conſiderable quantity of it, being afterwards increaſed by the almoſt ſudden death of the only operator I truſted in the making of it; I was altogether diſcouraged from repeating ſuch a troubleſome preparation, eſpecially being diverted by buſineſs, removes, ſickneſs, and more pleaſing ſtudies. And though I have not forgot ſome not deſpicable trials, that I made with our mercury, yet ſince they are not neceſſary to the queſtion, that occaſioned this paper, I ſhall paſs them over in ſilence, and only obſerve ſome few things I had almoſt forgot to tell you; namely, firſt, that whereas it is uſual to take four, five, or ſix, nay eight or ten parts of common quickſilver, to make an amalgame with one of gold, even when both are heated by the fire; I found our mercury ſo congruous to that metal, that it would preſently imbody with no leſs than an equal weight of it, and produce a pretty hard amalgame or mixture, in which the mercury was ſo diffuſed, that the gold had quite loſt its colour. Secondly, I ſhall add what, for aught I know, has not been yet obſerved, that this power of penetrating gold and growing hot with it, is ſo inherent, not to ſay radicated, in our mercury, that after it had been diſtilled from gold again and again, I found it to retain that property. And, laſtly, whereas it may be ſuſpected, that this faculty may be quickly loſt, (as that of the prepared Bononian ſtone to receive light, has been complained of as not durable) I found by trial, that a ſingle drachm of mercury, made after a certain manner, did, the third or fourth year after I had laid it by, grow ſo hot with gold, that I feared it would have burnt my hand.

THUS far the author to his friend: but when he ſent me the paper, he accompanied it with the following lines;

13. I have little at preſent to ſay to you about the papers, which this ſheet accompanies, ſave that one of the chief reaſons, that makes me backward to have the foregoing obſervations communicated to the curious, is, that I fear, we may thereby procure divers queries and perhaps requeſts, (relating to this mercury) which I would by all means avoid, for divers reaſons, and particularly for this, that a great weakneſs of that part diſables me to write with my own hand, and I know, you will not think it fit I ſhould, about ſuch a ſubject, employ that of an amanuenſis. And therefore I cannot conſent, this paper ſhould go out of your hands, unleſs you can think on ſome likely courſe to ſecure me from trouble, and from the unwelcome neceſſity of diſobliging ſome, whilſt I endeavour to gratify others. If this precaution be uſed, I may ſafely learn, by means of

NON dubito, quin hactenus à me enarrata indigitataque curioſitatem in te pepererint, aliquid amplius de hoc mercurio cognoſcendi: & fateor, ſi quid veri ſubſit ei, quod quidem ex probatiſſimis ſpagyricis de quodam auri diſſolvente, quod affine videtur noſtro, nec eo fortè multò eſt nobilius, tradiderunt; exſpectare fas fuerit, ipſiſſimum hoc noſtrum in inſignem, cum in medicina, tum in alchymia, uſum cedere poſſe. Verùm cum infortunium illud, quo inſigniori quantitate ejus fui privatus, ſtipatum fuerit ſubitâ morte operatoris unici, cui in eo parando penitùs fidebam, animum planè alienum ab iteranda tam moleſta præparatione ſenſi; maximè cùm occupationes, migrationes, adverſa valetudo, ſtudiaque gratiora aliorſum me traherent; & licèt experimenta quædam non ſpernenda, cum mercurio noſtro peracta, memoriâ meâ non exciderint; cùm tamen ad quæſtionem illam, quæ ſcriptum hoc peperit, non ſint neceſſaria, ſilentio ea involvam, paucula duntaxat annotaturus, quæ commemorare propemodum fuiſſem oblitus. Quorum primum eſt, quod, cùm ſolenne ſit capere mercurii vulgaris partes quatuor, 5 vel 6, imò 8 vel 10, ad amalgama faciendum cum una parte auri, etiam tum, quando utrumque incaluit igne; ego adeò congruum deprehenderim cum metallo illo mercurium noſtrum, ut non minus quàm æquale illius pondus intimè ſtatim pervaderet, ſatiſque durum amalgama cramàve produceret, in quo adeò diffuſus erat mercurius, ut aurum colorem ſuum penitus amitteret. Secundum eſt, (quod hactenus obſervatum fuiſſe haud putem,) vim ſcil. hanc, aurum penetrandi, cùmque eo incaleſcendi, mordicùs adeò inhærere mercurio noſtro, ne dicam ita in eo radicatam eſſe, ut poſtquam iterum atque iterum ab auro eſſet diſtillatus, proprietatis illius tenacem eum deprehenderim. Et denique, cùm ſuſpicio inceſſere lectorem poſſit, facultatem hanc citò deperdi, (ut de præparato ad hauriendam lucem lapide Bononienſi queruntur authores) experiundo didici, unicam drachmam mercurii, certo modo parati, poſt tertium quartúmve à quo ſepoſueram annum adeò cum auro incaluiſſe, ut ne adureret manum meam, timerem.

HACTENUS *author noſter ad amicum ſuum: ſed cùm mihi chartas illas mitteret, voluit eas ſequenti mantiſſâ locupletare;*

NON *diu te morabor diſſerendo de chartis hîc junctis: dicam ſolummodò, unam ex præcipuis rationibus, quæ in vulgandis prægreſſis obſervationibus cunctabundum me faciunt, hanc eſſe, quòd vereor, nos hoc ipſo variis circa mercurium hunc quæſtionibus & fortè ſollicitationibus anſam daturos, quas omni ſtudio præcavere velim, cum ob alias, tum hanc ob cauſam, quòd magna manuum mearum debilitas me impedit, quò minus meamet manu id conſignare literis valeam, quod conſcribi amanuenſis operâ conſultum haud judicaveris. Proindéque concedere haud poſſum, ſcriptum hoc è manibus tuis dimitti, niſi rationem ſuggeras probabilem, quâ ſecurum me præſtes à moleſtia, atque ab ingrata neceſſitate repulſam dandi nonnullis, dum aliis obſecundare ſtudeo. Hac cautelâ ſi utaris, potero ampliſſimæ tuæ conſuetudinis beneficio citra moleſtiam edoceri, quid ii, qui tantâ peritiâ tantoque judicio*

your diffuſed acquaintance, what thoſe, that are ſkilful and judicious enough to deſerve to be much conſidered in ſuch an affair, will think of our mercury, and whether, in caſe they have an eſteem of it approaching to that of divers eminent chemiſts (ſome of which importune me to impart it;) they judge the good, that the preparations of it (ſuch as precipitats and turbiths of divers kinds, mercurius dulcis, cinaber made of the ſulphur of antimony, and with gold, &c.) may do in phyſick, is likely much to exceed the political inconveniencies, that may enſue, if it ſhould prove to be of the beſt kind, and fall into ill hands. The knowledge of the opinions of the wiſe and ſkilful about this caſe will be requiſite to aſſiſt me to take right meaſures in an affair of this nature. And, till I receive this information, I am obliged to ſilence.

14. ONLY, in the mean while, I ſhall, for the ſake of the enquiries into the mercurial arcana, make bold to add a ſecret, which, I think, will to divers philalethiſts and other ſtudents of the chemical philoſophers books ſeem a paradox, if not an untruth; namely, that a mercury, qualified to heat with gold, and perhaps with other powders, may be made by more ways than one or two; experience having aſſured me (whatever authorities or theories may be urged to the contrary) that ſuch a mercury may be (I ſay not, eaſily or ſpeedily, but ſucceſsfully) prepared, not only by employing antimony and ſolid metals, as mars, but without any ſuch metal at all, or ſo much as antimony itſelf.

15. HERE I purpoſed to conclude: but, becauſe I am, as you know, very averſe (which I declare myſelf to be on this occaſion alſo) from making any promiſe to the publick, I think fit in this place to give you an advertiſement, and obviate a ſcruple. I ſhall therefore admoniſh thoſe inquiſitive Spagyriſts, that may be deſirous to try, whether their purified mercury be incaleſcent, that they be not too haſty to conclude it is not ſo; nor to reject it, unleſs they have made the trial with gold duly prepared. For I have found, that my mercury did not grow hot with the ſmalleſt filings of gold I could make (though indeed within a few hours after it did, without the help of fire, imbody with it into a hard amalgama,) which argued, that the corpuſcles of the metal were not yet ſmall enough to be ſuddenly penetrated by the quickſilver: nor will every calx of gold ſerve our turn, as I have found by employing, without ſucceſs, a very fine and ſpongy calx made after an uncommon way, the golden particles having, as it ſeemed, ſome extremely fine, though unobſerved duſt of the additament ſticking to them, which hindered the adhæſion of the mercurial ones. Now, the calx of gold, that I moſt uſed, as finding it ſtill to do well, was that made by quartation [*], as alchemiſts call it. But becauſe it is not ſo eaſy, as even chemiſts, that have not tried, imagine, to make good

valent, ut in hoc negotio magni fieri mereantur, de mercurio noſtro ſentiant; adhæc utrum, ſi æſtimationem de eo foveant illi ſupparem, quam præcellentium chymicorum complures (quorum nonnulli me urgent ad eum communicandum) præ ſe ferunt, veriſimile cenſeant, utilitatem, quam præparationes ipſius (cujuſmodi ſunt præcipitata & turbithi diverſorum generum, mercurius dulcis, cinnabaris ex antimonio & auro parata, &c.) afferre poſſint rei medicæ, longè ſuperaturam eſſe incommoda illa politica, quæ naſcitura forent, ſi fortè de præſtantiſſima eſſet indole, atque in maleferiatas manus incideret. Sapientum & peritorum hoc in caſu opiniones cognoſcere, neceſſarium mihi fuerit, ut recto tramite in iſtiuſmodi negotio incedere mihi detur. Atque, donec edoctus id fuero, ſilentii ſacra colere teneor.

INTERIM *in eorum gratiam, qui arcana mercurialia ſcrutantur, ſubjungere auſim ſecretum aliquod, quod philalethis compluribus, aliiſque, qui chymicorum philoſophorum libris meditandis incumbunt, paradoxum, quin & falſum fortè videbitur: mercurium ſcil. ad incaleſcendum cum auro aliiſve pulveribus idoneum, modis uno binóve pluribus parari poſſe; cùm per experientiam certò mihi conſtet, (quicquid in contrarium obtendant authoritates & theoriæ) talem mercurium poſſe, (non dicam facilè properéve, ſed cum ſucceſſu) parari, non modò antimonium ſolidáque metalla, putà martem, &c. adhibendo, ſed citra ullius omnino metalli, quin vel ipſius antimonii, uſum.*

HIC *ſtatueram finem huic ſermoni imponere: at cùm ægerrimè, ut noſti, tum aliàs, tum hac imprimis occaſione, promiſſi fidem publico obſtringam, viſum eſt mihi hoc loco monitum aliquod ſuggerere, & ſcrupulo cuidam obviam ire. Prius quod attinet, curioſos illos Spagyricos, quos fortè tentandi cupido inceſſerit, ſitne purgatus ipſorum mercurius incaleſcendi qualitate inſtructus, monebo, ne nimis feſtinantur concludant ipſum eâ præditum non eſſe, nève eum rejiciant, niſi experimentum fecerint cum auro ritè præparato. Comperi quippe, mercurium meum non incaleſcere cum ramentis auri, omnium quas conficere poteram minimis, (quanquam reverâ intra paucas exinde horas, ſine ignis adminiculo, cum ipſo in durum amalgama conflaretur;) quod argumento erat, metalli illius corpuſcula necdum exigua ſatis fuiſſe, ut properè à mercurio penetrarentur: neque quævis auri calx rem noſtram conficiet; ut comperi, dum perquàm ſubtilem ſpongioſamque calcem, modo non vulgari paratam, citra ſucceſſum adhibui, in qua, ut videtur, apprimè tenuis ſenſumque fugiens additamenti pulvis adhærebat particulis aureis, & mercurialium adhæſionem præpediebat. Jam verò calx auri, quâ plerumque utebar ſucceſſu ejus inductus, illa erat, quæ quartationis [†] (ut vocant) beneficio paratur. At quia non adeò facilè eſt, ut ipſi chymici, qui manum operi non admoverunt, ſibi imaginantur, bonæ notæ calces auri parare, cumque in*

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[*] That is, by melting together one part of fine gold, and three or four parts of cuppelled ſilver, and then putting the maſs, wherein the metals are mixed, almoſt *per minima*, into purified aqua-fortis, which diſſolving the ſilver only, leaves the gold in the form of a fine calx.

[†] *Hoc eſt, per fuſionem conflando unam partem auri puri, & tres quatuórve partes argenti cupellati, ut vocant, & tunc immittendo maſſam, in qua metalla miſcentur quaſi per minima, in purgatam aquam fortem, quæ ſolum argentum diſſolvens, aurum in forma calcis relinquit.*

good cakes of gold, and that in the way newly mentioned, there needs fuſion of gold and of ſilver (for which many chemiſts want conveniencies,) and they are often impoſed on by common refiners, who here uſually ſell in wires ſuch ſilver for fine (which indeed it is comparitively,) as I have found not to be without mixture; I ſhall add, that by making an amalgama the common way, with pure gold and vulgar mercury, and diſſolving the mercury in good aqua-fortis, there will remain a powder, which, being well waſhed with fair water to dulcify it, and kept a while in a moderate fire to dry it thoroughly without melting it, will become a calx, which I have more than once uſed with our mercury with good ſucceſs. It is true, both in this way and in that (by quartation) aqua-fortis, which is a corroſive liquor, is employed to bring the gold to powder, and therefore in a diffident mind ſome ſuſpicion may ariſe, that the incaleſcence may proceed only from the action of the acid particles of the menſtruum, which yet adhering to the corpuſcles of the gold works upon the quickſilver, as aqua-fortis is known to do: but, to omit thoſe anſwers, that cannot be given in few words, after I have taken notice, that, if the effect depends not on our mercury (as prepared) but only on the calx, it appears not, why this ſhould not grow hot with common mercury, as well as with ours; I ſhall need to add, for the removal of this ſubtile ſcruple, no more than this plain experiment, (which I twice or thrice made,) namely, that taking, inſtead of a calx of gold, a competent number of leaves of gold, ſuch as book-binders and the apothecaries uſe, this gold, that was without the help of ſalts reduced by beating to a ſufficient thinneſs (inſomuch that ſeventy odd leaves did not weigh a ſcruple,) I found (more than once) upon putting two or three times the weight of our mercury to them, that a ſmart heat was preſently produced in my hand.

in methodo jamjam memorata requiratur fuſio auri & argenti (cujus peragendæ commoditate non pauci chymici deſtituuntur,) cùm aliàs crebrò à vulgaribus metallorum purgatoribus fallantur, qui hîc paſſim, filorum formâ, ejuſmodi argentum pro puro venditant (quale, comparatè loquendo, reverà eſt,) quod non eſſe mixturæ expers deprehendi; adjiciam, quòd, dum communi more amalgama conficitur cum auro puro & mercurio vulgari, mercuriuſque diſſolvitur bonâ aquâ forti, remanſurus ſit pulvis, qui cum aqua pura, ad conciliandam ei, quam vocant, dulcedinem, probè elotus, & aliquandiu in temperato igne, ad eum penitus exiccandum citra fuſionem, aſſervatus, talem calcem præbebit, quâ pluries cum mercurio noſtro feliciter uſus fui. Fateor equidem, tum in hac methodo, tum in illa, quæ inſtituitur per quartationem, adhiberi aquam fortem, liquorem ſcil. corroſivum, ad aurum in pulverem redigendum, unde ſcrutanti genio ſuboriri ſuſpicio poterit, incaleſcentiam illam ſoli actioni acidarum particularum menſtrui acceptam ferendam eſſe, quod hærens etiamnum auri corpuſculis, in mercurium operatur, ſolenni aquæ fortis more. Verùm, (ut eas reſponſiones ſileam, quæ paucis tradi non poſſunt,) poſtquam notavi, quòd, ſi effectus hic non dependet à mercurio noſtro (ritè præparato,) ſed à ſola calce, non pateat, quare hæc non incaleſcat æquè cum mercurio vulgari ac noſtro; opus haud fuerit, aliud quicquam ad ſcrupulum hunc eximendum, quàm obvium hoc experimentum, quod ſequitur, quodque bis terve à me paractum fuit, adjicere: ſumpſi, inquam, calcis auri loco, ſufficientem numerum foliorum auri, qualibus utuntur bibliopegi & aurifabri; hoc aurum, quod citra ſalium opem tundendo redactum erat ad tenuitatem ſufficientem (adeò ut ultra ſeptuaginta folia vix unius ſcrupuli pondus æquarent,) hoc, inquam, aurum comperi (unâ vice pluries,) cùm binum trinumve mercurii noſtri pondus ipſi commiſcerem, inſignem in manu mea calorem mox peperiſſe.

EXPERIMENTS, NOTES, &c.
ABOUT THE
MECHANICAL ORIGIN OR PRODUCTION
OF
Divers Particular QUALITIES:
Among which is inſerted
A Diſcourſe of the Imperfection of the Chemist's Doctrine of Qualities;
TOGETHER WITH
Some Reflections upon the Hypotheſis of Alkali and Acidum.

The PUBLISHER to the READER.

TO keep the reader from being at all ſurprized at the date of the title-page, I muſt inform him, that a good part of the enſuing tracts were printed off, and in my cuſtody the laſt year; and the reſt had come out with them divers months ago, if the noble author had not been hindered from committing them to the preſs by the deſire and hope of being able in a ſhort time to ſend them abroad more numerous, and by his being hindered to do ſo, partly by remove, partly by the want of ſome papers, that were oddly loſt or ſpoiled, and partly by the ſickneſs of himſelf, and divers of his near relations. And ſome of theſe impediments do yet ſuppreſs what the author intended ſhould have made a part of the book, which now he ſuffers to be publiſhed without them, though divers of his papers about ſome other particular qualities have been written ſo long ago, as to have lain for many years neglected among other of his old writings: which that he may have both leiſure and health to review and fit for publication, is the ardent wiſh of the ſincere lovers of real knowledge, who have reaſon to look on it as no mean proof of his conſtant kindneſs to experimental philoſophy, that in theſe tracts he perſeveres in his courſe of freely and candidly communicating his experiments and obſervations to the publick, notwithſtanding the liberty, that hath been too boldly taken to mention them as their own by later writers; as particularly by the compiler of the treatiſe, entitled *Polygraphice*, who in two chapters hath allowed himſelf to preſent his reader with above fifty experiments, taken out of our author's book of colours, without owning any of them to him, or ſo much as naming him or his book in either of thoſe chapters, nor, that I remember, in any of the others. Nor did I think this practice juſtified by the confeſſion made in the preface, importing, that the compiler had taken the particulars he delivered from the writings of others. For this general and perfunctory acknowledgement neither doth right to particular authors, nor, by naming them, enables the reader to know, whether the things delivered come from perſons fit to be credited or not; and therefore, ſince it is but too likely, that ſuch concealment of the names, if not uſurpation of the labours of the benefactors to philoſophy, will prove much more forbidding to many others to impart their experiments, than as yet they have to our generous author; it ſeems to be the intereſt of the commonwealth of learning openly to diſcountenance ſo diſcouraging

couraging a practice, and to ſhew, that they do not think it fit, that poſſeſſors of uſeful pieces of knowledge ſhould be ſtrongly tempted to envy them to the publick, to the end only that a few compilers ſhould not be put upon ſo reaſonable and eaſy a work, as by a few words or names to ſhew themſelves juſt, if not grateful.

But not to keep the reader any longer from the peruſal of theſe tracts themſelves, I ſhall conclude with intimating only, that what our author ſaith in one of them concerning the inſufficiency of the chemical hypotheſis for explaining the effects of nature, is not at all intended by him to derogate from the ſober profeſſors of chemiſtry, or to diſcourage them from uſeful chemical operations; foraſmuch as I had the ſatisfaction, ſome years ſince, to ſee in the author's hands a diſcourſe of his about the Uſefulneſs of Chemiſtry for the Advancement of Natural Philoſophy; with which alſo it is hoped he will ere long gratify the publick.

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TO obviate ſome miſapprehenſions, that may ariſe concerning the enſuing notes about particular qualities, it may not be improper to add ſomething in this place to what has been ſaid in another paper * in reference to thoſe notes, and conſequently to premiſe to the particular experiments ſome few general advertiſements about them.

And I. we may conſider, that there may be three differing ways of treating hiſtorically of particular qualities. For either one may in a full and methodical hiſtory proſecute the phænomena; or one may make a collection of various experiments and obſervations, whence may be gathered divers phænomena to illuſtrate ſeveral, but not all of the heads or parts of ſuch an ample or methodical hiſtory; or, in the third place, one may in a more confined way content one's ſelf to deliver ſuch experiments and obſervations of the production, or the deſtruction or change of this or that quality, as being duly reaſoned on, may ſuffice to ſhew, wherein the nature of that quality doth conſiſt, eſpecially in oppoſition to thoſe erroneous conceits, that have been entertained about it. Of the firſt of theſe three ways of treating of a quality I pretend not to have given any complete example; but you will find, that I have begun ſuch hiſtories in my ſpecimens about fluidity and firmneſs, and in the experiments, obſervations, &c. that I have put together about cold. The ſecond ſort of hiſtorical writings I have given an inſtance of in my experiments about colours; but in theſe enſuing notes, the occaſion I had to make them, having obliged me chiefly to have an eye to the diſproval of the errors of the peripateticks and the chemiſts about them, I hope I ſhall not be thought to have fallen very ſhort in my attempt, if I have (here and there) performed, what may be required in the third way of writing hiſtorically of a quality; my preſent deſign being chiefly to give an intelligent and hiſtorical account of the poſſible mechanical origination, not of the various phænomena of the particular qualities ſuccinctly mentioned in theſe notes; though my ſecondary end being to become a benefactor to the hiſtory of qualities by providing materials for myſelf or better architects, I have not ſcrupled to add to thoſe, that tend more directly to diſcover the nature or eſſence of the quality treated of, and to derive it from mechanical principles, ſome others (which happened to come in my way) that acquaint us but with ſome of the leſs luciferous phænomena.

II. That you may not miſtake what is driven at in many of the experiments and reaſonings delivered or propoſed in the enſuing notes about particular qualities, I muſt deſire you to take notice with me, what it is, that I pretend to offer you ſome proofs of. For if I took upon me to demonſtrate, that the qualities of bodies cannot proceed from (what the ſchools call) ſubſtantial forms, or from any other cauſes but mechanical, it might be reaſonably enough expected, that my argument ſhould directly exclude them all. But ſince, in my explications of qualities, I pretend only, that they may be explicated by mechanical principles, without enquiring, whether they are explicable by any other; that, which I need to prove, is, not that mechanical principles are the neceſſary and only things, whereby qualities may be explained, but that probably they will be found ſufficient for their explication. And ſince theſe are confeſſedly more manifeſt and more intelligible, than ſubſtantial forms and other ſcholaſtic entities (if I may ſo call them) it is obvious, what the conſequence will be of our not being obliged to have recourſe to things, whoſe exiſtence is very diſputable, and their nature very obſcure.

There are ſeveral ways, that may be employed, ſome on one occaſion, and ſome on another, either more directly to reduce qualities (as well as divers other things in nature) to mechanical principles; or, by ſhewing the inſufficiency of the Peripatetick and chemical theories of qualities, to recommend the Corpuſcularian doctrice of them.

For further illuſtration of this point, I ſhall add on this occaſion, that there are three diſtinct ſorts of experiments (beſides other proofs) that may be reaſonably employed, (though they be not equally efficacious) when we treat of the origin of qualities. For ſome inſtances may be brought to ſhew, that the propoſed quality may be mechanically introduced into a portion of matter, where it was not before.

Other

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* See Tracts about Coſmical Qualities, &c. to which is prefixed an Introduction to the Hiſtory of Particular Qualities.

Other inſtances there may be to ſhew, that by the ſame means the quality may be notably varied as to degrees, or other not eſſential attributes. And by ſome inſtances alſo it may appear, that the quality is mechanically expelled from, or aboliſhed in, a portion of matter, that was endowed with it before. Sometimes alſo by the ſame operation the former quality is deſtroyed, and a new one is produced. And each of theſe kinds of inſtances may be uſefully employed in our notes about particular qualities. For as to the firſt of them, there will be ſcarce any difficulty. And as to the ſecond, ſince the permanent degrees, as well as other attributes of qualities are ſaid to flow from (and do indeed depend upon) the ſame principles, that the quality it ſelf does; if, eſpecially in bodies inanimate, a change barely mechanical does notably and permanently alter the degree or other conſiderable attribute; it will afford, though not a clear proof, yet a probable preſumption, that the principles, whereon the quality it ſelf depends; are mechanical. And laſtly, if, by a bare mechanical change of the internal diſpoſition and ſtructure of a body, a permanent quality, confeſſed to flow from its ſubſtantial form, or inward principle, be aboliſhed, and, perhaps, alſo immediately ſucceeded by a new quality mechanically producible; if, I ſay, this come to paſs in a body inanimate, eſpecially, if it be alſo, as to ſenſe ſimilar, ſuch a phænomenon will not a little favour that hypotheſis, which teaches, that theſe qualities depend upon certain contextures, and other mechanical affections of the ſmall parts of the bodies, that are endowed with them, and conſequently may be aboliſhed when that neceſſary modification is deſtroyed. This is thus briefly premiſed to ſhew the pertinency of alledging differing kinds of experiments and phænomena, in favour of the corpuſcular hypotheſis about qualities.

WHAT has been thus laid down, may, I hope, facilitate and ſhorten moſt of the remaining work of this preamble, which is to ſhew, though but very briefly, that there may be ſeveral ways, not impertinently employable to recommend the corpuſcularian doctrine of qualities.

FOR firſt, it may ſometimes be ſhewn, that a ſubſtantial form cannot be pretended to be the neceſſary principle of this or that quality; as will, for inſtance, hereafter be made manifeſt in the aſperity and ſmoothneſs of bodies, and in the magnetical virtue, reſiding in a piece of iron, that has been impregnated by a loadſtone. It is true, that the force of ſuch inſtances is indirect, and that they do not expreſly prove the hypotheſis, in whoſe favour they are alledged; but yet they may do it good ſervice, by diſproving the grounds and concluſions of the adverſaries, and ſo (by removing prejudices) making way for the better entertainment of the truth.

SECONDLY, we may ſometimes obtain the ſame, or the like quality, by artificial and ſometimes even temporary compoſitions, which, being but factitious bodies, are by learned adverſaries confeſſed, not to have ſubſtantial forms, and can indeed reaſonably be preſumed to have but reſulting temperaments: as will be hereafter exemplified in the production of green by compounding blue and yellow, and in the electrical faculty of glaſs; and in the temporary whiteneſs produced by beating clear oil and fair water into an ointment, and by beating water into a froth, and, more permanently, in making coral white by flawing it with heat; and in divers other particulars, that will more properly be elſewhere mentioned.

THIRDLY then, in ſome caſes the quality propoſed may be either introduced, or varied, or deſtroyed, in an inanimate body, when no change appears to be made in the body, except what is mechanical, and what might be produced in it, ſuppoſing ſuch a parcel of matter were artificially framed and conſtituted as the body is, though without any ſubſtantial form, or other ſuch like internal principle. So when a piece of glaſs, or of clarified roſin, is, by being beaten to powder, deprived of its tranſparency, and made white, there appears no change to be made in the pulverized body, but a comminution of it into a multitude of corpuſcles, that by their number, and the various ſituations of their ſurfaces are fitted copiouſly to reflect the ſincere light ſeveral ways, or give ſome peculiar modification to its rays; and hinder that free paſſage of the beams of light, that is requiſite to tranſparency.

FOURTHLY, as in the caſes belonging to the foregoing number there appears not to intervene in the patient or ſubject of the change, any thing but a mechanical alteration of the mechanical ſtructure or conſtitution; ſo in ſome other caſes it appears not, that the agent, whether natural or factitious, operates on the patient, otherwiſe than mechanically, employing only ſuch a way of acting, as may proceed from the mechaniſm of the matter, which itſelf conſiſts of, and that of the body it acts upon. As when goldſmiths burniſh a plate or veſſel of ſilver, that having been lately boiled looked white before, though they deprive it of the greateſt part of its colour, and give it a new power of reflecting the beams of light and viſible objects, in the manner proper to ſpecular bodies; yet all this is done by the intervention of a burniſhing tool, which often is but a piece of ſteel or iron conveniently ſhaped; and all that this burniſher does, is but to depreſs the little prominencies of the ſilver, and reduce them, and the little cavites of it, to one phyſically level or plain ſuperficies. And ſo when a hammer ſtriking often on a nail, makes the head of it grow hot, the hammer is but a purely mechanical agent, and works by local motion. And when by ſtriking a lump of glaſs, it breaks it into a multitude of ſmall parts, that compoſe a white powder, it acts as mechanically in the production of that whiteneſs, as it does in driving in a nail to the head. And ſo likewiſe, when the powdered glaſs, or colophony lately mentioned, is, by the fire, from a white and opacous body, reduced into a colourleſs (or a reddiſh) and tranſparent one, it appears not, that the fire, though a natural agent, need work otherwiſe than mechanically, by colliquating the incoherent grains of powder into one maſs; wherein, the ranks of pores not being broken and interrupted as before, the incident beams of light are allowed every way a free paſſage through them.

Fifthly, the like phænomena to thoſe of a quality to be explicated, or at leaſt as difficult in the ſame kind, may be produced in bodies and caſes, wherein it is plain we need not recur to ſubſtantial forms. Thus a varying colour, like that, which is admired in a pigeon's neck, may be produced in changeable taffety, by a particular way of ranging and connecting ſilk of ſeveral colours into one piece of ſtuff. Thus we have known opals caſually imitated and almoſt excelled by glaſs, which luckily degenerated in the furnace. And ſomewhat the like changeable and very delightful colour I remember to have introduced into common glaſs, with ſilver, or with gold and mercury. So likewiſe merely by blowing fine cryſtal-glaſs, at the flame of a lamp, to a very extraordinary thinneſs, we have made it to exhibit, and that vividly, all the colours (as they ſpeak) of the rainbow; and this power of pleaſing by diverſifying the light, the glaſs, if well preſerved, may keep for a long time. Thus alſo by barely beating gold into ſuch thin leaves, as artificers and apothecaries are wont to employ, it will be brought to exhibit a green colour, when you hold it againſt the light, whether of the day, or of a good candle; and this kind of greenneſs, as it is permanent in the foliated gold, ſo I have found by trial, that if the ſun-beams, ſomewhat united by a burning-glaſs, be trajected through the expanded leaf, and caſt upon a piece of white paper, they will appear there, as if they had been tinged in their paſſage. Nay, and ſometimes a ſlight and almoſt momentary mechanical change will ſeem to over-rule nature, and introduce into a body the quite oppoſite quality to that ſhe had given it: as when a piece of black horn is, only by being thinly ſcraped with the edge of a knife, or a piece of glaſs, reduced to permanently white ſhavings. And to theſe inſtances of colours, ſome emphatical, and ſome permanent, might be added divers belonging to other qualities, but that I ought not to anticipate what you will elſewhere meet with.

There is yet another way of arguing in favour of the Corpuſcularian doctrine of qualities, which, though it do not afford direct proofs of its being the beſt hypotheſis, yet it may much ſtrengthen the arguments drawn from other topicks, and thereby ſerve to recommend the doctrine itſelf. For, the uſe of an hypotheſis being to render an intelligible account of the cauſes of the effects, or phænomena propoſed, without croſſing the laws of nature, or other phænomena; the more numerous, and the more various the particles are, whereof ſome are explicable by the aſſigned hypotheſis, and ſome are agreeable to it, or, at leaſt, are not diſſonant from it, the more valuable is the hypotheſis, and the more likely to be true. For it is much more difficult, to find an hypotheſis, that is not true, which will ſuit with many phænomena, eſpecially, if they be of various kinds, than but with a few. And for this reaſon, I have ſet down among the inſtances belonging to particular qualities, ſome ſuch experiments and obſervations, as we are now ſpeaking of, ſince, although they be not direct proofs of the preferableneſs of our doctrine, yet they may ſerve for confirmation of it; though this be not the only, or perhaps the chief reaſon of their being mentioned. For, whatever they may be as argument, ſince they are matters of fact, I thought it not amiſs to take this occaſion of preſerving them from being loſt; ſince, whether or no they contribute much to the eſtabliſhment of the mechanical doctrine about qualities, they will, at leaſt, contribute to the natural hiſtory of them.

III. I ſhall not trouble the reader with a recital of thoſe unlucky accidents, that have hindered the ſubjects of the following book from being more numerous; and I hope he will the more eaſily excuſe their paucity, if he be advertiſed, that although the particular qualities, about which ſome experiments and notes, by way of ſpecimens, are here preſented, be not near half ſo many as were intended to be treated of; yet I was careful to chooſe them ſuch as might comprehend in a ſmall number a great variety; there being ſcarce one ſort of qualities, of which there is not an inſtance given in this ſmall book, ſince therein experiments and thoughts are delivered about heat and cold, which are the chief of the four firſt qualities; about taſtes and odours, which are of thoſe, that, being the immediate objects of ſenſe, are wont to be called ſenſible qualities; about volatility and fixity, corroſiveneſs and corroſibility, which, as they are found in bodies purely natural, are referable to thoſe qualities, that many phyſical writers call ſecond qualities, and which yet, as they may be produced and deſtroyed by the chemiſts art, may be ſtiled chemical qualities, and the ſpagyrical ways of introducing, or expelling them, may be referred to chemical operations, of which there is given a more ample ſpecimen in the mechanical account of chemical precipitations. And laſtly, ſome notes are added about magnetiſm and electricity, which are known to belong to the tribe of occult qualities.

IV. If a want of apt coherence, and exact method, be diſcovered in the following eſſays, it is hoped, that defect will be eaſily excuſed by thoſe, that remember and conſider, that theſe papers were originally little better than a kind of rhapſody of experiments, thoughts, and obſervations, occaſionally thrown together by way of annotations upon ſome paſſages of a diſcourſe, (about the differing parts and redintegration of nitre) wherein ſome things were pointed at, relating to the particular qualities, that are here more largely treated of. And though the particulars, that concern ſome of theſe qualities, were afterwards (to ſupply the place of thoſe borrowed by other papers whilſt theſe lay by me) encreaſed in number; yet it was not to be expected, that their acceſſion ſhould as well correct the form as augment the matter of our annotations. And as for the two tracts, that are inſerted among theſe eſſays about qualities; I mean, the diſcourſe of the imperfection of the chemical doctrine of them, and the reflections on the hypotheſis of acidum and alcali, the occaſion of their being made parts of this book, is ſo far expreſſed in the tracts themſelves, that I need not here trouble the reader with a particular account of it.

V. I do not undertake, that all the following accounts of particular qualities would prove to be the very true ones, nor every explication the beſt, that can be deviſed. For beſides that the difficulty of the ſubject, and incompleatneſs of the hiſtory we yet have of qualities, may well deter a man, leſs diffident of his own abilities than I juſtly am, from aſſuming ſo much to himſelf, it is not abſolutely neceſſary to my preſent deſign. For, mechanical explications of natural phænomena do give ſo much more ſatisfaction to ingenious minds, than thoſe, that muſt employ ſubſtantial forms, ſympathy, antipathy, &c. that the more judicious of the vulgar philoſophers themſelves prefer them before all others, when they can be had; (as is elſewhere ſhewn at large,) but then they look upon them either as confined to mechanical engines, or at leaſt, but as reaching to very few of nature's phænomena, and, for that reaſon, unfit to be received as phyſical principles. To remove therefore this grand prejudice and objection, which ſeems to be the chief thing, that has kept off rational inquiries from cloſing with the mechanical philoſophy, it may be very conducive, if not ſufficient, to propoſe ſuch mechanical accounts of particular qualities themſelves, as are intelligible and poſſible, and are agreeable to the phænomena whereto they are applied. And to this it is no more neceſſary, that the account propoſed ſhould be the trueſt and beſt, that can poſſibly be given, than it is to the proving, that a clock is not acted by a vital principle, (as thoſe Chineſes thought, who took the firſt, that was brought them out of *Europe*, for an animal,) but acts as an engine, to do more than aſſign a mechanical ſtructure made up of wheels, a ſpring, a hammer, and other mechanical pieces, that will regularly ſhew and ſtrike the hour, whether this contrivance be, or be not, the very ſame with that of the particular clock propoſed; which may indeed be made to move either with ſprings or weights, and may conſiſt of a greater or leſſer number of wheels, and thoſe differingly ſituated and connected; but for all this variety, it will ſtill be but an engine. I intend not therefore by propoſing the theories and conjectures ventured at in the following papers, to debar myſelf of the liberty either of altering them, or of ſubſtituting others in their places, in caſe a further progreſs in the hiſtory of qualities ſhall ſuggeſt better hypotheſes or explications. And it was but agreeable to this intention of mine, that I ſhould, as I have done, on divers occaſions in the following notes, employ the word *or*, and expreſs myſelf ſomewhat doubtingly, mentioning more than one cauſe of a phænomenon, or reaſon of an opinion, without dogmatically declaring for either; ſince my purpoſe in theſe notes was rather to ſhew, it was not neceſſary to betake ourſelves to the ſcholaſtick or chemical doctrine about qualities, than to act the umpire between the differing hypotheſes of the Corpuſcularians; and, provided I kept myſelf within the bounds of mechanical philoſphy, my deſign allowed me a great latitude in making explications of the phænomena I had occaſion to take notice of.

OF THE MECHANICAL ORIGIN OF HEAT AND COLD.

SECTION I.

About the MECHANICAL PRODUCTION *of* COLD.

HEAT and cold being generally looked upon as the moſt active among qualities, from which many other qualities are deducible, and by which many of nature's phænomena, eſpecially among the Peripateticks, are attempted to be explicated; I ſuppoſe it will be very proper to begin with inſtances of them to ſhew, that qualities may be mechanically produced or deſtroyed. A not uſeleſs paraphraſe of which expreſſion may be this, that a portion of matter may come to be endowed with a quality, which it had not before, or to be deprived of one, that it had, or ſometimes to acquire, or loſe a degree of that quality; though on the part of the matter (or, as ſome would ſpeak, of the patient) there do not appear to intervene any more than a change of texture, or ſome other mechanical alteration; and though the agents (on their part) do not appear to act upon it otherwiſe, than after a mechanical manner, that is, by their bigneſs, ſhape, motion, and thoſe other attributes, by virtue whereof mechanical powers and engines perform their operations; and this without having recourſe to the Peripatetick ſubſtantial forms and elements, or the hypoſtatical principles of the chemiſts.

AND having here (as in a proper place) to avoid ambiguity, premiſed once for all this * ſummary declaration of the ſenſe, agreeably whereunto I would have theſe terms underſtood in the following notes about the origin of particular qualities; I proceed now to ſet down ſome few examples of the mechanical production of cold and heat, beginning with thoſe, that relate to the former, becauſe, by reaſon of their paucity, they will be quickly diſpatched. And I hope I ſhall not need to make an apology for mentioning no greater number; ſince I ſcarce remember to have met with any inſtances of this kind in any of the claſſick writers of natural philoſophy.

EXPERIMENT I.

MY firſt experiment is afforded me by the diſſolution of ſal armoniac, which I have ſomewhat wondered, that chemiſts having often occaſion to purify that ſalt by the help of water, ſhould not have, long ſince, and publickly, taken notice of. For, if you put into three or four times its weight of water, a pound, or but half a pound (or even leſs) of powdered ſal armoniac, and ſtir it about to haſten the diſſolution, there will be produced in the mixture a very intenſe degree of coldneſs, ſuch as will not be only very ſenſible to his hand, that holds the glaſs whilſt the diſſolution is making, but will very manifeſtly diſcover itſelf by its operation upon a thermoſcope. Nay, I have more than once, by wetting the outſide of the glaſs, where the diſſolution was making, and nimbly ſtirring the mixture, turned that externally adhering water into real ice, (that was ſcraped off with a knife) in leſs than a minute of an hour. And this thus generated cold continued conſiderably intenſe, whilſt the action of diſſolution laſted; but afterwards by degrees abated, and within a very few hours ceaſed. The particular phænomena I have noted in the experiments, and the practical uſes, that may be made of it, I reſerve for another place †, the knowledge of them being not neceſſary in this, where what I have already related, may ſuffice for my preſent argument.

AND to ſhew, that not only a far more intenſe degree of cold may emerge in this mixture, than was to be found in either of the ingredients before they were mingled, but a conſiderable coldneſs may be begun to be produced between bodies, that were neither of them actually cold before they were put together, I will ſubjoin a tranſcript of what I find to this purpoſe among my *adverſaria*.

EXPERIMENT II.

[I REMEMBER, that once I had a mind to try, whether the coldneſs produced upon the ſolution of beaten ſal armoniac in water might not be more probably referred to ſome change of texture or motion reſulting from the action of the liquor upon the ſalt, than to any infrigidation of the water made by the ſudden diſperſion of ſo many ſaline grains of powder, which, by reaſon of their ſolidity, may be ſuſpected to be actually more cold than the water they are put into; I therefore provided a glaſs full of that liquor, and having brought it to ſuch a temper, that its warmth made the ſpirit of wine in the ſealed weather-glaſs, manifeſtly, though not nimbly, aſcend; I took out the thermoſcope, and laid it in powdered ſal armoniac, warmed beforehand; ſo that the tincted liquor was made to aſcend much nimblier by the ſalt than juſt before by the water; and having preſently removed the inſtrument into that liquor again, and poured the ſomewhat warm ſal armoniac into the ſame, I found, as I imagined, that within a ſpace of time, which I gueſſed to be about half a minute or leſs, the ſpirit of wine began haſtily to ſubſide, and within a few minutes fell above a whole diviſion and a quarter below the mark at which it ſtood in the water, before that liquor or the ſalt were warmed. Nor did the ſpirit in a great while re-aſcend to the height, which it had, when the water was cold.

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* See more of this in the preamble.

† Divers of the phænomena &c. of this experiment were afterwards printed. Numb. 15. of the *Ph. Tranſact.*

The ſame experiment, being at another time reiterated, was tried with the like ſucceſs; which ſecond may therefore ſerve for a confirmation of the firſt.]

EXPERIMENT III.

HAVING a mind likewiſe to ſhew ſome ingenious men, how much the production of heat and cold depends upon texture and other mechanical affections, I thought fit to make again a ſal armoniac by a way I formerly publiſhed, that I might be ſure to know what ingredients I employed, and ſhew their effects, as well before conjunction as after it. I took then ſpirit of ſalt, and ſpirit of fermented, or rather putrified urine; and having put a ſealed weather-glaſs into an open veſſel, where one of them was poured in, I put the other, by degrees, to it, and obſerved, that as, upon their mingling, they made a great noiſe with many bubbles, ſo, in this conflict, they loſt their former coldneſs, and impelled up the ſpirit of wine in the ſealed thermoſcope: Then ſlowly evaporating the ſuperfluous moiſture, I obtained a fine ſort of ſal armoniac, for the moſt part figured not unlike the other, when being diſſolved and filtrated, it is warily coagulated. This new ſalt being gently dried, I put into a wide glaſs of water, wherein I had before placed a ſealed weather-glaſs, that the included ſpirit might acquire the temper of the ambient liquor, and having ſtirred this ſalt in the water, though I took it then off the mantle-tree of a chimney, that had had fire in it divers hours before, it did, as I expected, make the tincted ſpirit haſtily ſubſide, and fall conſiderably low.

EXPERIMENT IV.

SINCE, if two bodies, upon their mixture, acquire a greater degree of cold than either of them had before, there is a production of this additional degree of that quality, it will be proper to add, on this occaſion, the enſuing experiment.

We took a competent quantity of acid ſpirit diſtilled from roch-allom, (that, though rectified, was but weak,) which, in the ſpirit of that ſalt, is not ſtrange. Of this we put into a wide-mouthed glaſs (that was not great) more than was ſufficient to cover the globulous part of a good ſealed thermoſcope, and then ſuffering the inſtrument to ſtay a pretty while in the liquor, that the ſpirit of wine might be cooled, as much as the ambient was, we put in, little by little, ſome volatile ſalt ſublimed from ſal armoniac and a fixed alcali, and notwithſtanding the very numerous (but not great) bubbles, and the noiſe and froth that were produced, as is uſual upon the re-action of acids and alcalies, the tincted ſpirit in the weather-glaſs, after having continued a good while at a ſtand, began a little to deſcend, and continued (though but very ſlowly) to do ſo, till the ſpirit of allom was glutted with the volatile ſalt; and this deſcent of the tincted liquor in the inſtrument being meaſured, appeared to be about an inch (for it manifeſtly exceeded ſeven eighths.) By comparing this experiment with the firſt part of the foregoing, we may gather, that when volatile and urinous ſalts or ſpirits (for the ſaline particles appear ſometimes in a dry, and ſometimes in a liquid form) tumultuate upon their being mixed with acids, neither the heat nor the cold, that enſues, is produced by a conflict with the acids preciſely as it is acid, ſince we have ſeen, that an urinous ſpirit produced an actual heat with ſpirit of ſalt, and the diſtilled ſalt of ſal armoniac, which is alſo urinous, with the acid ſpirit of roch-allom, produces not a true efferveſcence, but a manifeſt coldneſs: as the ſame ſalt alſo did in a trial of another ſort, which was this.

EXPERIMENT V.

WE took one part of oil of vitriol, and ſhaking it into twelve parts of water we made a mixture, that at firſt was ſenſibly warm; then ſuffering this to cool, we put a ſufficient quantity of it into a wide mouthed glaſs, and then we put a good thermoſcope hermetically ſealed, above whoſe ball the compounded liquor reached a pretty way. After ſome time had been allowed, that the liquor in the thermometer might acquire the temper of the ambient; we put in, by degrees, as much volatile ſalt of ſal armoniac, as would ſerve to ſatiate the acid ſpirits of the mixture: for, though theſe two made a notable conflict with tumult, noiſe, and froth, yet it was but a cold ebullition (if I may ſo ſtile it,) for the ſpirit in the thermoſcope deſcended about an inch beneath the mark it reſted at, when the ſeeming efferveſcence began.

EXPERIMENT VI.

IT is known, that ſalt-petre being put into common water produces a ſenſible coldneſs in it, as it alſo does in many other liquors: But that the ſame ſalt put into a liquor of another conſtitution may have a quite differing effect, I have convinced ſome inquiſitive perſons, by mingling eight ounces of fine ſalt-petre, powdered, with ſix ounces of oil of vitriol: For by that commixture with a ſalt, that was not only actually, but, as to many other bodies, potentially cold, the oil of vitriol, that was ſenſibly cold before, quickly conceived a conſiderable degree of heat, whoſe effects alſo became viſible in the copious fumes, that were emitted by the incaleſcent mixture.

EXPERIMENT VII.

THIS brings into my mind, that though gunpowder ſeems to be of ſo igneous a nature, that, when it is put upon a coal, it is turned preſently into flame capable of promoting the deflagration of the charcoal, and kindling divers bodies it meets with in its way; yet if ſome ounces of gunpowder, reduced to powder, be thrown into four or five times as much water, it will very manifeſtly impart a coldneſs to it, as experience made with, as well as without, a ſealed thermoſcope has aſſured me.

THIS and the foregoing experiment do readily ſuggeſt an enquiry into the nature of the coldneſs, which philoſophers are wont to oppoſe to that, which immediately, and upon the firſt contact, affect the organs of ſenſe, and which therefore they call actual or formal.

THE ſucceſs of this experiment upon a ſecond trial ſerved to confirm it, which is the more ſtrange, becauſe I have found, that a ſmall quantity of oil of vitriol, not beforehand mingled with water, would produce a notable heat in its conflict with a ſmall portion of juſt ſuch ſalt as I employed before (both the parcels having been, if I well remember, taken out of the ſame glaſs.) And this heat did, upon trial made with the former thermoſcope, make the tincted ſpirit aſcend much further than the lately recited experiment made it ſubſide.

A DIGRESSION ABOUT POTENTIAL COLDNESS.

POTENTIAL coldneſs has been generally looked upon, and that partly perhaps upon the ſcore of its very name, as ſo abſtruſe a quality, that it is not only rational, but neceſſary to derive it from the ſubſtancial forms of bodies. But, I confeſs, I ſee no neceſſity of believing it not to be referrible to mechanical principles. For, as to the chief inſtances of potential coldneſs, which are taken from the effects of ſome medicines and aliments in the bodies of men, it may be ſaid, without improbability, that the produced refrigeration proceeds chiefly from this, that the potentially cold body is made up of corpuſcles of ſuch ſize, ſhape, &c. that, being reſolved and disjoined by the menſtruum of the ſtomach, or the fluids it may elſewhere meet with, they do ſo aſſociate themſelves with the ſmall parts of the blood, and other liquors, as, by clogging them, or otherwiſe, to leſſen their wonted agitation, and perhaps make them act in a peculiar way, as well as leſs briſkly on the nervous and fibrous parts; and the perception of this imminution (and perhaps change) of motion in the organs of feeling, is that, which, being referred to the body, that produces it, we call it's potential coldneſs. Which quality appears by this account to be, as I was ſaying before, but a relative thing, and is wont to require the diffuſion or diſperſion of the ſmall parts of the corpuſcles of the agent, and their mingling themſelves with the liquors, or the ſmall parts of the body they are to refrigerate. And therefore, if it be granted, that, in agues, there is ſome morbifick matter, of a viſcous or not eaſily diſſipable texture, that is harboured in ſome part of the body, and requires ſuch a time to be made fluid and reſolvable, the cold fits of agues need not be ſo much admired as they uſually are; ſince, though juſt before the fit the ſame parcel of matter, that is to produce it, were actually in the body, yet it was not, by reaſon of its clamminesſ, actually reſolved into ſmall parts, and mingled with thoſe of the blood, and conſequently could not make ſuch a change in the motion of that liquor, as is felt in the cold fit of an ague; (for, of the further change, that occaſions the hot fit, I am not here to ſpeak.) And in ſome other diſeaſes, a ſmall quantity of matter, being reſolved into minute parts, may be able to produce a great ſenſe of coldneſs in ſome part of a body, which, by reaſon of the ſtructure of that part, may be peculiarly diſpoſed to be affected thereby; as I have known hypochondriac and hyſterical women complain of great degrees of coldneſs, that would ſuddenly invade ſome particular part, chiefly of the head or back, and be for a good while troubleſome there. And that, if a frigorific vapour, or matter, be exceeding ſubtile, an inconſiderable quantity of it being diſperſed through the blood, may ſuffice to produce a notable refrigeration, I have learned by enquiry into the effects of ſome poiſons; and it is not very material, whether the poiſon, generally ſpeaking, be cold or hot, if it meet with a body diſpoſed to have thoſe affections, that paſs for cold ones produced in it. For I have made a chemical liquor, that was penetrant and fiery enough to the taſte, and had acquired a ſubtlety and briſkneſs from diſtillation, with which I could, almoſt in a trice, giving it but in the quantity of about a drop, caſt an animal into that, which appeared a ſleep; and the like liquor, in a not much greater quantity, being, by I know not whoſe miſtake, applied to the aching tooth of a very ingenious perſon, did preſently, as he ſoon after told me, give him an univerſal refrigeration, and trembling, worſe than the cold paroxiſm of a quartane. And though ſcorpions do ſometimes cauſe, by their ſting, violent heats in the parts they hurt, yet ſometimes alſo the quite contrary happens, and their poiſon proves, in a high degree, potentially cold; as may be learned from the two following obſervations, recorded by eminent phyſicians. * *Famulum habui* (ſaith *Benivenius*,) *qui à ſcorpione ictus, tam ſubito ac tam frigido ſudore toto corpore perfuſus eſt, ut algentiſſimâ nive atque glacie ſeſe opprimi quereretur. Verùm cùm algenti illi ſolam theriacam ex vino potentiore exhibuiſſem, illicò curatus eſt*: thus far he: to whoſe narrative I add this of *Amatus Luſitanus*.

* *Beniven.* cap. 56. *Abditorum apud Schenk.* lib. 7. de venen. obſerv. 24.

VIR *qui à ſcorpione in manus digito punctus fuit, multum dolebat, & refrigeratus totus contremebat, & per corpus dolores, cute totâ quaſi acu punctâ, formicantes patiebatur, &c.*

Cent. 6. obſerv.

I cannot now ſtay to enquire, whether there may not be in theſe great refrigerations, made by ſo ſmall a quantity of poiſon, ſome ſmall concretions or coagulations made of the minute particles of the blood into little clots, leſs agile and more unweildy than they were, when they moved ſeparately: which may be illuſtrated by the little curdlings, that may be made of the parts of milk, by a very ſmall proportion of runnet, or ſome acid liquor, and the little coagulations made of the ſpirit of wine by that of urine: nor will I now enquire, whether, beſides the retardment of the motion of the blood, ſome poiſons, and other analogous agents, may not give the motion of it a new modification, (as if ſome corpuſcles, that uſually are more whirled or brandiſhed, be put into a more direct motion,) that may give it a peculiar kind of grating, or other action, upon the nervous and fibrous parts of the body. Theſe, I ſay, and other ſuſpicions, that have ſometimes come into my thoughts, I muſt not ſtay to examine; but ſhall now rather offer to conſideration, whether, ſince ſome parts of the human body are very differing from others in their ſtructure and internal conſtitution; and ſince alſo ſome agents may abound in corpuſcles of differing ſhapes, bulks, and motions, the ſame medicine may not, in reference to the ſame human body, be potentially cold, or potentially hot, according as it is applied; or perhaps may, upon one or both of the accounts newly mentioned, be cold, in reference to one part of the body, and hot, in reference to the other. And theſe effects need not be always aſcribed to the mere and immediate action of the corpuſcles of the medicine, but ſometimes to the new quality they acquire in their paſſage, by aſſociating themſelves with the blood, or other fluids of the body, or to the expulſion of ſome calorific or frigorific corpuſcles, or to the diſpoſition they give the part on which they operate, to be more or leſs permeated and agitated than before, by ſome ſubtile æthereal matter, or other efficients of heat or cold. Some of theſe conjectures about the relative nature of potentially cold bodies may be either confirmed or illuſtrated by ſuch inſtances as theſe; that ſpirit of wine, being inwardly taken, is potentially very hot; and yet, being outwardly applied to ſome burns, and ſome hot tumours, does notably abate the heat of the inflamed parts, though the ſame ſpirit, applied even outwardly to a tender eye, will cauſe a great and dolorous agitation in it. And camphire, which in the doſe of leſs than a half, or perhaps a quarter of a ſcruple, has been obſerved to diffuſe a heat through the body, is, with ſucceſs, externally applied by phyſicians and ſurgeons in refrigerating medicines.

BUT I leave the further inquiry into the operations of medicines to phyſicians, who may poſſibly, by what has been ſaid, be aſſiſted to compoſe the differences between ſome famous writers about the temperament of ſome medicines, as mercury, camphire, &c. which ſome will have to be cold, and others maintain to be hot; and ſhall only offer by way of confirming in general, that potential coldneſs is only a relative quality, a few particulars; the firſt whereof is afforded by comparing together the ſixth and the ſeventh experiment before going, (which have occaſioned this digreſſion about potential coldneſs;) ſince by them it ſeems probable, that the ſame thing may have it in reference to one body, and not to another, according to the diſpoſition of the body it operates upon, or that operates upon it. And the fumes of lead have been obſerved ſometimes (for I have not found the effect to ſucceed always) to arreſt the fluidity of mercury, which change is ſuppoſed to be the effect of a potential coldneſs belonging to the chemiſt's Saturn in reference to fluid mercury, though it have not that operation on any other liquor, that we know of.

AND laſtly, (for I would not be too prolix) though nitre and ſal armoniac be both apart and jointly cold in reference to water, and though, however nitre be thoroughly melted in a crucible, it will not take fire of itſelf, yet if, whilſt it is in fuſion, you ſhould by degrees caſt on it ſome powdered ſal armoniac, it will take fire and flaſh vehemently, almoſt as if ſulphur had been injected.

But our excurſion has, I fear, laſted too long, and therefore I ſhall preſently re-enter into the way, and proceed to ſet down ſome trials about cold.

EXPERIMENT VIII.

IN the firſt experiment we obſerved, that upon the pouring of water upon ſal armoniac there enſued an intenſe degree of cold; and we have elſewhere recited, that the like effect was produced by putting, inſtead of common water, oil of vitriol to ſal armoniac: but now, to ſhew further, what influence motion and texture may have upon ſuch trials, it may not be amiſs to add the following experiment: to twelve ounces of ſal armoniac we put, by degrees, an equal weight of water, and whilſt the liquor was diſſolving the ſalt, and by that action producing a great coldneſs, we warily poured in twelve ounces alſo of good oil of vitriol; of which new mixture the event was, that a notable degree of heat was quickly produced in the glaſs, wherein the ingredients were confounded, as unlikely as it ſeemed, that, whereas each of the two liquors is wont, with ſal armoniac, to produce an intenſe cold, both of them acting on it together ſhould produce the contrary quality. But the reaſon I had to expect the ſucceſs I met with, was this, that it was probable the heat, ariſing from the mixture of the two liquors, would overpower the coldneſs produceable by the operation of either, or both of them upon the ſalt.

EXPERIMENT IX.

IN moſt of the experiments, that we have hitherto propoſed, cold is wont to be regularly produced in a mechanical way; but I ſhall now add, that in ſome ſort of trials I found, that the event was varied by unobſerved circumſtances; ſo that, ſometimes manifeſt coldneſs would be produced by mixing two bodies together, which at another time would upon their congreſs diſcloſe a manifeſt heat, and ſometimes again, though more rarely, would have but a very faint and remiſs degree of either.

Of this ſort of experiments, whoſe events I could not confidently undertake for, I found to be, the diſſolution of ſalt of tartar in ſpirit of vinegar, and of ſome other ſalts, that were not acid, in the ſame menſtruum, and even ſpirit of verdigreaſe (made *per ſe*) though a more potent menſtruum than common ſpirit of vinegar would not conſtantly produce near ſuch a heat at the beginning of its operation, as the greatneſs of the ſeeming effervеſcence, then excited, would make one expect, as may appear by the following obſervation tranſcribed *verbatim* out of one of my *Adverſaria*.

[Into eight ounces of ſpirit of verdigreaſe (into which we had put a while before a ſtandard-thermoſcope, to acquire the like temper with the liquor) we put in a wide-mouthed glaſs two ounces of ſalt of tartar, as faſt as we durſt for fear of making the matter boil over; and though there were a great commotion excited by the action and reaction of the ingredients, which was attended with a copious froth and a hiſſing noiſe; yet it was a pretty while, ere the glaſs was ſenſibly warm on the outſide; but by that time the ſalt was all diſſolved, the liquor in the thermoſcope appeared to be impelled up about three inches and a half.

And yet, if my memory do not much deceive me, I have found, that by mixing ſalt of tartar with another ſalt, the texture of the fixed alkali was ſo altered, that upon the affuſion of ſpirit of verdigreaſe, (made without ſpirit of vinegar and ſpirit of wine) though there enſued a great conflict with noiſe and bubbles, yet, inſtead of an incaleſcence, a conſiderable degree of coldneſs was produced.

EXPERIMENT X.

IT is very probable, that further trials will furniſh us with more inſtances, to ſhew how the production of cold may, in ſome caſes, be effected, varied, or hindered by mechanical circumſtances, that are eaſily and uſually overlooked. I remember, on this occaſion, that though, in the experiment above recited, we obſerved, that oil of vitriol and water being firſt ſhaken together, the volatile ſalt of ſal armoniac being afterwards put to them, produced a ſenſible coldneſs; yet I found, that if a little oil of vitriol, and of the volatile ſalt, were firſt put together, though ſoon after a conſiderable proportion of water were added, there would be produced, not a coldneſs, but a manifeſt degree of heat, which would impel up the liquor in the thermoſcope to the height of ſome inches. And I remember too, that though ſalt of tartar will, as we ſhall ſee ere long, grow hot in the water, yet having diſtilled ſome ſalt of tartar and cinnabar in a ſtrong fire, and put the whole *Caput mortuum* into diſtilled or rain water, it made indeed a hiſſing there, as if it had been quick lime, but produced no heat, that I could by feeling perceive. I ſhall add, that not only, as we have ſeen already, ſome unheeded circumſtances may promote or hinder the artificial production of cold by particular agents, but, which will ſeem more ſtrange, ſome unobſerved, and perhaps hardly obſervable, indiſpoſition in the patient, may promote or hinder the effects of the grand and catholick efficients of cold, whatever thoſe be. This ſuſpicion I repreſent as a thing, that further experience may poſſibly countenance, becauſe I have ſometimes found, that the degree of the operation of cold has been much varied by latent circumſtances, ſome bodies being more wrought upon, and others leſs, than was, upon very probable grounds, expected. And particularly I remember, that though oil of vitriol be one of the firieſt liquors, that is yet known, and does perform ſome of the operations of fire itſelf, (as we ſhall elſewhere have occaſion to ſhew) and will thaw ice ſooner than ſpirit of wine, or any other liquor, as I have tried; yet having put about a pound or more, by our eſtimate, of choice rectified oil of vitriol, into a ſtrong glaſs vial proportionable to it, we found, that, except a little, that was fluid at the top, it was all congealed or coagulated into a maſs like ice, though the glaſs ſtood in a laboratory, where a fire was conſtantly kept not far from it, and where oil of vitriol very ſeldom, or never, has before, or ſince, been obſerved to congeal or coagulate ſo much as in part. And the oddneſs of our phænomenon was encreaſed by this circumſtance, that the maſs continued ſolid a good while after the weather was grown too mild to have ſuch operations upon liquors far leſs indiſpoſed to loſe their fluidity by cold, than even common oil of vitriol is. On the other ſide I remember, that about two years ago, I expoſed ſome oil of ſweet almonds hermetically ſealed up in a glaſs bubble, to obſerve what condenſation an intenſe cold could make of it, (for though cold expands water, (it condenſes common oil;) but the next day I found, to my wonder, that not only the oil remained unfrozen by the ſharp froſt it had been expoſed to, but that it had not its tranſparency troubled, though it is known, that oil will be brought to concrete, and turn opacous by a far leſs degree of cold than is requiſite to freeze water; notwithſtanding which, this liquor, which was lodged in a glaſs, ſo thin, that it was blown at the flame of a lamp, continued fluid and diaphanous in very froſty weather, ſo long till I loſt the expectation of ſeeing it congealed or concreted. And this brings into my mind, that though camphire be, as I formerly noted, reckoned by many potentially cold, yet we kept ſome oil of it, of our making, wherein the whole body of the camphire remained, being only by ſome nitrous ſpirits reduced to the form of an oil; we kept it, I ſay, in ſuch intenſe degrees of cold, that would have eaſily frozen water, without finding it to loſe its tranſparency, or its fluidity.

And here I ſhall put an end to the firſt ſection, (containing our notes about cold) the deſign of which may be not a little promoted by comparing with them the beginning of the enſuing ſection. For if it be true, that (as we there ſhew) the nature of heat conſiſts either only or chiefly in the local motion of the ſmall parts of a body mechanically modified by certain conditions, of which the principal is the vehemency of the various agitations of thoſe inſenſible parts; and if it be alſo true, as experience witneſſes it to be, that, when the minute parts of a body are in, or arrive at ſuch a ſtate, that they are more ſlowly or faintly agitated than thoſe of our fingers, or other organs of feeling, we judge them cold: theſe two things, laid together, ſeem plainly enough to argue, that a privation or negation of that local motion, that is requiſite to conſtitute heat, may ſuffice for the denominating a body cold, as coldneſs is a quality of the object, (which, as it is perceived by the mind, is alſo an affection of the ſentient:) and therefore an imminution of ſuch a degree of former motion, as is neceſſary to make a body hot as to ſenſe, and which is ſufficient to the production of ſenſible coldneſs, may be mechanically made, ſince ſlowneſs, as well as ſwiftneſs, being a mode of local motion, is a mechanical thing. And though its effect, which is coldneſs, ſeem a privation or negation; yet the cauſe of it may be a poſitive agent acting mechanically, by clogging the agile calorific particles, or deadning their motion, or perverting their determination, or by ſome other intelligible way bringing them to a ſtate of coldneſs, as to ſenſe: I ſay, coldneſs as to ſenſe; becauſe as it is a tactile quality, in the popular acception of it, it is relative to our organs of feeling; as we ſee, that the ſame luke-warm water will appear hot and cold to the ſame man's hands, if, when both are plunged into it, one of them ſhall have been newly held to the fire, and the other be benumbed with froſt. And indeed the cuſtom of ſpeaking has introduced an ambiguity into the word cold, which often occaſions miſtakes, not eaſily, without much attention, and ſometimes circumlocution alſo, to be avoided; ſince uſually by cold is meant that, which immediately affects the ſenſory of him, that pronounces a body cold, whereas ſometimes it is taken in a more general notion for ſuch a negation or imminution of motion, as though it operates not perceivably on our ſenſes, does yet upon other bodies; and ſometimes alſo it is taken (which is perhaps the more philoſophical ſenſe) for a perception, made in and by the mind, of the alteration produced in the corporeal organs by the operation of that, whatever it be, on whoſe account a body is found to be cold.

But the diſcuſſion of theſe points is here purpoſely omitted, as for other reaſons, ſo principally, becauſe they may be found expreſsly handled in a fitter place.

SECTION II.

Of the Mechanical Origin, *or* Production *of* HEAT.

After having diſpatched the inſtances I had to offer of the production of cold, it remains, that I alſo propoſe ſome experiments of heat, which quality will appear the more likely to be mechanically producible, if we conſider the nature of it, which ſeems to conſiſt mainly, if not only, in that mechanical affection of matter we call local motion mechanically modified, which modification, as far as I have obſerved, is made up of three conditions.

The firſt of theſe is, that the agitation of the parts be vehement, by which degree of rapidneſs the motion proper to bodies, that are hot, diſtinguiſhes them from bodies, that are barely fluid. For theſe, as ſuch, require not near ſo briſk an agitation, as is wont to be neceſſary to make bodies deſerve the name of hot. Thus we ſee, that the particles of water, in its natural (or uſual) ſtate, move ſo calmly, that we do not feel it at all warm, though it could not be a liquor, unleſs they were in a reſtleſs motion; but when water comes to be actually hot, the motion does manifeſtly and proportionably appear more vehement, ſince it does not only briſkly ſtrike our organs of feeling, but ordinarily produces ſtore of very ſmall bubbles, and will melt butter or coagulated oil caſt upon it, and will afford vapours, that, by the agitation they ſuffer, will be made to aſcend into the air. And if the degree of heat be ſuch, as to make the water boil, then the agitation becomes much more manifeſt by the confuſed motions, and waves, and noiſe, and bubbles, that are excited, and by other obvious effects, and phænomena of the vehement and tumultous motion, which is able to throw up viſibly into the air great ſtore of corpuſcles, in the form of vapours or ſmoke. Thus, in a heated iron, the vehement agitation of the parts may be eaſily inferred, from the motion and hiſſing noiſe it imparts to drops of water, or ſpittle, that fall upon it. For it makes them hiſs and boil, and quickly forces their particles to quit the form of a liquor, and fly into the air in the form of ſteams. And, laſtly, fire, which is the hotteſt body we know, conſiſts of parts ſo vehemently agitated, that they perpetually and ſwiftly fly abroad in ſwarms, and diſſipate or ſhatter all the combuſtible bodies they meet with in their way; fire making ſo fierce a diſſolution, and great a diſperſion of its own fuel, that we may ſee whole piles of ſolid wood (weighing perhaps many hundred pounds) ſo diſſipated, in very few hours, into flame and ſmoke, that, oftentimes, there will not be one pound of aſhes remaining. And this is the firſt condition required to heat.

The ſecond is this, that the determinations be very various, ſome particles moving towards the right, ſome to the left hand, ſome directly upwards, ſome downwards, and ſome obliquely, &c. This variety of determinations appears to be in hot bodies, both by ſome of the inſtances newly mentioned, and eſpecially that of flame, which is a body; and by the diffuſion, that metals acquire, when they are melted, and by the operations of heat, that are exerciſed by hot bodies upon others, in what poſture or ſituation ſoever the body to be heated be applied to them. As a thoroughly ignited coal will appear every way red, and will melt wax, and kindle brimſtone, whether the body be applied to the upper or to the lower, or to any other part of the burning coal. And congruouſly to this notion, though air and water be moved never ſo vehemently, as in high winds and cataracts; yet we are not to expect, that they ſhould be manifeſtly hot, becauſe the vehemency belongs to the progreſſive motion of the whole body; notwithſtanding which, the parts it conſiſts of may not be near ſo much quickened in their motions, made according to other determinations, as to become ſenſibly hot. And this conſideration may keep it from ſeeming ſtrange, that, in ſome caſes, where the whole body, though rapidly moved, tends but one way, it is not by that ſwift motion perceived to be made hot.

Nay, though the agitation be very various, as well as vehement, there is yet a third condition required to make it calorific; namely, that the agitated particles, or at leaſt the greateſt number of them, be ſo minute, as to be ſingly inſenſible. For though a heap of ſand, or duſt itſelf, were vehemently and confuſedly agitated by a whirl-wind, the bulk of the grains or corpuſcles, would keep their agitation from being properly heat, though, by their numerous ſtrokes upon a man's face, and the briſk commotion of the ſpirits, and other ſmall particles, that may thence enſue, they may perchance occaſion the production of that quality.

If ſome attention be employed, in conſidering the formerly propoſed notion of the nature of heat, it may not be difficult to diſcern, that the mechanical production of it may be divers ways effected. For, excepting in ſome few, anomalous caſes, (wherein the regular courſe of things happens to be over-ruled,) by whatever ways the inſenſible parts of a body are put into a very confuſed and vehement agitation, by the ſame ways heat may be introduced into that body: agreeably to which doctrine, as there are ſeveral agents and operations, by which this calorific motion (if I may ſo call it) may be excited, ſo there may be ſeveral ways of mechanically producing heat, and many experiments may be reduced to almoſt each of them, chance itſelf having, in the laboratories of chemiſts, afforded divers phænomena, referable to any one or other of thoſe heads. Many of the more familiar inſtances, applicable to our preſent purpoſe, have been long ſince collected by our juſtly-famous *Verulam*, in his ſhort, but excellent paper *de forma calidi*, wherein (though I do not acquieſce in every thing I meet with there) he ſeems to have been, at leaſt among the moderns, the perſon, that has firſt handled the doctrine of heat like an experimental philoſopher. I ſhall therefore decline accumulating a multitude of inſtances of the production of heat, and I ſhall alſo forbear to inſiſt on ſuch known things, as the incaleſcence, obſervable upon the pouring either of oil of vitriol upon ſalt of tartar, (in the making of tartarum vitriolatum) or of aqua fortis upon ſilver or quickſilver, (in the diſſolution of theſe metals,) but ſhall rather chuſe to mention ſome few inſtances not ſo notorious as the former, but not ſo unfit, by their variety, to exemplify ſeveral of the differing ways of exciting heat.

And yet I ſhall not decline the mention of the moſt obvious and familiar inſtance of all, namely, the heat obſerved in quick-lime, upon the affuſion of cold water, becauſe, among learned men, and eſpecially Peripateticks, I find cauſes to be aſſigned, that are either juſtly queſtionable, or manifeſtly erroneous. For, as to what is inculcated by the ſchools, about the incaleſcence of a mixture of quick-lime and water, by virtue of a ſuppoſed Antiperiſtaſis, or invigoration of the internal heat of the lime, by its being invironed by cold water, I have elſewhere ſhewn, that this is but an imaginary cauſe, by delivering, upon experiment, (which any man may eaſily make,) that if, inſtead of cold water, the liquor be poured on very hot, the ebullition of the lime will not be the leſs, but rather the greater: and oil of turpentine, which is a lighter, and is looked upon as a ſubtiler liquor than water, though it be poured quite cold on quick-lime, will not, that I have obſerved, grow ſo much as ſenſibly hot with it.

And now I have mentioned the incaleſcence of lime, which, though an obvious phænomenon, has exerciſed the wits of divers philoſophers and chemiſts, I will add two or three obſervations, in order to an enquiry, that may be ſome other time made into the genuine cauſes of it; which are not ſo eaſy to be found, as many learned men may, at firſt ſight, imagine. The acute *Helmont* indeed, and his followers, have ingeniouſly enough attempted to derive the heat under conſideration from the conflict of ſome alcalizate and acid ſalts, that are to be found in quick-lime, and are diſſolved, and ſo ſet at liberty, to fight with one another by the water that ſlakes the lime. But, though we have ſome manifeſt marks of an alcalizate ſalt in lime, yet, that it contains alſo an acid ſalt, has not, that I remember, been proved; and if the emerging of heat be a ſufficient reaſon to prove a latent acid ſalt in lime, I know not, why I may not infer, that the like ſalt lies concealed in other bodies, which the chemiſts take to be of the pureſt or mereſt ſort of alcalies.

EXPERIMENT I.

For I have purpoſely tried, that by putting a pretty quantity of dry ſalt of tartar in the palm of my hand, and wetting it well in cold water, there has been a very ſenſible heat produced in the mixture; and when I have made the trial with a more conſiderable quantity of ſalt and water in a vial, the heat proved troubleſomely intenſe, and continued to be at leaſt ſenſible a good while after.

This experiment ſeems to favour the opinion, that the heat produced in lime, whilſt it is quenching, proceeds from the empyreuma, as the chemiſts call it, or impreſſion left by the violent fire, that was employed to reduce the ſtone to lime. But if by empyreuma be meant a bare impreſſion made by the fire, it will be more requiſite than eaſy, to declare intelligibly, in what that impreſſion conſiſts, and how it operates to produce ſuch conſiderable effects. And if the effect be aſcribed to ſwarms of atoms of fire, that remain adherent to the ſubſtance of the lime, and are ſet at liberty to fly away by the liquor, which ſeems to be argued by the ſlaking of lime without water, if it be for ſome time left in the air, whereby the atoms of fire get opportunity to fly away by little and little: if, this, I ſay, be alledged, I will not deny, but there may be a ſenſe, which I cannot explicate in few words, wherein the co-operation of a ſubſtantial effluvium, (for ſo I call it,) of the fire, may be admitted in giving an account of our phænomenon. But the cauſe formerly aſſigned, as it is crudely propoſed, leaves in my mind ſome ſcruples. For it is not ſo eaſy to apprehend, that ſuch light and minute bodies, as thoſe of fire, are ſuppoſed, ſhould be ſo long detained, as by this hypotheſis they muſt be allowed to be, in quick-lime, kept in well-ſtopped veſſels, from getting out of ſo lax and porous a body as lime, eſpecially ſince we ſee not a great incaleſcence or ebullition enſue upon the pouring of water upon minium, or *crocus Martis per ſe*, though they have been calcined by violent and laſting fires, whoſe effluviums or emanations appear to adhere to them by the increaſe of weight, that lead, if not alſo *Mars*, does manifeſtly receive from the operation of the fire. To which I ſhall add, that, whereas one would think, that the igneous atoms ſhould either fly away, or be extinguiſhed by the ſupervening of water, I know, and elſewhere give account, of an

EXPERIMENT II.

In which two liquors, whereof one was furniſhed me by nature, did by being ſeveral times ſeparated and reconjoined without additament, at each congreſs produce a ſenſible heat.

EXPERIMENT III.

And an inſtance of this kind, though not ſo odd, I purpoſely ſought and found in ſalt of tartar, from which, after it had been once heated by the affuſion of water, we abſtracted or evaporated the liquor, without violence of fire, till the ſalt was again dry; and then putting on water a ſecond time, the ſame ſalt grew hot again in the vial, and, if I miſremember not, it produced this incaleſcence the third time, if not the fourth; and might probably have done it oftener, if I had had occaſion to proſecute the experiment. Which ſeems at leaſt to argue, that the great violence of fire is not neceſſary to impreſs what paſſes for an empyreum upon all calcined bodies, that will heat with water.

And on this occaſion I ſhall venture to add, that I have ſometimes doubted, whether the incaleſcence may not much depend upon the particular diſpoſition of the calcined body, which being deprived of its former moiſture, and made more porous by the fire, doth by the help of thoſe igneous effluviums, for the moſt part of a ſaline nature, that are diſperſed through it, and adhere to it, acquire ſuch a texture, that the water impelled by its own weight, and the preſſure of the atmoſphere, is able to get into a multitude of its pores at once, and ſuddenly diſſolve the igneous and alcalizate ſalt it every where meets with there, and briſkly disjoin the earthy and ſolid particles, that were blended with them; which being exceeding numerous, though each of them perhaps be very minute, and moves but a very little way, yet their multitude makes the confuſed agitation of the whole aggregate of them, and of the particles of the water and ſalt vehement enough to produce a ſenſible heat; eſpecially if we admit, that there is ſuch a change made in the pores, as occaſions a great increaſe of this agitation, by the ingreſs and action of ſome ſubtile ethereal matter, from which alone Monſieur *Des Cartes* ingeniouſly attempts to derive the incaleſcence of lime and water, as well as that of metals diſſolved in corroſive liquors; though as to the phænomena we have been conſidering, there ſeems at leaſt to concur a peculiar diſpoſition of body, wherein heat is to be produced to do one or both of theſe two things, namely, to retain good ſtore of the igneous effluvia, and to be, by their adheſion or ſome other operation of the fire, reduced to ſuch a texture of its component particles, as to be fit to have them eaſily penetrated, and briſkly, as well as copiouſly, diſſipated, by invading water. And this conjecture (for I propoſe it as no other) ſeems favoured by divers phænomena, ſome whereof I ſhall now annex. For here it may be obſerved, that both the diſſolved ſalt of tartar lately mentioned, and the artificial liquor, that grows hot with the natural, re-acquires that diſpoſition to incaleſcence upon abare conſtipation, or cloſer texture of the parts from the ſuperfluous moiſture they were drowned in before; the heat, that brought them to this texture, having been ſo gentle, that it is no way likely, that the igneous exhalations could themſelves produce ſuch a heat, or at leaſt, that they ſhould adhere in ſuch numbers, as muſt be requiſite to ſuch an effect, unleſs the texture of the ſalt of tartar, or other body, did peculiarly diſpoſe it to detain them; ſince

EXPERIMENT IV.

I HAVE found by trial, that ſal armoniac diſſolved in water, though boiled up with a briſker fire to a dry ſalt, would, upon its being again diſſolved in water, not produce any heat, but a very conſiderable degree of cold. I ſhall add, that though one would expect a great cognation between the particles of fire adhering to quick-lime, and thoſe of high rectified ſpirit of wine, which is of ſo igneous a nature, as to be totally inflammable; yet I have not found, that the affuſion of alkool of wine upon quick-lime would produce any ſenſible incaleſcence, or any viſible diſſolution or diſſipation of the lime, as common water would have done, though it ſeemed to be greedily enough ſoaked in by the lumps of lime. And I further tried, that, if on this lime ſo drenched I poured cold water, there enſued no manifeſt heat, nor did I ſo much as find the lump ſwelled, and thereby broken, till ſome hours after; which ſeems to argue, that the texture of the lime was ſuch, as to admit the particles of the ſpirit of wine into ſome of its pores, which were either larger or more congruous, without admitting it into the moſt numerous ones, whereinto the liquor muſt be received, to be able ſuddenly to diſſipate the corpuſcles of lime into their minuter particles, into which (corpuſcles) it ſeems, that the change, that the aqueous particles received by aſſociating with the ſpirituous ones, made them far leſs fit to penetrate and move briſkly there, than if they had entered alone.

I made alſo an experiment, that ſeems to favour our conjecture, by ſhewing, how much the diſpoſition of lime to incaleſcence may depend upon an idoneous texture, and the experiment, as I find it, regiſtered in one of my memorials, is this.

EXPERIMENT V.

[UPON quick-lime we put in a retort as much moderately ſtrong ſpirit of wine, as would drench it, and ſwim a pretty way above it; and then diſtilling with a gentle fire, we drew off ſome ſpirit of wine much ſtronger than that, which had been put on, and then the phlegm following it, the fire was encreaſed, which brought over a good deal of phlegmatick ſtrengthleſs liquor; by which one would have thought, that the quick-lime had been ſlacked; but when the remaining matter had been taken out of the retort, and ſuffered to cool, it appeared to have a fiery diſpoſition, that it had not before. For, if any lump of it, as big as a nutmeg, or an almond, was caſt into the water, it would hiſs as if a coal of fire had been plunged into the liquor, which was ſoon thereby ſenſibly heated. Nay, having kept divers lumps of this prepared calx well covered from the air for divers weeks, to try, whether it would retain this property, I found, as I expected, that the calx operated after the ſame manner, if not more powerfully. For ſometimes, eſpecially when it was reduced to ſmall pieces, it would upon its coming into the water make ſuch a briſk noiſe, as might almoſt paſs for a kind of exploſion.]

THESE phænomena ſeem to argue, that the diſpoſition, that lime has to grow hot with water, depends much on ſome peculiar texture, ſince the aqueous parts, that one would think capable of quenching all, or moſt of the atoms of fire, that are ſuppoſed to adhere to quick-lime, did not near ſo much weaken the diſpoſition of it to incaleſcence, as the acceſſion of the ſpirituous corpuſcles and their contexture, with thoſe of the lime, encreaſed that igneous diſpoſition. And that there might intervene ſuch an aſſociation, ſeems to me the more probable, not only becauſe much of the diſtilled liquor was as phlegmatick, as if it had been robbed of its more active parts, but becauſe I have ſometimes had ſpirit of wine come over with quick-lime not in unobſerved ſteams, but white fumes. To which I ſhall add, that beſides, that the taſte, and perhaps odour of the ſpirit of wine, is often manifeſtly changed by a well-made diſtillation from quick-lime; I have ſometimes found that liquor to give the lime a kind of alcalizate penetrancy, not to ſay fierineſs of taſte, that was very briſk and remarkable. But I will not undertake, that every experimenter, nor I myſelf, ſhall always make trials of this kind with the ſame ſucceſs, that I had in thoſe above recited, in regard, that I have found quick-limes to differ much, not only according to the degree of their calcination,

cination, and to their recentneſs, but alſo, and that eſpecially, according to the differing natures of the ſtones and other bodies calcined. Which obſervation engages me the more to propoſe what hath been hitherto delivered about quick-lime, as only narratives and a conjecture; which I now perceive has detained us ſo long, that I am obliged to haſten to the remaining experiments, and to be the more ſuccinct in delivering them.

EXPERIMENT VI.

AND it will be convenient to begin with an inſtance or two of the production of heat, wherein there appears not to intervene any thing in the part of the agent or patient, but local motion, and the natural effects of it. And as to this ſort of experiments, a little attention and reflection may make ſome familiar phænomenon appoſite to our preſent purpoſe. When, for example, a ſmith does haſtily hammer a nail, or ſuch like piece of iron, the hammered metal will grow exceeding hot, and yet there appears not any thing to make it ſo, ſave the forcible motion of the hammer, which impreſſes a vehement, and variouſly determined agitation of the ſmall parts of the iron; which being a cold body before, by that ſuperinduced commotion of its ſmall parts, becomes in divers ſenſes hot; firſt, in a more lax acceptation of the word in reference to ſome other bodies, in reſpect of whom it was cold before, and then ſenſibly hot; becauſe this newly gained agitation, ſurpaſſes that of the parts of our fingers. And in this inſtance, it is not to be overlooked, that oftentimes neither the hammer, by which, nor the anvil, on which a cold piece of iron is forged, (for all iron does not require precedent ignition to make it obey the hammer) continue cold, after the operation is ended; which ſhews, that the heat acquired by the forged piece of iron was not communicated by the hammer or anvil as heat, but produced in it by motion, which was great enough to put ſo ſmall a body, as the piece of iron, into a ſtrong and confuſed motion of its parts, without being able to have the like operation upon ſo much greater maſſes of metal, as the hammer and the anvil; though, if the percuſſions were often and nimbly renewed, and the hammer were but ſmall, this alſo might be heated, (though not ſo ſoon, nor ſo much, as the iron;) by which one may alſo take notice, that it is not neceſſary, a body ſhould be itſelf hot, to be calorifick. And now I ſpeak of ſtriking an iron with a hammer, I am put in mind of an obſervation, that ſeems to contradict, but does indeed confirm our theory; namely, that if a ſomewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers ſtrokes on the head before it grow hot; but when it is driven to the head, ſo that it can go no further, a few ſtrokes will ſuffice to give it a conſiderable heat; for whilſt, at every blow of the hammer, the nail enters further and further into the wood, the motion, that is produced, is chiefly progreſſive, and is of the whole nail tending one way; whereas, when that motion is ſtopped, then the impulſe given by the ſtroke, being unable either to drive the nail further on, or deſtroy its intireneſs, muſt be ſpent in making a various vehement and inteſtine commotion of the parts among themſelves, and in ſuch an one we formerly obſerved the nature of heat to conſiſt.

EXPERIMENT VII.

IN the foregoing experiment, the briſk agitation of the parts of a heated iron was made ſenſible to the touch. I ſhall now add one of the attempts, that I remember I made, to render it diſcoverable to the eye itſelf. In order to this, and that I might alſo ſhew, that not only a ſenſible, but an intenſe degree of heat, may be produced in a piece of cold iron by local motion, I cauſed a bar of that metal to be nimbly hammered by two or three luſty men, accuſtomed to manage that inſtrument; and theſe ſtriking with as much force, and as little intermiſſion, as they could, upon the iron, ſoon brought it to that degree of heat, that not only it was a great deal too hot to be ſafely touched, but probably would, according to my deſign, have kindled gun-powder, if that, which I was fain to make uſe of, had been of the beſt ſort: for, to the wonder of the by-ſtanders, the iron kindled the ſulphur of many of the grains of the corns of powder, and made them turn blue, though I do not well remember, that it made any of them go off.

EXPERIMENT VIII.

BESIDES the effects of manifeſt and violent percuſſions, ſuch as thoſe we have been taking notice of to be made with a hammer, there are among phænomena obvious enough, ſome, that ſhew the producibleneſs of heat, even in cold iron, by cauſing an inteſtine commotion of its parts: for we find, that, if a piece of iron, of a convenient ſhape and bulk, be nimbly filed with a large rough file, a conſiderable degree of heat will be quickly excited in thoſe parts of the iron where the file paſſes to and fro, the many prominent parts of the inſtrument giving a multitude of ſtrokes or puſhes to the parts of the iron, that happen to ſtand in their way, and thereby making them put the neighbouring parts into a briſk and confuſed motion, and ſo into a ſtate of heat. Nor can it be well objected, that, upon this account, the file itſelf ought to grow as hot as the iron, which yet it will not do; ſince, to omit other anſwers, the whole body of the file being moved to and fro, the ſame parts, that touch the iron this moment, paſs off the next; and, beſides, have leiſure to cool themſelves, by communicating their newly received agitation to the air, before they are brought to grate again upon the iron, which, being ſuppoſed to be held immoveable, receives almoſt perpetual ſhakes in the ſame place.

WE find alſo, that attrition, if it be any thing vehement, is wont to produce heat in the ſolideſt bodies; as when the blade of a knife, being nimbly whetted, grows preſently hot. And if, having taken a braſs nail, and driven it as far as you can to the end of the ſtick, to keep it faſt, and gain a handle, you then ſtrongly rub the head to and fro againſt the floor, or a plank of wood, you may quickly find it to have acquired a heat intenſe enough to offend, if not burn one's fingers. And I remember, that going once, in exceeding hot weather, in a coach, which, for certain reaſons, we cauſed to be driven very faſt; the attrition of the nave of the wheel, againſt the axel-tree, was ſo vehement, as obliged us to light out of the coach, to ſeek for water to cool the over-chafed parts, and ſtop the growing miſchief the exceſſive heat had begun to do.

The vulgar experiment, of ſtriking fire with a flint and ſteel, ſufficiently declares, what a heat, in a trice, may be produced in cold bodies by percuſſion, or colliſion; the latter of which ſeems but mutual percuſſion.

But inſtances of the ſame ſort, with the reſt-mentioned in this VI. experiment, being obvious enough, I ſhall forbear to multiply and inſiſt on them.

EXPERIMENT IX.

For the ſake of thoſe, that think the attrition of contiguous air is neceſſary to the production of manifeſt heat, I thought, among other things, of the following experiment, and made trial of it.

We took ſome hard black pitch, and having, in a baſon, porringer, or ſome ſuch veſſel, placed it a convenient diſtance under water, we caſt on it, with a good burning-glaſs, the ſunbeams, in ſuch a manner, that, notwithſtanding the refraction, that they ſuffered in the paſſage through the interpoſed water, the focus fell upon the pitch; wherein it would produce ſometimes bubbles, ſometimes ſmoke, and quickly communicated a degree of heat capable to make pitch melt, if not alſo to boil.

EXPERIMENT X.

Though the firſt and ſecond experiments of Section I. ſhew, that a conſiderable degree of cold is produced by the diſſolution of ſal armoniac in common water; yet, by an additament, though but ſingle, the texture of it may be ſo altered, that, inſtead of cold, a notable degree of heat will be produced, if it be diſſolved in that liquor. For the manifeſtation of which, we deviſed the following experiment.

We took quick-lime, and ſlaked it in common cold water, that all the igneous, or other particles, to which its power of heating that liquor is aſcribed, might be extracted and imbibed, and ſo the calx freed from them; then, on the remaining powder, freſh water was often poured, that all adhering reliques of ſalt might be waſhed off. After this, the thus dulcified calx, being again well dried, was mingled with an equal weight of powdered ſal armoniac, and having, with a ſtrong fire, melted the maſs, the mixture was poured out; and, being afterwards beaten to powder, having given it a competent time to grow cold, we put two or three ounces of it into a wide-mouthed glaſs; and pouring water upon it, within about a minute of an hour, the mixture grew warm, and quickly attained ſo intenſe a heat, that I could not hold the glaſs in my hand. And though this heat did not long laſt at the ſame height, it continued to be very ſenſible for a conſiderable time after.

EXPERIMENT XI.

To confirm this experiment, by a notable variation, we took finely powdered ſal armoniac, and filings or ſcales of ſteel, and when they were very diligently mixed, (for that circumſtance ought to be obſerved,) we cauſed them to be gradually ſublimed in a glaſs veſſel, giving a ſmart fire towards the latter end. By this operation, ſo little of the mixture aſcended, that, as we deſired, far the greateſt part of the ſal armoniac ſtaid at the bottom with the metal; then, taking out the caput mortuum, I gave it time thoroughly to cool, but in a glaſs well ſtopped, that it might not imbibe the moiſture of the air, (as it is very apt to do.) And laſtly, though the filings of ſteel, as well as the ſal armoniac, were bodies actually cold, and ſo might be thought likely to increaſe, not check the coldneſs wont to be produced in water by that ſalt; yet, putting the mixture into common water, there enſued, as we expected, an intenſe degree of heat. And I remember, that, having ſublimed the forementioned ſalt in diſtinct veſſels, with the filings of ſteel, and with filings of copper, and, for curioſity-ſake, kept one of the caput mortuums (for I cannot certainly call to mind which of the two it was) divers months, (if I miſtake not, eight or nine,) we at length took it out of the veſſel, wherein it had been kept carefully ſtopped; and, upon trial, were not deceived in having expected, that all that while the diſpoſition to give cold water a notable degree of heat, was preſerved in it.

EXPERIMENT XII.

If experiments were made after the above recited manner with ſal armoniac and other mineral bodies than iron and copper, it is not improbable, that ſome of the emerging phænomena would be found to confirm what has been ſaid of the intereſt of texture, (and ſome few other mechanical affections) in the production of heat and cold. Which conjecture is ſomewhat favoured by the following trial. Three ounces of antimony, and an equal weight of ſal armoniac being diligently powdered and mixed, were, by degrees of fire, ſublimed in a glaſs veſſel, by which operation we obtained three differing ſubſtances, which we cauſed to be ſeparately powdered, when they were taken out of the ſubliming glaſs, leſt the air or time ſhould make any change in them; and having before put the ball of a good ſealed weather-glaſs for a while into water, that the ſpirit of wine might be brought to the temper of the external liquor, we put on a convenient quantity of the powdered Caput mortuum, which amounted to two ounces, and ſeemed to be little other than antimony, which accordingly did ſcarce ſenſibly raiſe the ſpirit of wine in the thermoſcope, though that were a tender one. Then laying aſide that water, and putting the inſtrument into freſh, of the ſame temper, we put to it a very yellow ſublimate, that aſcended higher than the other parts, and ſeemed to conſiſt of the more ſulphureous flowers of the antimony, with a mixture of the more volatile parts of the ſal armoniac. And this ſubſtance made the tincted ſpirit in the thermoſcope deſcend very ſlowly about a quarter of an inch; but when the inſtrument was put into freſh water of the ſame temper, and we had put in ſome of the powder of the lower ſort of ſublimate, which was dark coloured, though both the antimony and ſal armoniac, it conſiſted of, had been long expoſed to the action of a ſubliming heat; yet the water was thereby ſpeedily and notably cooled, inſomuch, that the ſpirit of wine in the weather-glaſs haſtily deſcended, and continued to ſink, till, by our gueſs, it had fallen not much ſhort of three inches. Of theſe phænomena the ætiology, as ſome moderns call the theory, which propoſes the cauſes of things, is more eaſy to be found by a little conſideration, than to be made out in few words.

We made alſo an experiment like that above recited, by ſubliming three ounces a piece of minium and ſal armoniac; in which trial we found, that though, in the Caput mortuum, the ſalt had notably wrought upon the calx of lead, and was in part aſſociated with it, as appeared by the whiteneſs of the ſaid Caput mortuum, by its ſweetiſh taſte, and by the weight (which exceeded, four drachms, that of all the minium;) yet a convenient quantity of this powdered mixture being put into water, wherein the former weather-glaſs had been kept a while, the tincted ſpirit of wine was not manifeſtly either raiſed or depreſſed. And when, in another glaſs, we proſecuted the trial with the ſal armoniac, that had been ſublimed from the minium, it did indeed make the ſpirit of wine deſcend, but ſcarce a quarter ſo much as it had been made to fall by the lately mentioned ſublimate of ſal armoniac and antimony.

EXPERIMENT XIII.

It is known, that many learned men, beſides ſeveral chemical writers, aſcribe the incaleſcences, that are met with in the diſſolution of metals, to a conflict ariſing from a certain antipathy or hoſtility, which they ſuppoſe between the conflicting bodies, and particularly between the acid ſalt of the one, and the alcalizate ſalt, whether fixed or volatile, of the other. But ſince this doctrine ſuppoſes a hatred between inanimate bodies, in which it is hard to conceive, how there can be any true paſſions, and does not intelligibly declare, by what means their ſuppoſed hoſtility produces heat; it is not likely, that, for theſe and ſome other reaſons, inquiſitive naturaliſts will eaſily acquieſce in it. And on the other ſide it may be conſidered, whether it be not more probable, that heats, ſuddenly produced in mixtures, proceed either from a very quick and copious diffuſion of the parts of one body through thoſe of another, whereby both are confuſedly tumbled and put into a calorific motion; or from this, that the parts of the diſſolved body come to be every way, in great numbers, violently ſcattered; or from the fierce and confuſed ſhocks or juſtlings of the corpuſcles of the conflicting bodies, or maſſes, which may be ſuppoſed to have the motions of their parts differingly modified according to their reſpective natures: or from this, that, by the plentiful ingreſs of the corpuſcles of the one into the almoſt commenſurate parts of the other, the motion of ſome etherial matter, that was wont before ſwiftly to permeate the diſtinct bodies, comes to be checked and diſturbed, and forced to either brandiſh or whirl about the parts in a confuſed manner, till it have ſettled itſelf a free paſſage through the new mixture, almoſt as the light does through divers troubled liquors and vitrified bodies, which, at length, it makes tranſparent. But, without here engaging in a ſolemn examination of the hypotheſis of alcali and acidum, and without determining whether any one, or more, of the newly mentioned mechanical cauſes, or whether ſome other, that I have not yet named, is to be entitled to the effect; it will not be impertinent to propoſe divers inſtances of the production of heat by the operation of one agent, oil of vitriol, that it may be conſidered whether it be likely, that this ſingle agent ſhould, upon the ſcore of antipathy, or that of its being an acid menſtruum, be able to produce an intenſe heat in many bodies of ſo differing natures as are ſome of thoſe, that we ſhall have occaſion to name. And now I proceed to the experiments themſelves.

Take ſome ounces of ſtrong oil of vitriol, and ſhaking it with three or four times its weight of common water, though both the liquors were cold, when they were put together, yet their mixture will, in a trice, grow intenſely hot, and continue conſiderably ſo for a good while. In this caſe it cannot probably be pretended by the chemiſts, that the heat ariſes from the conflict of the acid and alcalizate ſalts abounding in the two liquors, ſince the common water is ſuppoſed an elementary body devoid of all ſalts; and at leaſt, being an inſipid liquor, it will ſcarce be thought to have alcali enough to produce, by its re-action ſo intenſe a heat. That the heat emergent upon ſuch a mixture may be very great, when the quantities of the mingled liquors are conſiderably ſo, may be eaſily concluded from one of my memorials, wherein I find, that no more than two ounces of oil of vitriol being poured (but not all at once) into four ounces only of diſtilled rain-water, made and kept it manifeſtly warm for a pretty deal above an hour, and during no ſmall part of that time, kept it ſo hot, that it was troubleſome to be handled.

EXPERIMENT XIV.

THE former experiment brings into my mind one, that I mention, without teaching it in the hiſtory of cold, and it appeared very ſurpriſing to thoſe, that knew not the ground of it. For having ſometimes merrily propoſed to heat cold liquors with ice, the undertaking ſeemed extravagant, if not impoſſible, but was eaſily performed by taking out of a baſon of cold water, wherein divers fragments of ice were ſwimming, one or two pieces, that I perceived were well drenched with the liquor, and immerſing them ſuddenly into a widemouthed glaſs, wherein ſtrong oil of vitriol had been put; for this menſtruum, preſently mingling with the water, that adhered to the ice, produced in it a briſk heat, and that ſometimes with a manifeſt ſmoke, which nimbly diſſolved the contiguous parts of ice, and thoſe the next, and ſo the whole ice being ſpeedily reduced to water, and the corroſive menſtruum being, by two or three ſhakes, well diſperſed through it, and mingled with it, the whole mixture would grow, in a trice, ſo hot, that ſometimes the vial, that contained it, was not to be endured in one's hand.

EXPERIMENT XV.

NOTWITHSTANDING the vaſt difference betwixt common water and high rectified ſpirit of wine, whereof men generally take the former for the moſt contrary body to fire, and whereof the chemiſts take the latter to be but a kind of liquid ſulphur, ſince it may preſently be all reduced into flame; yet, as I expected, I found, upon trial, that oil of vitriol, being mingled with pure ſpirit of wine, would as well grow hot, as with common water. Nor does this experiment always require great quantities of the liquors. For when I took but one ounce of ſtrong oil of vitriol, though I put to it leſs than half an ounce of choice ſpirit of wine, yet thoſe two being lightly ſhaken together, did, in a trice, conceive ſo briſk a heat, that they almoſt filled the vial with fumes, and made it ſo hot, that I had, unawares, like to have burned my hand with it before I could lay it aſide.

I made the like trial with the ſame corroſive menſtruum, and common aqua vitæ, bought at a ſtrong-water-ſhop, by the mixture of which liquors heat was produced in the vial, that I could not well endure.

The like ſucceſs I had in an experiment, wherein oil of vitriol was mixed with common brandy; ſave that in this the heat produced ſeemed not ſo intenſe as in the former trial, which itſelf afforded not ſo fierce a heat, as that, which was made with rectified ſpirit of wine.

EXPERIMENT XVI.

THOSE chemiſts, who conceive, that all the incaleſcences of bodies, upon their being mixed, proceed from their antiquity or hoſtility, will not perhaps expect, that the parts of the ſame body, (either numerically, or in ſpecie, as the ſchools phraſe it,) ſhould, and that without manifeſt conflict, grow very hot together. And yet having for trial's ſake put two ounces of colcothar ſo ſtrongly calcined, that it was burnt almoſt to blackneſs, into a retort, we poured upon it two ounces of ſtrong oil of Engliſh vitriol, and found, that after about a minute of an hour they began to grow ſo hot, that I could not endure to hold my hand to the bottom of the veſſel, to which the mixture gave a heat, that continued ſenſible on the outſide for between twenty and thirty minutes.

EXPERIMENT XVII.

THOUGH I have not obſerved any liquor to equal oil of vitriol in the number of liquors, with which it will grow hot; yet I have not met with any liquor, wherewith it came to a greater incaleſcence, than it frequently enough did with common oil of turpentine. For when we cauſed divers ounces of each to be well ſhaken together in a ſtrong veſſel, faſtened, to prevent miſchief, to the end of a pole or ſtaff; the ebullition was great and fierce enough to be not undeſervedly admired by the ſpectators. And this brings into my mind a pleaſant adventure afforded by theſe liquors, of each of which, having for the production of heat and other purpoſes, cauſed a good bottle full to be put up with other things into a box, and ſent down into the country, with a great change, that care ſhould be had of the glaſſes; the waggon, in which the box was carried, happened, by a great jolt, that had almoſt overturned it, to be ſo rudely ſhaken, that theſe glaſſes were both broken, and the liquors, mingling in the box, made ſuch a noiſe and ſtink, and ſent forth ſuch quantities of ſmoke by the vents, which the fumes had opened to themſelves, that the paſſengers with great outcries and much haſte threw themſelves out of the waggon, for fear of being burnt in it.

THE trials we made with oil of turpentine, when ſtrong ſpirit of nitre was ſubſtituted in the ſtead of oil of vitriol, belong not to this place.

EXPERIMENT XVIII.

BUT though petroleum, eſpecially when rectified, be, as I have elſewhere noted, a moſt ſubtile liquor, and the lighteſt I have yet had occaſion to try; yet to ſhew you, how much the incaleſcence of liquors may depend upon their texture, I ſhall add, that having mixed by degrees one ounce of rectified petroleum, with an equal weight of ſtrong oil of vitriol, the former liquor ſeemed to work upon the ſurface of this laſt named, almoſt like a menſtruum, upon a metal, in numerous and ſmall bubbles continually aſcending for a while into the oleum petræ, which had its colour manifeſtly altered and deepened by the operation of the ſpirituous parts. But by all the action and re-action of theſe liquors, there was produced no ſuch ſmoking and boiling, or intenſe heat, as if oil of turpentine had been employed inſtead of oil of vitriol; the change, which was produced, as to qualities, being but a kind of tepidneſs diſcoverable by the touch.

Almost the like ſucceſs we had in the conjunction of petroleum and ſpirit of nitre; a more full account whereof may be elſewhere met with.

In this, and the late trials, I did not care to make uſe of ſpirit of ſalt, becauſe, at leaſt, if it be but ordinarily ſtrong, I found its operation on the liquors above-mentioned inconſiderable, (and ſometimes perhaps ſcarce ſenſible) in compariſon of thoſe of oil of vitriol, and in ſome caſes of dephlegmed ſpirit of nitre.

EXPERIMENT XIX.

Experienced chemiſts will eaſily believe, that it were not difficult to multiply inſtances of heat producible by oil of vitriol upon ſolid bodies, eſpecially mineral ones. For it is known, that, in the uſual preparation of vitriolum martis, there is a great efferveſcence excited upon the affuſion of the oil of vitriol upon filings of ſteel, eſpecially, if they be well drenched in common water. And it will ſcarce be doubted, but that, as oil of vitriol will (at leaſt partly) diſſolve a great many, both calcined and teſtaceous bodies, as I have tried with lime, oyſter-ſhells, &c. ſo it will, during the diſſolution, grow ſenſibly, if not intenſely hot with them, as I found it to do, both with thoſe newly named, and others, as chalk, lapis calaminaris, &c. with the laſt of which, if the liquor be ſtrong, it will heat exceedingly.

EXPERIMENT XX.

Wherefore I will rather take notice of its operation upon vegetable, as bodies, which corroſive menſtuums have ſcarce been thought fit to diſſolve and grow hot with. To omit then cherries, and divers fruits abounding in watery juices, with which, perhaps on that very account, oil of vitriol will grow hot; I ſhall here take notice, that, for trial ſake, having mixed a convenient quantity of that liquor with raiſins of the ſun beaten in a mortar, the raiſins grew ſo hot, that, if I miſ-remember not, the glaſs, that contained it, had almoſt burnt my hand.

These kind of heats may be alſo produced by the mixture of oil of vitriol with divers other vegetable ſubſtances; but, as far as I have obſerved, ſcarce ſo eminently with any dry body, as with the crumbs of white bread, (or even of brown,) with a little of which we have ſometimes produced a ſurpriſing degree of heat, with ſtrong or well-dephlegmed oil of vitriol, which is to be ſuppoſed to have been employed in the foregoing experiments, and all others mentioned to be made by the help of that menſtruum in our papers about qualities, unleſs it be in any particular caſe otherwiſe declared.

EXPERIMENT XXI.

It is as little obſerved, that corroſive menſtruums are able to work, as ſuch, on the ſoft parts of dead animals, as on thoſe of vegetables; and yet I have, more than once, produced a notable heat, by mixing oil of vitriol with minced fleſh, whether roaſted or raw.

EXPERIMENT XXII.

Though common ſea-ſalt does uſually impart ſome degree, though not an intenſe one, of coldneſs unto common water, during the act of diſſolution; yet ſome trials have informed me, that, if it were caſt into a competent quantity of oil of vitriol, there would, for the moſt part, enſue an incaleſcence, which yet did not appear to ſucceed ſo regularly, as in moſt of the foregoing experiments. But, that heat ſhould be produced uſually, though not perhaps conſtantly, by the above-named menſtruum and ſalt, ſeems therefore worthy of our notice, becauſe it is known to chemiſts, that common ſalt is one main ingredient of the few, that make up common factitious ſal armoniac, that is wont to be ſold in the ſhops. And I have been informed, that the excellent academians of *Florence* have obſerved, that oil of vitriol would not grow hot, but cold, by being put upon ſal armoniac: ſomething like which, I took notice of in rectified ſpirit of ſulphur, made *per campanam*, but found the effect much more conſiderable, when, according to the ingenious Florentine experiment, I made the trial with oil of vitriol; which liquor, having already furniſhed us with as many phænomena for our preſent purpoſe, as could be well expected from one agent, I ſhall ſcarce, in this paper about heat, make any farther uſe of it, but proceed to ſome other experiments, wherein it does not intervene.

EXPERIMENT XXIII.

We took a good lump of common ſulphur, of a convenient ſhape, and, having rubbed or chafed it well, we found, as we expected, that, by this attrition, it grew ſenſibly warm; and, that there was an inteſtine agitation, which you know is local motion, made by this attrition, did appear not only by the newly mentioned heat, whoſe nature conſiſts in motion, and by the antecedent preſſure, which was fit to put the parts into a diſorderly vibration, but alſo by the ſulphureous ſteams, which it was eaſy to ſmell, by holding the ſulphur to one's noſe as ſoon as it had been rubbed. Which experiment, though it may ſeem trivial in itſelf, may be worth the conſideration of thoſe chemiſts, who would derive all the fire and heat we meet with in ſublunary bodies from ſulphur. For, in our caſe, a maſs of ſulphur, before its parts were put into a new and briſk motion, was ſenſibly cold; and as ſoon as its parts were put into a greater agitation than thoſe of a man's fingers, it grew ſenſibly hot; which argues, that it was not by its bare preſence, or any emanative action, (as the ſchools ſpeak,) that the ſulphur communicated any heat to my hand; and alſo, that, when it was briſkly moved, it did impreſs that quality, was no more than another ſolid body, though incombuſtible as common glaſs, would have done, if its parts had been likewiſe put into an agitation ſurpaſſing that of my organs of feeling; ſo that, in our experiment, ſulphur itſelf was beholden, for its actual heat, to local motion, produced by external agents in its parts.

EXPERIMENT XXIV.

WE thought it not amiſs to try, whether, when ſal armoniac, that much infrigidates water, and quick-lime, which is known to heat it, were by the fire exquiſitely mingled, the mixture would impart to the liquor a moderate or an intenſe degree of either of thoſe qualities. In proſecution of which enquiry, we took equal parts of ſal armoniac and quick-lime, which we fluxed together, and putting an ounce, by gueſs, of the powdered mixture into a vial, with a convenient quantity of cold water, we found, that the colliquated maſs did, in about a minute, ſtrike ſo great a heat through the glaſs upon my hand, that I was glad to remove it haſtily, for fear of being ſcorched.

EXPERIMENT XXV.

WE have given ſeveral, and might have given many more, inſtances of the incaleſcence of mixtures, wherein both the ingredients were liquors, or, at leaſt, one of them was a fluid body. But ſometimes heat may alſo be produced by the mixture of two powders; ſince it has been obſerved, in the preparation of the butter or oil of antimony, that, if a ſufficient quantity of beaten ſublimate be very well mingled with powdered antimony, the mixture, after it has, for a competent time (which varies much according to circumſtances, as the weather, veſſel, place, &c. wherein the experiment is made,) ſtood in the air, would ſometimes grow manifeſtly hot, and now and then ſo intenſely ſo, as to ſend forth copious and fetid fumes, almoſt as if it would take fire. There is another experiment, made by the help of antimony, and a pulverized body, wherein the mixture, after it had been for divers hours expoſed to the air, viſibly afforded us mineral fumes. And to theſe I could add more conſiderable, and perhaps ſcarce credible inſtances of bodies growing hot without liquors, if philanthropy did not forbid me. But, to return to our butter of antimony, it ſeems not unfit to be enquired, whether there do not unobſervedly intervene an aqueous moiſture, which (capable of relaxing the ſalts, and ſetting them a-work,) I therefore ſuſpected might be attracted (as men commonly ſpeak) from the air, ſince the mixture of the antimony and the ſublimate is preſcribed to be placed in cellars; and in ſuch we find, that ſublimate, or at leaſt the ſaline part of it, is reſolved *per deliquium*, (as they call it,) which is nothing but a ſolution made by the watery ſteams wandering in the air.

EXPERIMENT XXVI.

I Have formerly delivered ſome inſtances of the incaleſcence produced by water in bodies, that are readily diſſolved in it, as ſalt of tartar and quick-lime. But one would not lightly expect, that mere water ſhould produce an incaleſcence in ſolid bodies, that are generally granted to be inſoluble in it; and are not wont to be, at leaſt without length of time, viſibly wrought on by it; and yet trial has aſſured me, that a notable incaleſcence may be produced by common water in flower or fine powder of ſulphur, and filings of ſteel or iron. For when, in ſummer-time, I cauſed to be mingled a good quantity, (as half a pound, or rather a pound, of each of the ingredients,) and cauſed them to be thoroughly drenched with common water, in a convenient quantity whereof they were very well ſtirred up and down, and carefully mingled, the mixture would, in a ſhort time, perhaps leſs than an hour, grow ſo hot, that the veſſel, that contained it, could not be ſuffered in one's hand; and the heat was manifeſted to other ſenſes than the touch, by the ſtrong ſulphureous ſtink, that invaded the noſe, and the thick ſmoke, that aſcended out of the mixture, eſpecially, when it was ſtirred with a ſtick or ſpattle. Whether the ſucceſs will be the ſame at all times of the year, I do not know, and ſomewhat doubt, ſince I remember not, that I had occaſion to try it in other ſeaſons, than in ſummer, or in autumn.

EXPERIMENT XXVII.

IN the inſtances, that chemiſtry is wont to afford us of the heat produced by the action of menſtruums upon other bodies, there intervenes ſome liquor, properly ſo called, that wets the hands of thoſe, that touch it; and there are divers of the more judicious chemiſts, that join with the generality of the naturaliſts in denying, that quickſilver, which is indeed a fluid body, but not a moiſt and wetting one, in reference to us, will produce heat by its immediate action on any other body, and particularly on Gold. But, though I was long inclinable to their opinion, yet I cannot now be of it, ſeveral trials having aſſured me, that a mercury, whether afforded by metals and minerals, or impregnated by them, may, by its preparation, be enabled to inſinuate itſelf nimbly into the body of gold, whether calcined or crude, and become manifeſtly incaleſcent with it in leſs than two or three minutes of an hour.

EXPERIMENT XXVIII.

SINCE we know, that ſome natural ſalts, and eſpecially ſalt-petre, can produce a coldneſs in the water they are diſſolved in, I thought it might not be impertinent to our enquiry into heat and cold, and might perhaps alſo contribute ſomewhat to the diſcovery of the ſtructure of metals, and the ſalts, that corrode them, if ſolutions were made of ſome ſaliformed bodies, as chemiſts call them, that are made up of metalline and ſaline parts, and do ſo abound with the latter, that the whole concretions are, on their account, diſſoluble in common water.

OTHER experiments of this ſort belonging leſs to this place than to another, I ſhall here only, for example-ſake, take notice of one, that we made upon quickſilver, which is eſteemed the coldeſt of metals. For having, by diſtilling from it four times its weight of oil of vitriol, reduced it to a powder, which, on the account of the adhering ſalts of the menſtruum, that it detained, was white and gliſtering; we put this powder into a wide-mouthed glaſs of water, wherein a ſealed weather-glaſs had been left, before it began manifeſtly to heat the water, as appeared by the quick and conſiderable aſcent of the tincted ſpirit of wine, that continued to riſe, upon putting in more of the magiſtery; which warm event is the more remarkable, becauſe of the obſervation of *Helmont*, that the ſalt, adhering to the mercury, corroded in good quantity by oil of vitriol, if it be waſhed off, and coagulated, becomes a kind of alom.

THE event of the former trial deſerves the more notice, becauſe, having, after the ſame manner, and with the ſame weather-glaſs, made an experiment of common water, and the powder of vitriolum martis, made with oil of vitriol and the filings of ſteel, the tincted ſpirit of wine was not at all impelled up as before, but rather, after a while, began to ſubſide, and fell though very ſlowly, about a quarter of an inch. The like experiment being tried with powdered ſublimate in common water, the liquor in the thermoſcope was ſcarce at all ſenſibly either raiſed or depreſſed, which argued the alteration, as to heat or cold, to have been either none, or very inconſiderable.

HAVING given warning at the beginning of this ſection, that in it I aimed rather at offering various, than numerous experiments about the production of heat, I think, what has been already delivered may allow me to take leave of this ſubject, without mentioning divers inſtances, that I could eaſily add, but think it fitter at preſent to omit. For thoſe afforded me by trials about antiperiſtaſis belong to a paper on that ſubject. Thoſe, that might be offered about potential heat in human bodies, would, perchance, be thought but unneceſſary, after what has been ſaid of potential coldneſs; from which an attentive conſiderer may eaſily gather what, according to our doctrine, is to be ſaid of the contrary quality. And divers phænomena, which would have been of the moſt conſiderable, I could have mentioned of the production of heat, ſince in them that quality is the moſt exalted, I reſerve for the title of combuſtibleneſs and incombuſtibility, having already ſuffered this collection (or rather chaos) of particulars about the production of heat, to ſwell to too great a bulk.

EXPERIMENTS AND OBSERVATIONS, ABOUT THE Mechanical PRODUCTION of TASTES.

TO make out the mechanical origin, or production of ſapors, as far as is neceſſary for my preſent purpoſe, it will be expedient to premiſe in general, that, according to our notion of taſtes, they may depend upon the bigneſs, figure, and motion of the ſaporifick corpuſcles, conſidered ſeparately, and as the affections of ſingle, and very minute particles of matter; or elſe in a ſtate of conjunction, as two or more of theſe affections, and the particles they belong to, may be combined or aſſociated, either among themſelves, or with other particles, that were not ſaporous before. And as theſe coalitions, and other aſſociations come to be diverſified; ſo the taſtes, reſulting from them, will be altered or deſtroyed.

BUT, to handle theſe diſtinctly, and fully, were a taſk not only too difficult and long, but improper in this place, where I pretend to deliver not ſpeculations, but matters of fact: in ſetting down whereof, nevertheleſs, to avoid too much confuſion, I am content, where I can do it readily and conveniently, in ſome of my trials, to couch ſuch references, as may beſt point at thoſe heads, whence the mechanical explications may be derived, and conſequently our doctrine confirmed.

BY taſte conſidered, as belonging to the object, (under which notion I here treat of it,) I mean, that quality, or whatever elſe it be, which enables a body, by its operation, to produce in us that ſenſation, which we feel, or perceive, when we ſay we taſte.

THAT this ſomething, whether you will call it a quality, or whatever elſe it be, that makes, or denominates an object ſaporous, or rather (if I may be allowed a barbarous term) ſaporifick, may ſo depend upon the ſhape, ſize, motion, and other mechanical affections of the ſmall parts of the taſted body, and reſult from the aſſociation of two or more of them, not excluding their congruity, or incongruity to the organs of taſting, may be made probable by the following inſtances.

EXPERIMENT I.

To divide a body, almoſt inſipid, into two bodies of very ſtrong, and very differing taſtes.

IT is obſerved, that ſalt-petre refined, and by that purification freed from the ſea-ſalt, that is wont to be mingled with it, does rather cool the tongue, than make any great ſaporifick impreſſions on it. And though I will not ſay, that it is, as ſome have thought, an inſipid body; yet the bitteriſhneſs, which ſeems to be its proper taſte, is but very faint and languid. And yet this almoſt inſipid body, being diſtilled by the way of inflammation, (which I elſewhere teach,) or even by the help of an additament of ſuch clay, as is itſelf a taſteleſs body, will afford a nitrous ſpirit, that is extremely ſharp or corroſive upon the tongue, and will diſſolve ſeveral metals themſelves, and a fixed ſalt, that is likewiſe very ſtrongly taſted, but of a taſte altogether different from that of the ſpirit, that is extremely ſharp or corroſive upon the tongue; and accordingly, this ſalt will diſſolve divers compact bodies, that the other will not work on, and will precipitate divers metals, and other concretes, out of thoſe ſolutions, that have been made of them by the ſpirit.

EXPERIMENT II.

Of two bodies, the one highly acid and corroſive, and the other alkalizate and fiery, to produce a body almoſt inſipid.

THIS may be performed by the way I have elſewhere mentioned of compoſing ſalt-petre. For, if upon a liquor of fixed nitre, made *per deliquium*, you warily drop good ſpirit of nitre, till it be juſt enough to ſatiate the alkaly, (for if there be too much, or too little, the experiment may miſcarry,) we may, by a gentle evaporation, and ſometimes without it, and that in a few minutes, obtain cryſtals, which, being dried after they have been, if it be needful, freed from any adhering particles, (not of their own nature,) will have upon the tongue, neither a ſharp nor an alkalizate taſte, but that faint, and ſcarce ſenſible bitterneſs, that belongs to ſalt-petre, if it be pure ſalt-petre; for the impure may, perhaps, ſtrongly reliſh of the common ſalt, that is uſually contained in it.

THE like production of ſalt-petre we have ſometimes made in far leſs time, and ſometimes indeed in a trice, by ſubſtituting, inſtead of the fixed ſalt of nitre, the ſaline parts of good pot-aſhes, carefully freed by ſolution and filtration from the earthy and feculent ones.

I have ſometimes conſidered, whether the phænomena of theſe two experiments may not be explicated, by ſuppoſing them to ariſe from the new magnitudes and figures of the particles, which the fire, by breaking them, or forcibly rubbing them one againſt the other, or alſo againſt the corpuſcles of the additament, may be preſumed to give them; as if, for example, ſince we find the larger and beſt formed cryſtals of nitre to be of a priſmatical ſhape with ſix ſides, we ſhould ſuppoſe the corpuſcles of nitre to be little priſms, whoſe angles and ends are too obtuſe or blunt to make vigorous and deep impreſſions on the tongue; and yet, if theſe little priſms be by a violent heat ſplit, or otherwiſe broken, or forcibly made, as it were, to grind one another, they may come to have parts ſo much ſmaller than before, and endowed with ſuch ſharp ſides, and angles, that, being diſſolved and agitated by the ſpittle, that uſually moiſtens the tongue, their ſmallneſs may give them great acceſs to the pores of that organ, and the ſharpneſs of their ſides and points may fit them to ſtab and cut, and perhaps, ſear the nervous and membranous parts of the organ of taſte, and that variouſly, according to the grand diverſities, as to ſhape and bulk of the ſaporifick particles themſelves. And this being granted, it ſeemed further conceivable, that when the alkalizate and acid particles come to be put together in the fluid mixture, wherein they ſwam, many of them might, after a multitude of various juſtlings and occurſions, meet with one another ſo luckily and opportunely, as to re-compoſe little priſms, or convene into other bodies, almoſt like thoſe, that made up the cryſtals of nitre, before it was expoſed to the fire. To illuſtrate which, we may conceive, that, though a priſm of iron may be ſo ſhaped, that it will be wholly unfit to pierce the ſkin; yet it may be ſo cut by tranſverſe planes reaching to the oppoſite baſes or ends, as to afford wedges, which, by the ſharpneſs of their edges, may be fit both to cleave wood, and cut the ſkin; and theſe wedges, being again put together, after a requiſite manner, may re-compoſe a priſm, whoſe extremes ſhall be too blunt to be fit for the former uſe. This may be alſo illuſtrated by the breaking of a dry ſtick, circularly cut off at the ends, which, though it is unapt, whilſt entire, and of that bulk, to prick the hand; yet if it be violently broken, the ragged ends of it, and the ſplinters, may prove ſtiff, ſlender, and ſharp enough to pierce and run into the hand: to which divers other ſuch mechanical illuſtrations might be added. But, ſince I fear you think, as well as I, the main conjecture may not be worthy any farther proſecution, I ſhall not inſiſt any longer on it. And becauſe the hiſtorical part of theſe experiments was for the main delivered by me already in the eſſay about the analyſis and redintegration of nitre, I ſhall now proceed to other trials.

EXPERIMENT III.

Of two bodies, the one extremely bitter, and the other exceeding ſalt, to make an inſipid mixture.

TO make this experiment, we muſt very warily pour upon cryſtals made of ſilver, diſſolved in good aqua fortis or ſpirit of nitre, ſtrong brine made of common ſalt and water. For the mixture of theſe two being dried, and afterwards brought to fuſion in a crucible, and kept a competent while in that ſtate, will afford a tough maſs, the chemiſts call luna cornea, which you may lick divers times, and ſcarce judge it other than inſipid; nor will it eaſily be brought to diſſolve in much more piercing menſtruums than our ſpittle, as I have elſewhere ſhewn.

EXPERIMENT IV.

Of two bodies, the one extremely ſweet, and the other ſalter than the ſtrongeſt brine, to make an inſipid mixture.

THE doing of this requires ſome ſkill and much warineſs in the experimenter, who, to perform it well, muſt take a ſtrong ſolution of minium, made with an appropriated menſtruum, as good ſpirit of vinegar, or elſe ſaccharum ſaturni itſelf, diſſolved in a convenient vehicle; and then muſt have great care and caution to put to it, by degrees, a juſt proportion of ſtrong ſpirit of ſal armoniac, or the like urinous ſpirit, till the whole be precipitated; and if the two former taſtes are not ſufficiently deſtroyed in the mixture, it may be dried and fluxed, as was above directed about luna cornea.

EXPERIMENT V.

Of an inſipid body and a ſour one, to make a ſubſtance more bitter than gall or aloes.

THIS is eaſily performed by diſſolving in ſtrong ſpirit of nitre or good aqua-fortis as much pure ſilver as the menſtruum will take up; for this ſolution, being filtrated, has been often eſteemed more bitter than ſo much gall or wormwood, or any other of thoſe ſimples, that have been famous for that quality: and if the ſuperfluous moiſture be abſtracted, you may by coagulation obtain cryſtals of luna, that have been judged more ſtrongly bitter than the ſolution itſelf. And that the corpuſcles of theſe cryſtals ſhould leave a far more laſting taſte of themſelves, than the above-mentioned bitter bodies are wont to do, will not ſeem ſo marvellous, as I remember ſome, that tried, have complained; if we take notice, how deep the particles of theſe cryſtals may pierce into the ſpungy organs of taſte, ſince, if one does but touch the pulp or nail of one's finger, (firſt a little wetted with ſpittle or otherwiſe,) with the powder of theſe cryſtals, they will ſo penetrate the ſkin or nail, and ſtick ſo faſt there, that you cannot in a reaſonable time waſh the ſtain off of the ſkin, and much leſs off of the nail, but it will continue to appear many hours on the former, and many days on the other.

EXPERIMENT VI.

Of an inſipid body and a highly corroſive one, to make a ſubſtance as ſweet as ſugar.

THIS is eaſily done, by putting upon good minium purified aqua-fortis or ſpirit of nitre, and letting them work upon one another in a gentle heat, till the liquor have diſſolved its full proportion of the metal. For then, if the ingredients were good, and the operation rightly performed, the menſtruum would have a ſweetneſs like that of ordinary ſaccharum ſaturni. But it was not for nothing, that I intimated, the ingredients ſhould be alſo pure and good in their kind; for, if the minium be adulterated, as often it is, or the ſpirit of nitre or aqua-fortis be mingled, as it is uſual before it be purged with ſpirit of common ſalt, or other unfit ingredients, the operation may be ſucceſsleſs, as I have more than once obſerved.

EXPERIMENT VII.

Of obtaining, without addition from the ſweeteſt bodies, liquors corroſive enough to diſſolve metals.

IF ſugar be put into a ſufficiently capacious retort, and warily diſtilled, (for otherwiſe it will be apt to break the veſſel) it will afford, among other things, a copious red ſpirit, which, being ſlowly rectified, will loſe its colour, and come over clear. The *caput mortuum* of the ſugar, which I have more than once had of an odd contexture, may be found either almoſt or altogether inſipid. And though the ſpirit will be of a very penetrant taſte, yet it will be very far from any kind of ſweetneſs; and though that liquor be thought to be homogeneous, and to be one of the principles of the analized ſugar, yet (as I have elſewhere ſhewn) I found it to be a mixture of two ſpirits; with the one of which, beſides bodies of a leſs cloſe texture, I diſſolved (even in the cold) crude copper, as was eaſy to be ſeen by the deep and lovely colour of the ſolution. And to theſe ſour ſpirits, afforded by ſugar itſelf, we have reſtored a kind of ſaccharine ſweetneſs, by compounding them with the particles of ſo inſipid a body as minium; part of which they will in digeſtion diſſolve. A like ſpirit to that diſtilled from ſugar may be obtained from honey; but in regard of its aptneſs to ſwell exceedingly, chemiſts are not wont to diſtil it without ſand, brick, or ſome other additament.

EXPERIMENT VIII.

To divide a body, bitter in the higheſt degree, into two ſubſtances, the one extremely ſour, and the other perfectly inſipid.

THIS is eaſily done by putting ſome fine cryſtals of *Luna* into a good retort, and then diſtilling them in a ſand-furnace, capable of giving them ſo ſtrong a fire, as to drive away all the ſpirits from the ſilver. For, this remaining behind, according to its metalline nature, will be inſipid, and the ſpirits, that are driven away from it, will unite in the receiver into an acid and corroſive menſtruum.

EXPERIMENT IX.

To produce variety of taſtes in one inſipid body, by aſſociating it with divers menſtruums.

AS this operation may, upon the account I elſewhere mention, be ſerviceable to inveſtigate the figures of the particles of diſſolved metals and other bodies; ſo it is very fit to maniſeſt, what we would here have it ſhew, how much taſte may be diverſified by, and conſequently depend upon texture; ſince a body, that has no taſte, may, in conjunction with ſapid bodies, give them ſtrong taſtes, all differing from one another, and each of them from that, which the ſaporous bodies had before. I could propoſe divers ways of bringing this to trial, there being ſeveral inſipid bodies, which I have found this way diverſifiable. But becauſe I remember not, that I have met with any mineral, that is diſſoluble by near ſo many ſaline menſtruums, as zink, I look on that, as the moſt fertile ſubject to afford inſtances to our preſent purpoſe. For I have found, that it will be diſſolved, not only by aqua-fortis, aqua regis, oil of vitriol, ſpirit of nitre, ſpirit of ſalt, and other mineral menſtruums, but alſo by vegetable ſpirits, as diſtilled vinegar, and by animal ones too, as ſpirit of ſal armoniac; though the one be acid, and the other urinous. And if the ſeveral ſolutions, which may be made of this mineral, by ſo many differing liquors, be compared, the number of their differing taſtes will ſuffice to make good the title of the experiment.

EXPERIMENT X.

To produce variety of taſtes with one menſtruum, by aſſociating it with inſipid bodies.

THIS propoſition a mathematician would go near to call the converſe of the foregoing; and as it may ſerve, as well as that, to diſcover the ſtructure of the minute parts of divers metalline and mineral bodies; ſo it may not only as well, but better than that, ſerve us to illuſtrate the corpuſcularian doctrine of taſtes, by ſhewing us, that a ſingle, and, as far as chemiſtry teaches us, a ſimple body, endowed with a peculiar taſte, may, by being compounded with others, each of them inſipid of itſelf, produce a conſiderable number of differing taſtes. There may be more inſtruments than one made uſe of in this trial; but of thoſe, that are known, and we may eaſily obtain, the moſt proper are ſpirit of nitre, and good aqua-fortis: for that, with refined ſilver, will make a ſolution bitter as gall; with lead, it will be of a ſaccharine ſweetneſs; with that part of tin, which it will keep diſſolved, (for the greateſt it is wont but to corrode and precipitate) it produces a taſte very diſtant from both the former, but not odious; with copper, it affords an abominable taſte; with mercury and iron, it affords other kinds of bad taſtes. Nor are metals the only mineral bodies it will work upon; for, it will diſſolve tin-glaſs, antimony, braſs; to which I could add emery, zink, and other bodies, whereon I have tried it. All which together will make up no deſpicable number of differing taſtes.

EXPERIMENT XI.

Of two liquors, the one highly corroſive, and the other very pungent and not pleaſant, to compoſe a body of a pleaſant and aromatick taſte.

THIS experiment, which I elſewhere mention to other purpoſes, does in ſome regards better ſuit our preſent deſign, than moſt of the foregoing; ſince here the corroſive menſtruum is neither mortified by fixed nor urinous ſalts, ſuppoſed to be of a contrary nature to it; nor yet, as it were, tired out nor diſarmed by corroding of metals or other ſolid bodies. The experiment being ſomewhat dangerous to make at firſt in great, it may ſuffice for our preſent turn, to make it in the leſs quantity, as follows.

Take one ounce of ſtrong ſpirit of nitre, or of very good aqua-fortis itſelf, and put to it by little and little, (which caution, if you neglect, you may ſoon repent it,) another ounce of ſuch rectified ſpirit of wine, as, being kindled in a ſpoon, will flame all away: when theſe two liquors are well mixed, and grown cold again, you may, after ſome digeſtion, or, if haſte require, without it, diſtil them totally over together, to unite them exquiſitely into one liquor, in which, if the operation have been well performed, the corroſive particles of the ſalts will not only loſe all their cutting acidity, wherewith they wounded the palate; but by their new compoſition with the vinous ſpirits, the liquor acquires a vinous taſte, that is not only not acid or offenſive, but very pleaſing, as if it belonged to ſome new or unknown ſpice.

EXPERIMENT XII.

To imitate by art, and ſometimes even in minerals, the peculiar taſtes of natural bodies, and even vegetables.

THIS is not a fit place to declare, in what ſenſe I do or do not admit of ſouls in vegetables, nor what I allow or deny to the ſeminal or plaſtick principle aſcribed to plants: but perhaps it will not be erroneous to conceive, that, whatever be the agent in reference to thoſe taſtes, that are ſaid to be ſpecifick to this or that plant, that, on whoſe immediate account it is, or becomes of this or that nature, is a complication of mechanical affections, as ſhape, ſize, &c. in the particles of that matter, which is ſaid to be endowed with ſuch a ſpecifick taſte.

To illuſtrate this, I thought it expedient, to endeavour to imitate the taſte of ſome natural bodies by artificial compoſitions or preparations, but found it not eaſy, beforehand, to be aſſured of the ſucceſs of ſuch trials: and therefore I ſhall content my ſelf here to mention three or four inſtances, that, except the firſt, are rather obſervations than ſuch experiments as we are ſpeaking of.

I remember then, that, making ſome trials to alter the ſenſible qualities of ſmell, taſte, &c. of oil of vitriol, and ſpirit of wine, I obtained from them, among other things, that ſuited with my deſign, a certain liquor, which, though at firſt pleaſant, would, at a certain nick of time, make one, that had it in his mouth, think it had been imbued with garlick.

And this brings into my mind, that a ſkilful perſon, famous for making good cyder, coming one day to adviſe with me, what he ſhould do to heighten the taſte of it, and make it keep the longer, complained to me, that having, among other trials, put into a good veſſel full of juice of apples a certain proportion of muſtard-ſeed, with hopes it would make the cyder more ſpirituous and piquant, he found, to his wonder and loſs, that, when he came to draw it, it ſtunk of garlick ſo rank, that every body rejected it.

I remember alſo, that, by fermenting a certain proportion (for that we found requiſite) of *ſemen dauci* with beer or ale, the liquor had a very pleaſant reliſh of lemon-pills.

But that ſeems much more conſiderable, which I ſhall now add; that, with an inſipid metal and a very corroſive menſtruum, one may compound a taſte, that I have ſeveral times obſerved to be ſo like a vegetable, that I preſume it may deceive many. This may be done by diſſolving gold, without any groſs ſalt, in the mixture of aqua-fortis and the ſpirit of ſalt, or even in common aqua-regis, made by diſſolving ſal-armoniac in aqua-fortis. For if the experiment be happily made, one may obtain either a ſolution or a ſalt, whoſe auſtere taſte will very much reſemble that of ſloes, or of unripe bullace. And this taſte, with ſome little variety, I found in gold diſſolved without any diſtilled liquor at all; and alſo, if I much forget not, in gold, that by a peculiar menſtruum I had volatilized.

The laſt inſtance I ſhall give of the imitation of taſtes, I found to have been, for the main, known to ſome ingenious ladies. But to make the experiment ſucceed very well, a due proportion is the principal circumſtance, which is wont to be neglected. I cannot readily call to mind that, which I found to ſucceed beſt; but the trial may be indifferently well made after ſuch a manner as this:

Take a pint or a pound of *Malaga* or *Canary* ſack, (for though French, and the like wines, may ſerve the turn, yet they are not ſo proper;) and put into it a drachm or two of good odoriferous orrice roots, cut into thin ſlices, and let them infuſe in the liquor a convenient time, till you perceive, that they have given it a deſired taſte and ſmell; then keep the thus perfumed wine exactly ſtopped in a cool place: according to which way, I remember, that (when I hit on the right proportion of ingredients, and kept them a due time in infuſion) I had many years ago a wine, which, being coloured with cochineal, or ſome ſuch tinging ingredient, was taken for good raſberry-wine, not only by ordinary perſons, but, among others, by a couple of eminent phyſicians, one of whom pretended to an extraordinary criticalneſs of palate on ſuch occaſions; both of them wondering, how at ſuch an unlikely time of the year, as I choſe to preſent them that liquor among others, I could have ſuch excellent raſberry-wine: ſome of which (to add that by the by) I found to preſerve the ſpecifick taſte two or three years after it was made.

A Short EXCURSION about ſome CHANGES made of TASTES by MATURATION.

IT will not perhaps be thought impertinent but rather neceſſary, to add a word or two on this occaſion for their ſakes, that think the maturation of fruits, and the changes of taſtes, by which it is uſually known, muſt needs be the effect of the vegetable ſoul of the plant. For, after the fruit is gathered, and ſo, by being no longer a part of the tree, does, according to the moſt common opinion, ceaſe to be a part of the living plant, as a hand or a foot cut off is no more reckoned among the limbs of the man it belonged to; yet it is very poſſible, that ſome fruits may receive maturation, after they have been ſevered from the plants, that bore them. For, not to mention, that apples, gathered ſomewhat before the time, by lying in heaps, do uſually obtain a mellowneſs, which ſeems to be a kind or degree of maturation; or that medlars, gathered whilſt they are hard and harſh, do become afterwards in proceſs of time ſoft and better taſted; in which ſtate, though ſome ſay they are rotten, yet others think that ſuppoſed rottenneſs is the proper maturity of that kind of fruit: not to mention theſe, I ſay, or the like inſtances, it is a famous aſſertion of ſeveral writers of the Indian affairs, that the fruit they call bananas, is uſually gathered green, and hung up in bunches or cluſters in the houſe, whereby they ripen by degrees, and have an advantageous change made both of their colour and of their taſte. And this an ancient acquaintance of mine, a literate and obſerving perſon, of whom I enquired about it, aſſured me, he had himſelf lately tried and found to be true in *America*. And indeed I ſee not, why a convenient degree of warmth, whether external from the ſun and fire, or internal from ſome degree of fermentation or analogous inteſtine commotion, may not (whether the fruit be united to the plant or no) put the ſaporifick corpuſcles into motion, and make them, by various and inſenſible tranſcurſions, rub againſt each other, and thereby make the little bodies more ſlender or thin, and leſs rigid, or cutting and harſh, than they were before, and by various motions bring the fruit they compoſe to a ſtate, wherein it is more ſoft in point of conſiſtence, and abound in corpuſcles leſs harſh and more pliable, than they were before, and more congruous to the pores of the organ of taſte; and, in a word, make ſuch a change in the conſtitution of the fruit, as men are wont to expreſs by the name of Maturity. And that ſuch mechanical changes of texture may much alter the qualities, and among them the taſte of a fruit, is obvious in bruiſed cherries and apples, which in the bruiſed parts ſoon come to look and taſte otherwiſe than they did before. The poſſibility of this is alſo obvious by wardens, when ſlowly roaſted in embers, with ſo gentle a fire, as not to burn off the paper they are wont to be wrapped in, to be kept clean from the aſhes. And I have ſeen in the bordering country between *France* and *Savoy*, a ſort of pears, (whoſe name I now remember not,) which being kept for ſome hours in a moderate heat, in a veſſel exactly cloſed, with embers and aſhes above and beneath them, will be reduced to a juicy ſubſtance of a lovely red colour, and very ſweet and luſcious to the taſte. Many other ſorts of fruit in other countries, if they were handled after the ſame way, or otherwiſe ſkilfully wrought on by a moderate heat, would admit as great alterations in point of taſte. Neither is that ſort of pear to be here omitted, which by mere compreſſion, duly ordered, without external heat, will in a few minutes be brought to exchange it's former hardneſs and harſhneſs for ſo yielding a contexture and pleaſant a taſte, as I could not but think very remarkable. And that even more ſolid and ſtubborn ſalts, than thoſe of vegetables, may have the ſharpneſs and piercingneſs of their taſtes very much taken off by the bare internal action of one part upon the other, without the addition of any ſweetening body, I have been induced to think by having found, upon trial, that, by the help of inſipid water, we may, without any violence of fire, reduce ſea-ſalt into a brine of ſo mild and peculiar (I had almoſt ſaid) pleaſant a taſte, that one would ſcarce ſuſpect what it had been, or believe, that ſo great a change of a mineral body could be effected by ſo ſlight an inteſtine commotion, as indeed produced it; eſpecially, ſince the alteration of taſtes was not the moſt conſiderable, that was produced by this operation.

As to liquors, that come from vegetables, the emerging of new ſapors upon the inteſtine commotion of the ſaporifick parts, as conſequences of ſuch commotions, is more obvious than is commonly conſidered in the juice of grapes, which, from a ſweet and ſpiritleſs liquor, do by that internal motion, we call fermentation, acquire that pleaſing pungency and briſkneſs of taſte, that belongs to wine, and afterwards degenerates into that acid and cutting taſte, that is proper to vinegar; and all this, by a change of conſtitution made by the action of the parts themſelves on one another, without the help of any external additament.

EXPERIMENTS AND OBSERVATIONS, ABOUT THE Mechanical Production of ODOURS.

SINCE taſtes and odours (perhaps by reaſon of the nearneſs of the organs they affect) are wont, by phyſical writers, to be treated of next to one another, I alſo ſhall imitate them in handling thoſe two qualities, not only for the intimated reaſon, but becauſe, what I have premiſed in general, and ſome other things, that I have ſaid already under the title of taſtes, being applicable to odours alſo, it will not be neceſſary, and therefore it would be tedious, to repeat them here.

EXPERIMENT I.

With two bodies, neither of them odorous, to produce immediately a ſtrong urinous ſmell.

TAKE good quick lime and ſal armoniac, and rub or grind them well together, and holding your noſe to the mixture, you will be ſaluted with an urinous ſmell produced by the particles of the volatile ſalt, untied by this operation, which will alſo invade your eyes, and make them to water.

EXPERIMENT II.

By the bare addition of common water, to produce immediately a very ſtrong ſmell in a body, that had no ſuch ſmell before.

THIS is one of the phænomena of an experiment made with camphire and oil of vitriol, which I have elſewhere mentioned to another purpoſe. For, if in that corroſive menſtruum you diſſolve a good proportion, but not too much, of the ſtrongly ſcented gum, the odour of the camphire will be quite concealed in the mixture; but if you pour this mixture into a good quantity of fair water, the diſſolved gum will immediately recover out of the menſtruum, and ſmell as ſtrong as before, if not (by reaſon of the warmth produced in the operation) more ſtrongly.

EXPERIMENT III.

Of producing ſome odours, each of them quite differing from that of any of the ingredients.

HAVING taken two ounces, or parts of clear oil of turpentine, and mixed it with one ounce, or part, of oil of vitriol, (which muſt be done by degrees, for otherwiſe the veſſel will be endangered,) the clear liquor, that came over, upon the diſtillation of the mixture in a ſand-furnace, inſtead of the odour of turpentine, (for the oil of vitriol alone is wont to be inodorous,) ſmelt very ſtrong of ſulphur; inſomuch, that once, when I ſhewed this experiment, approaching my noſe very boldly and haſtily to the receiver newly ſevered from the retort, the ſulphureous ſtink proved ſo ſtrong, that it had almoſt (to ſpeak with the vulgar) taken away my breath. And to illuſtrate yet farther the poſſible emergency of ſuch odours upon the mixture of ingredients, as neither of them was apart endowed with, we cauſed the ſubſtance, that remained behind in the retort (in the form of a thin extract) after one of the newly mentioned diſtillations to be farther preſſed by a ſtronger fire, which forced moſt of it over, partly in the form of a thick oil, and partly in that of butter; both which we keep together in the ſame vial, becauſe their odour is neither that of oil of turpentine, nor that of brimſtone, but they ſmell exceedingly like the diſtilled oil of bees-wax.

EXPERIMENT IV.

About the production of ſome odours by local motion.

I Shall not now examine, whether the local motion of an external agent may not, without materially concurring to the operation, produce, by agitating and ſhuffling the parts, odorous corpuſcles: but that the celerity and other modifications of the local motion of the effluvia of bodies may not only ſerve to diverſify their odours, but ſo far produce them, as to make them perceptible by the ſenſe, which otherwiſe would not be ſo, may be gathered from ſome obſervations, which, being obvious, are not ſo proper for this place. Wherefore I ſhall rather take notice, that I know ſeveral bodies, that are not only inodorous when cold, but when conſiderably hot, and are fixed in the fire, and yet, by having their parts put into a peculiar kind of agitation, will preſently grow plainly odorous. On this occaſion I ſhall add, that, as there are ſome very hard woods, that acquire a ſtrong ſmell by the motion they may be expoſed to in a turner's lath, (as I have obſerved by trials particularly made with the hard and ponderous lignum vitæ,) ſo ſome afford, whilſt the operation laſts, an unexpected odour. And having enquired about this matter of two eminent artiſts, (whom I often employ) concerning the odour of beech-wood, whilſt it is turning, they both agreed, that it would emit well-ſcented effluviums. And one of them affirmed to me farther, that, having bought a great block of that wood, to make divers pieces of workmanſhip with it, when he came to turn it, there would iſſue out not only a copious odour, but of ſuch a peculiar fragrancy, that one, that knew not whence it proceeded, would have concluded he was ſmelling roſes.

EXPERIMENT V.

By mixing a good proportion of a very ſtrongly ſcented body with an almoſt inodorous one, to deprive it ſpeedily of all its ſmell.

TAKE ſalt of tartar, and drop upon it either ſpirit of nitre, or aqua fortis, not too much dephlegmed, till all the effervеſcence ceaſe, and the liquor will no longer work upon the alcali. Theſe, by a ſlow evaporation of the ſuperfluous moiſture, may be made to ſhoot into cryſtals, like thoſe of nitre, which, after you have (if need be) by rubbing them with a dried cloth, freed them from looſe adhering corpuſcles, will emulate ſalt-petre, as in other qualities, ſo in its not being odorous; though, if you diſtil them, or burn them on kindled coals, their fumes will quickly make you ſenſible, that they abounded with the ſtinking ſpirits, that make aqua fortis ſo offenſive to the ſmell.

EXPERIMENT VI.

By putting a very ſtrongly ſtinking body to another of a not ſweet ſmell, to produce a mixture of a pleaſant and ſtrongly aromatic odour.

WHAT is here propoſed is performed at the ſame time, that the eleventh of the foregoing experiments of taſtes is made. For the liquor thereby produced, if it be well prepared, has not only a ſpicy taſte, but alſo a kind of aromatic and pleaſant ſmell; and I have ſome now by me, that, though kept not over-carefully, does, after ſome years, retain much of its former odour, though not ſo much as of its taſte.

EXPERIMENT VII.

By digeſting two bodies, neither of them well ſcented, to produce bodies of a very ſubtile and ſtrongly fragrant odour.

WE took a pound (for inſtance) of Spaniſh wine, and put to it ſome ounces of oil of vitriol; then, keeping them for a reaſonable time in digeſtion, we obtained, as we expected, a mixture odoriferous enough. But this trial you will find improved by that, which enſues.

EXPERIMENT VIII.

By the bare addition of a body almoſt inodorous, and not well ſcented, to give a pleaſant and aromatic ſmell to ſpirit of wine.

THIS we have ſeveral times done, by the ways elſewhere related for another ſcope, the ſum of which, as far as it needs be mentioned in this place, is this:

WE took good oil of blue vitriol (that was brought from *Dantzic*) though the very common will ſerve well, and having put to it, by degrees, an equal weight of ſpirit of wine totally inflammable, we digeſted them together, for two, three, or four weeks, (ſometimes much longer, and then with better ſucceſs;) from which, when we came to diſtil the mixture, we had a very fragrant ſpirit, which was ſometimes ſo ſubtile, that, though diſtilled in a tall glaſs with a gentle heat, it would (in ſpite of our care to ſecure the cloſeneſs of the veſſels at the junctures) pierce through, and fill the laboratory with a perfume, which, though men could not gueſs what body afforded it, yet they could not but wonder at it. Whence we may learn, both how much thoſe ſpirituous and inflammable particles, the chemiſts call the vegetable ſulphur of wine, may work on and enoble a mineral ſulphur; (for, that ſuch an one there is in oil of vitriol, I have elſewhere proved by experience;) and how much the new commiſtions and contextures, made by digeſtion, may alter the odours of bodies, whether vegetable or mineral. That alſo another conſtitution of the ſame matter, without any manifeſt addition or receſs of particles, may proceed to exhibit a very differing ſmell, will appear by the following trial.

EXPERIMENT IX.

To make the forementioned fragrant body, without addition or fire, degenerate into the rank ſmell of garlick.

TO make out this, I need only relate, that I have more than once put the above mentioned fragrant liquor in ſtopped glaſſes, whereof the one, and not the other, ſtood in a warm place, till, in proceſs of time, I found that odoriferous liquor ſo to degenerate in point of ſcent, that one would have thought it to have been ſtrongly infected with garlick. And the like unpleaſant ſmell I obſerved in a certain oil made of vegetable and mineral ſubſtances diſtilled together.

AND on this occaſion I will add, (though not as an argument,) this obſervation, which though I ſhall not undertake it will always ſucceed, I think may not impertinently be ſet down in this place, partly becauſe of the likeneſs of the odour produced, to that, which was the effect of the laſt named trial; and partly (or rather cheifly) becauſe it may ſhew us, that a body, which itſelf is not only inodorous, but very fixed, may yet, in ſome caſes, have a great ſtroke in the phænomena of odours; whether by being wrought on by, and ſometimes mingled with the parts of the odorous body, and thereby giving it a new modificatian, I ſhall not now ſtay to enquire.

We took then good ſalt of tartar, and put to it, ſeveral times, its weight of the expreſſed juice of onions; we kept them in a light digeſtion for a day or two, and then unſtopping the vial, we found the former ſmell of the onions quite degenerated into a rank ſmell of garlick, as was judged, even when freſh juice of garlick was procured to compare them. To vary this experiment, we made, with fixed ſalts, and ſome other ſtrongly ſcented juices, trials, whoſe events it would perhaps be tedious here to relate.

EXPERIMENT X.

With an inodorous body, and another not well ſcented, to produce a muſky ſmell.

THIS we have ſometimes done by caſting into ſpirit (not oil) of vitriol a large proportion of ſmall pearls unbroken. For the action of the acid menſtruum upon theſe being moderated, partly by the weakneſs of the menſtruum, and partly by the entireneſs of the pearls, the diſſolution would ſometimes laſt many hours. Holding, from time to time, my noſe to the open orifice of the glaſs, it was eaſy to perceive a pleaſant muſky ſmell, which alſo others, to whom I mentioned it, took notice of, as well as I. And, if I miſremember not, I took notice of the like ſmell, upon pearls not only diſſolved in ſpirit of vinegar, but in another liquor, that had but a bad ſcent of its own. The foregoing experiment calls to my mind that, which follows.

EXPERIMENT XI.

With fixed metals, and bodies either inodorous or ſtinking, to produce ſtrong and pleaſant ſmells, like thoſe of ſome vegetables and minerals.

THAT gold is too fixed a body to emit any odour, and that aqua regis has an odour, that is very ſtrong and offenſive, I think will be eaſily granted. But yet aurum fulminans being made (as it is known) by precipitating, with the inodorous oil of tartar, the ſolution made of the former in the latter, and this precipitate being to be farther proceeded with in order to another experiment; we fulminated it *per ſe* in a ſilver veſſel like that, but better contrived, that is (if I miſremember not) ſomewhere deſcribed by *Glauberus*. And among other phænomena of this operation, that belong not to this place, we obſerved with pleaſure, that, when the fulmination was recently made, the ſteams, which were afforded by the metal, that had been fired, were endowed with a delightful ſmell, not unlike that of muſk. From which experiment, and the foregoing, we may learn, that art, by lucky contextures, may imitate the odours that are preſumed to be natural and ſpecifick; and that mineral and vegetable ſubſtances may compound a ſmell, that is thought to be peculiar to animals.

And as art ſometimes imitates nature in the production of odours, as may be confirmed by what is above related concerning counterfeit raſberry wine, wherein thoſe, that drank it, believed they did not only taſte, but ſmell the raſberry; ſo ſometimes nature ſeems to imitate herſelf, in giving like odours to bodies extremely differing. For, not yet to diſmiſs the ſmell of muſk, there is a certain ſeed, which, for the affinity of its odour to that perfume, they call the muſk-ſeed; and indeed, having ſome of it preſented me by a gentleman, that had newly brought it from the *Weſt-Indies*, I found it, whilſt it was freſh, to have a fragancy ſuitable to the name, that was given it. There is alſo a ſort of rats in *Muſcovy*, whoſe ſkins, whereof I have ſeen ſeveral, have a ſmell, that has procured them the name of muſk-rats. To which I know not whether we may not add the mention of a certain ſort of ducks, which ſome call muſk-ducks, becauſe at a certain ſeaſon of the year, if they be chaſed by violent motion, they will, under the wing, emit a muſky inſtead of a ſweaty ſcent; which, upon trial, I perceived to be true. On the other ſide, I have known a certain wood, growing in the *Indies*, which, eſpecially when the ſcent is excited by rubbing, ſtinks ſo rankly, and ſo like *Paracelſus*'s zibetum occidentale, (ſtercus humanum) that one would ſwear it were held under his noſe. And ſince I have been ſpeaking of good ſcents, produced by unlikely means, I ſhall not pretermit this obſervation, that, though generally the fire impreſſes a ſtrong offenſive ſmell, which chemiſts therefore call empyreumatical, upon the odorous bodies, that it works ſtrongly on; yet the conſtitution of a body may be ſuch, that the new contexture, that is made of its parts, even by the violence of the fire, ſhall be fit to afford effluviums, rather agreeable to the organs of ſmelling, than any way offenſive. For I remember, that, having, for a certain purpoſe, diſtilled ſaccharum Saturni in a retort with a ſtrong fire, I then obtained, (for I dare not undertake for the like ſucceſs to every experiment,) beſides a piercing and empyreumatical liquor, that was driven over into the receiver, a good lump of a caput mortuum, of a grayiſh colour, which, notwithſtanding the ſtrong impreſſion it had received from the fire, was ſo far from having any empyreumatical ſcent, that it had a pleaſing one; and when it was broken, ſmelled almoſt like a fine cake new baked, and broken, whilſt yet warm. And as the fire, notwithſtanding the empyreuma it is wont to give to almoſt all the bodies it burns, may be reduced to confer a good ſmell on ſome of them, if they be fitted upon ſuch a contexture of their parts, to emit ſteams of ſuch a nature, (whatever were the efficient cauſe of ſuch a contexture;) ſo we obſerve in the muſk animal, that nature in that cat, or rather deer, (though it properly belong to neither kind,) produces muſk by ſuch a change, as is wont in other animals to produce a putrefactive ſtink. So that, provided a due conſtitution of parts be introduced into a portion of matter, it may, on that account, be endowed with noble and deſirable ſcents, or other qualities, though that conſtitution were intro-

See in the paper of taſtes, exper. xii.

duced by ſuch unlikely means, as combuſtion and putrefaction themſelves. In confirmation of which, I ſhall ſubjoin, in the enſuing account, a notable, though caſual phænomenon, that occurred to a couple of virtuoſi of my acquaintance.

AN eminent profeſſor of mathematicks affirmed to me, that, chancing one day in the heat of ſummer, with another mathematician, (who, I remember, was preſent, when this was told,) to paſs by a large dunghil, that was then in *Lincoln's-inn-fields*, when they came to a certain diſtance from it, they were both of them ſurpriſed to meet with a very ſtrong ſmell of muſk, (occaſioned, probably, by a certain degree or a peculiar kind of putrefaction,) which each was for a while ſhy of taking notice of, for fear his companion ſhould have laughed at him for it; but, when they came much nearer the dunghil, that pleaſing ſmell was ſucceeded by a ſtink proper to ſuch a heap of excrements. This puts me in mind of adding, that, though the excrements of animals, and particularly their ſweat, are uſually foetid; yet, that it is not the nature of an excrement, but the conſtitutions, that uſually belong to them, make them ſo, hath ſeemed probable to me, upon ſome obſervations. For, not to mention what is related of *Alexander* the Great, I knew a gentleman of a very happy temperature of body, whoſe ſweat, upon a critical examination, wherein I made uſe alſo of a ſurprize, I found to be fragrant; which was confirmed alſo by ſome learned men of my acquaintance, and particularly a phyſician, that lay with him.

THOUGH civet uſually paſſes for a perfume, and as ſuch is wont to be bought at a great rate; yet it ſeems to be but a clammy excrement of the animal, that affords it, which is ſecreted into bags provided by nature to receive it. And I the rather mention civet, becauſe it uſually affords a phænomenon, that agrees very well with the mechanical doctrine concerning odours, though it do not demonſtrate it. For, when I have had the curioſity to viſit divers of thoſe civet-cats, (as they call them) though they have heads liker foxes than cats; I obſerved, that a certain degree of laxity (if I may ſo ſtile it) of the odorous atmoſphere was requiſite to make the ſmell fragrant. For, when I was near the cages, where many of them were kept together, or any great veſſel full of civet, the ſmell (probably by the plenty, and perhaps the over-briſk motion of the effluvia,) was rather rank and offenſive, than agreeable; whereas, when I removed into the next room, or to ſome other convenient diſtance, the ſteams (being leſs crowded, and farther from their fountain,) preſented themſelves to my noſtrils, under the notion of a perfume.

AND, not to diſmiſs this our eleventh experiment, without touching once more upon muſk, I ſhall add, that an ingenious lady, to whom I am nearly related, ſhewed me an odd monkey, that had been preſented her as a rarity by the then admiral of *England*, and told me, among other things, that ſhe had obſerved in it, that, being ſick, he would ſeek for ſpiders as his proper remedies, for ſome of which he then ſeemed to be looking, and thereby gave her occaſion to tell me this; which, when he had eaten, the alteration it made in him, would ſometimes fill the room with a muſky ſcent: but he had not the good luck to light on any, whilſt my viſit laſted.

EXPERIMENT XII.

To heighten good ſmells by compoſition.

IT is well known to perfumers, and is eaſy to be obſerved, that amber-greaſe alone, though eſteemed the beſt and richeſt perfume, that is yet known in the world, has but a very faint, and ſcarce a pleaſant ſcent. And I remember, that I have ſeen ſome hundreds of ounces together, newly brought from the *East-Indies*; but if I had not been before acquainted with the ſmell of ambergreaſe alone, and had had only the vulgar conceit of it, that it is the beſt and ſtrongeſt of perfumes, my noſtrils would ſcarce have made me ſuſpect thoſe lumps to have been any thing a-kin to ambergreaſe. But if a due proportion of muſk, or even civet, be dexterouſly mixed with amber, the latent fragrancy, though it be thereby ſomewhat compounded, will quickly be called forth, and exceedingly heightened. And indeed it is not, as it is commonly preſumed, the plenty of the richeſt ingredients, as amber-greaſe and muſk, but the juſt proportion and ſkilful mixture of them, that makes the nobleſt and moſt laſting perfume, of which I have had ſufficient experience; ſo that with a far leſs quantity of muſk and amber, than not only ordinary perſons, but perfumers themſelves are wont to employ, we have had ſeveral perfumes, that, for fragrancy, were much preferred to thoſe, where muſk and amber-greaſe are ſo plentifully employed. The proportions and ways of mixture we beſt approved of, would be too long, and are not neceſſary to be here ſet down; but you will not much err in making uſe of ſuch a proportion as this, viz. eight parts of ambergreaſe, two of muſk, and one of civet: which quantities of ingredients, if they be ſkilfully and exactly mingled, you will not miſs of a good compoſition, with which you may ennoble other materials, as benzoin, ſtorax, ſweet flowers, &c. fit to make paſtils, ointments for leather, pomander, &c. And we may here add, that, upon the ſcore of the new texture acquired by compoſition, ſome things, that are not fragrant themſelves, may yet much heighten the fragrancy of odoriferous bodies. And of liquid perfumes, I remember, it was the ſecret of ſome court ladies, noted for curioſity about perfumes, to mingle always a due proportion of wine-vinegar with the odoriferous ingredients. And, on this occaſion, to ſhew the power of mixtures in improving odours, I ſhall add ſomething about a liquor of mine, that has had the good fortune to be very favourably ſpoken of by perſons of quality accuſtomed to choice perfumes. This liquor, though thought an elaborate preparation, as well for another reaſon, as to recommend it to ſome, whoſe critical palates can taſte the very titles of things, I called it eſſence of muſk, is indeed a very plain ſimple preparation, which I thus make.

I take an arbitrary quantity of choice muſk, without finely powdering it, and pour upon it about a finger's breadth of pure ſpirit of wine; theſe, in a glaſs cloſely ſtopped, I ſet in a quiet place to digeſt, without the help of any furnace; and after ſome days, or a few weeks, (according as circumſtances determined,) the ſpirit, which is ſomewhat odd, will, in the cold, have made a ſolution of the fineſt parts of the muſk, and will be thereby much tinged, but not of a red colour. This liquor, being decanted, I keep by itſelf as the richeſt of all; and pour a like quantity of ſpirit on the remaining muſk, which uſually will, in the cold, though more ſlowly, draw a tincture, but fainter than the former; which being poured off, the remaining muſk may be employed for inferior uſes. Now that, which made me mention this preparation as pertinent to our preſent ſubject, is this phænomenon of it; that the firſt eſſence, or rather tincture, being ſmelled to by itſelf, has but a faint, and not very pleaſing odour of muſk, ſo that every body would not diſcover, that there was muſk in it; but if a ſingle drop, or two drops at moſt, were mixed with a pint, or perhaps a quart of good ſack, the whole body of the wine would preſently acquire a conſiderably muſky ſcent, and be ſo richly perfumed, as to taſte and ſmell, as ſeemed ſtrange enough to thoſe, that knew the vaſt diſproportion of the ingredients.

OF THE IMPERFECTION OF THE CHEMIST'S DOCTRINE OF QUALITIES.

CHAP. I.

SINCE a great part of thoſe learned men, eſpecially phyſicians, who have diſcerned the defects of the vulgar philoſophy, but are not yet come to underſtand and reliſh the corpuſcularian, have ſlid into the doctrine of the chemiſts; and ſince the ſpagyriſts are wont to pretend to make out all the qualities of bodies, from the predominancy of ſome one of their three hypoſtatical principles, I ſuppoſe it may both keep my opinion from appearing too preſumptuous, and (which is far more conſiderable) may make way for the fairer reception of the mechanical hypotheſis about qualities, if I here intimate (though but briefly and in general) ſome of thoſe defects, that I have obſerved in chemiſt's explications of qualities.

AND I might begin with taking notice of the obſcurity of thoſe principles, which is no ſmall defect in notions, whoſe proper office it ſhould be to conduce to the illuſtration of others. For, how can that facilitate the underſtanding of an obſcure quality, or phænomenon, which is itſelf ſcarcely intelligible, or, at leaſt, needs almoſt as much explanation, as the thing it is deſigned and pretended to explicate? Now a man need not be very converſant in the writings of chemiſts to obſerve, in how lax, indefinite, and almoſt arbitrary ſenſes they employ the terms of ſalt, ſulphur, and mercury; of which I could never find, that they were agreed upon any certain definitions, or ſettled notions; not only differing authors, but not unfrequently one and the ſame, and perhaps in the ſame brook, employing them in very differing ſenſes. But I will not give the chemiſts any riſe to pretend, that the chief fault, that I find with their hypotheſis, is but verbal; though that itſelf may not a little blemiſh any hypotheſis, one of the firſt of whoſe requiſites ought to be clearneſs; and therefore I ſhall now advance, and take notice of defects, that are manifeſtly of another kind.

AND, firſt, the doctrine, that all their theory is grounded on, ſeems to me inevident, and undemonſtrated, not to ſay precarious. It is ſomewhat ſtrange to me, that neither the ſpagyriſts themſelves, nor yet their adverſaries, ſhould have taken notice, that chemiſts have rather ſuppoſed than evinced, that the analyſis of bodies by fire, or even, that at leaſt ſome analyſis, is the only inſtrument of inveſtigating what ingredients mixed bodies are made up of, ſince, in divers caſes, that may be diſcovered by compoſition, as well as by reſolution; as it may appear, that vitriol conſiſts of metalline parts, (whether martial or venereal, or both,) aſſociated by coagulation with acid ones, one may, I ſay, diſcover this, as well by making true vitriol with ſpirit (improperly called oil) of ſulphur, or that of ſalt, as by diſtilling or reſolving vitriol by the fire.

BUT I will not here enlarge on this ſubject, nor yet will I trouble you with what I have largely diſcourſed in the Sceptical Chymiſt, to call in queſtion the grounds on which chemiſts aſſert, that all mixed bodies are compounded of ſalt, ſulphur, and mercury. For it may ſuffice me now to tell you, that, whatſoever they may be able to obtain from other bodies, it does not appear by experience, which is the grand, if not the only argument they rely on, that all mixed bodies, that have qualities, conſiſt of their *tria prima*, ſince they have not been able, that we know, truly, and without new compoſitions, to reſolve into thoſe three, either gold, or ſilver, or cryſtal, or Venetian talck, or ſome other bodies, that I elſewhere name; and yet theſe bodies are endowed with divers qualities, as the two former with fuſibleneſs and malleability, and all of them with weight and fixity; ſo that in theſe and the like bodies, whence chemiſts have not made it yet appear, that their ſalt, ſulphur, and mercury, can be truly and adequately ſeparated, it will ſcarce be other than precarious, to derive the malleableneſs, colour, and other qualities of ſuch bodies from thoſe principles.

UNDER this head I conſider alſo, that a great part of the chemical doctrine of qualities is bottomed on, or ſuppoſes, beſides their newly queſtioned analyſis by fire, ſome other things, which as far as I know, have not yet been well proved, and I queſtion whether they ever will be.

ONE of their main ſuppoſitions is, that this or that quality muſt have its πρῶτον δεκτικὸν, as *Sennertus*, the learnedeſt champion of this opinion, calls it, or ſome particular material principle, to the participation of which, as of the primary native and genuine ſubject, all other bodies muſt owe it: but upon this point having purpoſely diſcourſed elſewhere, I ſhall now only obſerve, that, not to mention local motion and figure, I think it will be hard to ſhew, what is the πρῶτον δεκτικὸν of gravity, volatility, heat, ſonorouſneſs, tranſparency, and opacity, which are qualities to be indifferently met with in bodies, whether ſimple or mixed.

AND whereas the ſpagyriſts are wont to argue, that becauſe this or that quality is not to be derived truly from this or that particular principle, as ſalt, for inſtance, and mercury; therefore it muſt needs be derivable from the third, as ſulphur. This way of arguing involves a further ſuppoſition than that newly examined. For it implies, that every quality in a compounded body muſt ariſe from ſome one of the *tria prima*, whereas experience aſſures us, that bodies may, by compoſition, obtain qualities, that were not to be found in any of the ſeparate ingredients. As we ſee in painting, that though blue and yellow be neither of them green, yet their mixture will be ſo. And though no ſingle ſound will make an octave or diapaſon; yet two ſounds, whoſe proportion is double, will have an eighth. And tin and copper melted and mingled together in a due proportion, will make a bell-metal far more ſonorous than either of them was before. It is obvious enough for chemiſts themſelves to obſerve, that, though lead be an inſipid body, and ſpirit of vinegar a very ſharp one, yet ſaccharum ſaturni, that is compounded out of theſe two, has a ſweetneſs, that makes it not ill deſerve its name.

BUT this ill-grounded ſuppoſition of the chemiſts, is extended farther in an uſual topic of theirs, according to which they conclude, that I know not how many qualities, as well manifeſt as occult, muſt be explicated by their *tria prima*, becauſe they are not explicable by the four elements of the Peripateticks. To make which argumentation valid, it muſt be proved, (which I fear it will never be) that there are no other ways, by which thoſe qualities may be explicated, but by a determinate number of material principles, whether four or three: beſides that, till they have ſhewn that ſuch qualities may be intelligibly explicated by their principles, the objection will lie as ſtrong for the Ariſtotelians againſt them, as for them againſt the Ariſtotelians.

CHAP. II.

NEXT I conſider, that there are divers qualities even in mixed bodies, wherein it does not appear, that the uſe of the chemical doctrine is neceſſary. As for inſtance, when pure gold is by heat only brought to fuſion, and conſequently to the ſtate of fluidity, and upon the remiſſion of that heat, grows a ſolid and conſiſtent body again, what addition or expulſion, or change of any of the *tria prima*, does appear to be the cauſe of this change of of conſiſtence? which is eaſy to be accounted for according to the mechanical way, by the vehement agitation, that the fire makes of the minute parts of the gold to bring it to fuſion; and the coheſion of thoſe parts, by virtue of their gravity and fitneſs to adhere to one another, when that agitation ceaſes. When Venice glaſs is merely, by being beaten to powder, deprived of its tranſparency and turned into a body opacous and white, what need or uſe of the *tria prima* have we in the explication of this phaenomenon? Or of that other, which occurs, when by barely melting down this white and opacous body it is deprived of its opacity and colour, and becomes diaphanous; and of this ſort of inſtances you will meet with divers in the following notes about particular qualities; for which reaſon I ſhall forbear the mention of them here.

CHAP. III.

I OBSERVE too, that the ſpagyrical doctrine of qualities is inſufficient, and too narrow to reach to all the phænomena, or even to all the notable ones, that ought to be explicable by them. And this inſufficiency I find to be twofold; for, firſt, there are divers qualities, of which chemiſts will not ſo much as attempt to give us explications, and of other particular qualities, the explications, ſuch as they are, that they give us, are often very deficient and unſatisfactory; and do not ſometimes ſo much as take notice of divers conſiderable phænomena, that belong to the qualities, whereof they pretend to give an account; of which you will meet with divers inſtances in the enſuing notes. And therefore I ſhall only (to declare my meaning the better) invite you to obſerve with me, that though gold be the body they affect to be moſt converſant with; yet it will be very hard to ſhew, how the ſpecific weight of gold can be deduced from any, or all, of the three principles, ſince mercury itſelf, that is, of bodies known to us, the heavieſt next to gold, is ſo much lighter than gold, that, whereas I have uſually found mercury to be to an equal weight of water, ſomewhat, though little, leſs than fourteen to one, I find pure gold to be about nineteen times as heavy as ſo much water. Which will make it very difficult, not to ſay impoſſible for them to explain, how gold ſhould barely, by participating of mercury, which is a body much lighter than itſelf, obtain that great ſpecific gravity we find it to have; for the two other hypoſtatical principles, we know, are far lighter than mercury. And I think it would much puzzle the chemiſts, to give us any examples of a compounded body, that is ſpecifically heavier than the heavieſt of the ingredients, that it is made up of. And this is the firſt kind of inſufficiency I was taking notice of in the chemical doctrine of qualities.

The other is, that there are ſeveral bodies, which the moſt learned among themſelves confeſs not to conſiſt of their *tria prima*, and yet are endowed with qualities, which conſequently are not in thoſe ſubjects to be explicated by the *tria prima*, which are granted not to be found in them. Thus elementary water, though never ſo pure, (as diſtilled rain water,) has fluidity and coldneſs and humidity and tranſparency and volatility, without having any of the *tria prima*. And the pureſt earth, as aſhes, carefully freed from the fixed ſalt, has gravity and conſiſtence and dryneſs and colour and fixity, without owing them either to ſalt, ſulphur, or mercury; not to mention, that there are celeſtial bodies, which do not appear, nor are wont to be pretended to conſiſt of the *tria prima*, that yet are endowed with qualities. As the ſun has light and, as many philoſophers think, heat and colour; and the moon has a determinate conſiſtence and figuration, (as appears by her mountains) and aſtronomers obſerve, that the higher planets, and even the fixed ſtars, appear to be differingly coloured. But I ſhall not multiply inſtances of this kind, becauſe what I have ſaid may not only ſerve for my preſent purpoſe, but bring a great confirmation to what I lately ſaid, when I noted, that the chemical principles were in many caſes not neceſſary to explicate qualities: for ſince in earth, water, &c. ſuch diffuſed qualities, as gravity, fixedneſs, colour, tranſparency and fluidity, muſt be acknowledged not to be derived from the *tria prima*; it is plain, that portions of matter may be endowed with ſuch qualities by other cauſes and agents than ſalt, ſulphur and mercury. And then why ſhould we deny, that alſo in compounded bodies thoſe qualities may be (ſometimes at leaſt) produced by the ſame or the like cauſes? as we ſee, that the reduction of a diaphanous ſolid to powder produces whiteneſs, whether the comminution happens to rock-cryſtal or to Venice glaſs, or to ice: the firſt of which is acknowledged to be a natural and perfectly mixed body; the ſecond a factitious, and not only mixed, but decompounded body; and the laſt, for aught appears, an elementary body, or at moſt very ſlightly and imperfectly mixed. And ſo by mingling air in ſmall portions with a diaphanous liquor, as we do, when we beat ſuch a liquor into foam, a whiteneſs is produced, as well in pure water, which is acknowledged to be a ſimple body, as in white wine, which is reckoned among perfectly mixed bodies.

CHAP. IV.

I FURTHER obſerve, that the chemiſt's explications do not reach deep and far enough. For firſt, moſt of them are not ſufficiently diſtinct and full, ſo as to come home to the particular phænomena, not oftentimes ſo much as to all the grand ones, that belong to the hiſtory of the qualities they pretend to explicate. You will readily believe, that a chemiſt will not eaſily make out by his ſalt, ſulphur and mercury, why a loadſtone, capped with ſteel, may be made to take up a great deal more iron, ſometimes more than eight or ten times as much, than if it be immediately applied to the iron: or why, if one end of the magnetic needle is diſpoſed to be attracted by the north-pole, for inſtance, of the load-ſtone, the other pole of the load-ſtone will not attract it, but drive it away: or why a bar or rod of iron, being heated red-hot, and cooled perpendicularly, will, with its lower end, drive away the flower de luce, or the north end of a mariner's needle, which the upper end of the ſame bar or rod will not repel, but draw to it. In ſhort, of above threeſcore properties, or notable phænomena of magnetic bodies, that ſome writers have reckoned up, I do not remember, that any three have been by chemiſts ſo much as attempted to be ſolved by their three principles. And even in thoſe qualities, in whoſe explications theſe principles may more probably, than elſewhere, pretend to have a place, the Spagyriſts accounts are wont to fall ſo ſhort of being diſtinct and particular enough, that they uſe to leave divers conſiderable phænomena untouched, and do but very lamely or ſlightly explicate the more obvious or familiar. And I have ſo good an opinion of divers of the embracers of the Spagyrical theory of qualities (among whom I have met with very learned and worthy men) that I think, that if a quality being propoſed to them, they were at the ſame time preſented with a good catalogue of the phænomena, that they may take, in the hiſtory of it, as it were with one view, they would plainly perceive, that there are more particulars to be accounted for, than at firſt they were aware of; and divers of them ſuch, as may quite diſcourage conſidering men from taking upon them to explain them all by the *tria prima*, and oblige them to have recourſe to more catholic and comprehenſive principles. I know not, whether I may not add on this occaſion, that, methinks, a chemiſt, who by the help of his *tria prima*, takes upon him to interpret that book of nature, of which the qualities of bodies make a great part, acts at but a little better rate than he, that ſeeing a great book written in a cypher, whereof he were acquainted but with three letters, ſhould undertake to decypher the whole piece. For though it is like he would, in many words, find one of the letters of his ſhort key, and in divers words two of them, and perhaps in ſome all three; yet, beſides that in moſt of the words, wherein the known letter or letters may be met with, they may be ſo blended with other unknown letters, as to keep him from decyphering a good part of thoſe very words, it is more than probable, that a great part of the book would conſiſt of words wherein none of his three letters were to be found.

CHAP. V.

AND this is the firſt account, on which I obſerve, that the chemical theory of qualities does not reach far enough: but there is another branch of its deficiency. For even, when the explications ſeem to come home to the phænomena, they are not primary, and, if I may ſo ſpeak, fontal enough. To make this appear, I ſhall at preſent employ but theſe two conſiderations. The firſt is, that thoſe ſubſtances themſelves, that chemiſts call their principles, are each of them endowed with ſeveral qualities. Thus ſalt is a conſiſtent, not a fluid body; it has its weight, it is diſſoluble in water, is either diaphanous or opacous, fixed or volatile, ſapid or inſipid; (I ſpeak thus diſjunctively, becauſe chemiſts are not all agreed about theſe things; and it concerns not my argument, which of the diſputable qualities be reſolved upon.) And ſulphur, according to them, is a body fuſible, inflammable, &c. and, according to experience, is conſiſtent, heavy, &c. So that it is by the help of more primary and general principles, that we muſt explicate ſome of thoſe qualities, which being found in bodies, ſuppoſed to be perfectly ſimilar or homogeneous, cannot be pretended to be derived in one of them from the other. And to ſay, that it is the nature of a principle to have this or that quality, as for inſtance, of ſulphur to be fuſible, and therefore we are not to exact a reaſon why it is ſo; though I could ſay much by way of anſwer, I ſhall now only obſerve, that this argument is grounded but upon a ſuppoſition, and will be of no force, if from the primary affections of bodies one may deduce any good mechanical explication of fuſibility in the general, without neceſſarily ſuppoſing ſuch a primogeneal ſulphur, as the chemiſts fancy, or deriving it from thence in other bodies. And indeed, ſince not only ſalt petre, ſea ſalt, vitriol and allom, but ſalt of tartar, and the volatile ſalt of urine, are all of them fuſible; I do not well ſee, how chemiſts can derive the fuſibleneſs even of ſalts obtained by their own analyſis (ſuch as ſalt of tartar and of urine) from the participation of the ſulphureous ingredient; eſpecially ſince, if ſuch an attempt ſhould be made, it would overthrow the hypotheſis of three ſimple bodies, whereof they will have all mixed ones to be compounded; and ſtill it would remain to be explicated, upon what account the principle, that is ſaid to endow the other with ſuch a quality, comes to be endowed therewith itſelf. For it is plain, that a maſs of ſulphur is not an atomical or adamantine body, but conſiſts of a multitude of corpuſcles of determinate figures, and connected after a determinate manner; ſo that it may be reaſonably demanded, why ſuch a convention of particles, rather than many another, that does not, conſtitutes a fuſible body.

CHAP. VI.

AND this leads me to a further conſideration, which makes me look upon the chemiſt's explications, as not deep and radical enough; and it is this, that, when they tell us, for inſtance, that the fuſibleneſs of bodies proceeds from ſulphur, in caſe they ſay true, they do but tell us what material ingredient it is, that being mingled with, and diſperſed through the other parts of a body, makes it apt to melt: but this does not intelligibly declare, what it is, that makes a proportion of matter fuſible, and how the ſulphureous ingredient introduces that diſpoſition into the reſt of the maſs, wherewith it is commixed or united. And yet it is ſuch explications as theſe, that an inquiſitive naturaliſt chiefly looks after, and which I therefore call philoſophical. And to ſhew, that there may be more fontal explications, I ſhall only obſerve, that, not to wander from our preſent inſtance, ſulphur itſelf is fuſible. And therefore, as I lately intimated, fuſibility, which is not the quality of one atome, or particle, but of an aggregate of particles, ought itſelf to be accounted for in that principle, before the fuſibleneſs of all other bodies be derived from it. And it will in the following notes appear, that in ſulphur itſelf, that quality may be probably deduced from the convention of corpuſcles of determinate ſhapes and ſizes, contexed, or connected, after a convenient manner. And if either nature, or art, or chance, ſhould bring together particles endowed with the like mechanical affections, and aſſociate them after the like manner, the reſulting body would be fuſible, though the component particles had never been parts of the chemiſt's primordial ſulphur: and ſuch particles ſo convening, might, perhaps, have made ſulphur itſelf, though before there had been no ſuch body in the world. And what I ſay to thoſe chemiſts, that make the ſulphureous ingredient the cauſe of fuſibility, may eaſily, *mutatis mutandis*, be applied to their hypotheſis, that rather aſcribe that quality to the mercurial, or the ſaline principle, and conſequently cannot give a rational account of the fuſibility of ſulphur. And therefore, though I readily allow, (as I ſhall have afterwards occaſion to declare,) that ſulphur, or an other of the *tria prima*, may be met with, and even abound in ſeveral bodies endowed with the quality, that is attributed to their participation of that principle; yet, that this may be no certain ſign, that the propoſed quality muſt flow from that ingredient, you may perhaps be aſſiſted to diſcern by this illuſtration, that if tin be duly mixed with copper or gold, or, as I have tried, with ſilver or iron, it will make them very brittle; and it is alſo an ingredient of divers other bodies, that are likewiſe brittle, as blue, green, white, and otherwiſe coloured amels, which are uſually made of calcined tin, (which the tradeſmen call putty) colliquated with the ingredients of cryſtal glaſs, and ſome ſmall portion of mineral pigment. But though, in all the above-named brittle bodies, tin be a conſiderable ingredient; yet it were very unadviſed to affirm, that brittleneſs, in general, proceeds from tin. For, provided the ſolid parts of conſiſtent bodies touch one another but according to ſmall portions of their ſurfaces, and be not implicated by their contexture, the metalline, or other compoſition, may be brittle, though there be no tin at all in it. And, in effect, the materials of glaſs, being brought to fuſion, will compoſe a brittle body, as well when there is no putty colliquated with them, as when there is. Calcined lead, by the action of the fire, may be melted into a brittle maſs, and even into tranſparent glaſs, without the help of tin, or any other additament. And I need not add, that there are a multitude of other bodies, that cannot be pretended to owe their brittleneſs to any participation of tin, of which they have no need, if the matter they conſiſt of, wants not the requiſite mechanical diſpoſitions.

And here I ſhall venture to add, that the way employed by the chemiſts, as well as the Peripateticks, of accounting for things by the ingredients, whether elements, principles, or other bodies, that they ſuppoſe them to conſiſt of, will often fruſtrate the naturaliſts expectation of events, which may frequently prove differing from what he promiſed himſelf, upon the conſideration of the qualities of each ingredient. For the enſuing notes contain divers inſtances, wherein there emerges a new quality differing from, or even contrary to any, that is conſpicuous in the ingredients; as two tranſparent bodies may make an opacous mixture; a yellow body and a blue, one, that is green; two malleable bodies, a brittle one; two actually cold bodies, a hot one; two fluid bodies, a conſiſtent one, &c. And as this way of judging, by material principles, hinders the foreknowledge of events from being certain; ſo it much more hinders the aſſignation of cauſes from being ſatisfactory; ſo that, perhaps, ſome would not think it very raſh to ſay, that thoſe, who judge of all mixed bodies, as apothecaries do of medicines, barely by the qualities and proportions of the ingredients (ſuch as, among the Ariſtotelians, are the four elements, and among the chemiſts the *tria prima*,) do, as if one ſhould pretend to give an account of the phænomena and operations of clocks and watches, and their diverſities by this, that ſome are made of braſs wheels, ſome of iron, ſome have plain ungilt wheels, others of wheels overlaid with gold, ſome furniſhed with gut-ſtrings, others with little chains, &c. and that therefore the qualities and predominancies of theſe metals, that make parts of the watch, ought to have aſcribed to them, what indeed flows from their co-ordination and contrivance.

CHAP. VII.

THE laſt defect I obſerve in the chemical doctrine of qualities, is, that in many caſes it agrees not well with the phænomena of nature, and that by one or both of theſe ways. Firſt, there are divers changes of qualities, wherein one may well expect, that a chemical principle ſhould have a great ſtroke, and yet it does not at all appear to have ſo. He, that conſiders, what great operations divers of the Hermeticks aſcribe to this or that hypoſtatical principle, and how many qualities, according to them, muſt from it be derived, can ſcarce do other than expect, that a great change, as to thoſe qualities, happening in a mixed body, ſhould, at leaſt, be accompanied with ſome notable action of, or alteration in the principle. And yet I have met with many inſtances, wherein qualities are produced, or aboliſhed, or very much altered, without any manifeſt introduction, expulſion, or conſiderable change of the principle, whereon that quality is ſaid to depend, or perhaps of either of the two others: as when a piece of fine ſilver, that having been nealed in the fire, and ſuffered to cool leiſurely, is very flexible, is made ſtiff, and hard to bend, barely by a few ſtrokes of a hammer. And a ſtring of a lute acquires or loſes a ſympathy, as they call it, with another ſtring of the ſame or another inſtrument, barely by being either ſtretched, ſo as to make an uniſon with it, or ſcrewed up, or let down, beyond or beneath that degree of tenſion.

To multiply inſtances of this kind, would be to anticipate thoſe, you will hereafter meet with in their due places. And therefore I ſhall paſs on, from the firſt ſort of phænomena, that favour not the chemical hypotheſis about qualities, to the other, which conſiſts of thoſe, wherein either, that does not happen, which, according to their hypotheſis, ought to happen, or the contrary happens to what, according to their hypotheſis, may juſtly be expected. Of this you will meet with inſtances hereafter; I ſhall now trouble you but with one, the better to declare my meaning. It is not unknown to thoſe chemiſts, that work much in ſilver and in copper, that the former will endure ignition, and become red-hot in the fire, before it will be brought to fuſion; and the latter is yet far more difficult to be melted down than the other: yet if you ſeparately diſſolve thoſe two metals in aqua fortis, and by evaporation reduce them to cryſtals, theſe will be brought to fuſion in a very little time, and with a very moderate heat, without breaking the glaſſes that contain them. If you aſk a vulgar chemiſt the cauſe of this facility of fuſion, he will, probably, tell you, without ſcruple, that it is from the ſaline parts of the aqua fortis, which, being imbodied in the metals, and of a very fuſible nature, impart that eaſineſs of fuſion to the metals they are mixed with. According to which plauſible explication one might well expect, that, if the ſaline corpuſcles were exquiſitely mingled with tin, they would make it far more fuſible than of itſelf it is. And yet, as I have elſewhere noted, when I put tin into a convenient quantity of aqua fortis, the metal being corroded, ſubſided, as is uſual, in the form of whites of eggs, which being well dried, the tin was ſo far from being grown more fuſible by the addition of the ſaline particles of the menſtruum, that, whereas it is known, that ſimple tin will melt long before it come to be red-hot, this prepared tin would endure, for a good while, not only a thorough ignition, but the blaſt of a double pair of bellows, (which we uſually employed to melt ſilver and copper itſelf,) without being at all brought to fuſion. And as for thoſe Spagyriſts, that admit, as moſt of them are granted to do, that all kinds of metals may be turned into gold, by a very ſmall proportion of what they call the philoſophers elixir, one may, I think, ſhew them, from their own conceſſions, that divers qualities may be changed, even in ſuch conſtant bodies as metals, without the addition of any conſiderable proportion of the ſimple ingredients, to which they are wont to aſcribe thoſe qualities; provided the agent, (as an efficient, rather than as a material cauſe,) be able to make a great change in the mechanical affections of the parts whereof the metal it acts on is made up. Thus if we ſuppoſe a pound of ſilver, a pound of lead, and a pound of iron to be tranſmuted into gold, each by a grain of the powder of projection, this tinging powder, as a material cauſe, is inconſiderable, by reaſon of the ſmallneſs of its bulk, and as an efficient cauſe, it works differing, and even contrary effects, according to the diſpoſition, wherein it finds the metal to be tranſmuted, and the changes it produces in the conſtituent texture of it. Thus it brings quickſilver to be fixed, which it was not before, and deprives it of the fluidity, which it had before; it brings ſilver to be indiſſolvable in aqua fortis, which readily diſſolved it before, and diſſoluble in aqua regis, which before would not touch it; and which is very conſiderable to our preſent purpoſe, whereas it makes iron much more fuſible than *Mars*, it makes lead much leſs fuſible than whilſt it retained its priſtine form, ſince *Saturn* melts ere it come to ignition, which gold requires to bring it to fuſion. But this is propoſed only as an argument *ad hominem*, till the truth of the tranſmutation of metals into gold, by way of projection, be ſufficiently proved, and the circumſtances, and phænomena of it, particularly declared.

I muſt not forget to take notice, that ſome learned modern chemiſts would be thought to explicate divers of the changes, that happen to bodies in point of odours, colours, &c. by ſaying, that, in ſuch alterations, the ſulphur, or other hypoſtatical principle, is intraverted or extraverted, or, as others ſpeak, inverted. But I confeſs, to me theſe ſeem to be rather new terms, than real explications. For, to omit divers of the arguments mentioned in this preſent treatiſe, that may be applied to this way of ſolving the phænomena of qualities, one may juſtly object, that the ſuppoſed extraverſion or intraverſion of ſulphur, can by no means reach to give an account of ſo great a variety of odours, colours, and other qualities, as may be found in the changed portions of matter we are ſpeaking of. And, which is more, what they call by theſe and the like names, cannot be done without local motion tranſpoſing the particles of the matter, and conſequently producing in it a change of texture, which is the very thing we would infer, and which being ſuppoſed, we may grant ſulphur to be oftentimes actually preſent in the altered bodies, without allowing it to be always neceſſary to produce the alterations in them, ſince corpuſcles, ſo conditioned and contexed, would perform ſuch effects, whether ſulphur, as ſuch, did, or did not make up the ſubject matter of the change.

AND now I ſhall conclude, and partly recapitulate what has been delivered in this and the two foregoing chapters, with this ſummary conſideration; that the chemiſt's ſalt, ſulphur, and mercury themſelves are not the firſt and moſt ſimple principles of bodies, but rather primary concretions of corpuſcles, or particles more ſimple than they, as being endowed only with the firſt, or moſt radical, (if I may ſo ſpeak) and moſt catholick affections of ſimple bodies, namely, bulk, ſhape, and motion, or reſt; by the different conventions or coalitions of which, minuteſt portions of matter are made thoſe differing concretions, that chemiſts name ſalt, ſulphur, and mercury. And to this doctrine it will be conſonant, that ſeveral effects of this or that ſpagyrical principle need not be derived from ſalt, for inſtance, or ſulphur as ſuch, but may be explained by the help of ſome of thoſe corpuſcles, that I have lately called more ſimple and radical; and ſuch explications being more ſimple and mechanical, may be thought, upon that ſcore, more fundamental and ſatisfactory.

CHAP. VIII.

I KNOW it may be objected, in favour of the chemiſts, that as their hypoſtatical principles, ſalt, ſulphur, and mercury, are but three, ſo the corpuſcularian principles are but very few; and the chief of them bulk, ſize, and motion, are but three neither; ſo that it appears not, why the chemical principles ſhould be more barren than the mechanical. To which allegation I anſwer, that, beſides that theſe laſt named principles are more numerous, as taking in the poſture, order, and ſituation, the reſt, and, above all, the almoſt infinitely diverſifiable contextures of the ſmall parts, and the thence reſulting ſtructures of particular bodies, and fabrick of the world: beſides this, I ſay, each of the three mechanical principles, ſpecified in the objection, though but one in name, is equivalent to many in effect; as figure, for inſtance, comprehends not only triangles, ſquares, rhomboides, trapeziums, and a multitude of polygons, whether ordinate or irregular; but, beſides cubes, priſms, cones, ſpheres, cylinders, pyramids, and other ſolids of known denominations, a ſcarce numerable multitude of hooked, branched, eel-like, ſcrew-like, and other irregular bodies; whereof, though theſe, and ſome others, have diſtinct appellations, yet the greateſt part are nameleſs; ſo that it need be no wonder, that I ſhould make the mechanical principles ſo much more fertile, that is, applicable to the production and explication of a far greater number of phænomena, than the chemical; which, whilſt they are conſidered but as ſimilar bodies, that are ingredients of mixed and compounded ones, are chiefly variable but by the greater or leſſer quantity, that is employed by nature or art to make up the mixed body. And painters obſerve, that black and white, though mixed in differing proportions, will ſtill make but lighter and darker greys. And if it be ſaid, that theſe ingredients, by the texture reſulting from their mixtures, may acquire qualities, that neither of them had before; I ſhall anſwer, that to alledge this, is, in effect, to confeſs, that they muſt take in the mechanical principles (for to them belongs the texture or ſtructure of bodies) to aſſiſt the chemical ones. And, on this occaſion, to borrow an illuſtration from our unpubliſhed dialogue of the requiſites of a good hypotheſis, I ſhall add, that a chemiſt, that ſhould pretend, that becauſe his three principles are as many as thoſe of the Corpuſcularians, they are as ſufficient, as theſe, to give an account of the book of nature, methinks, I ſay, he would do like a man, that ſhould pretend, that, with four and twenty words, he would make up a language, as well as others can with the four and twenty letters of the alphabet, becauſe he had as many words already formed, as they had of bare letters; not conſidering, that, inſtead of the ſmall number of variations, that can be made of his words by prepoſitions and terminations, the letters of the alphabet being variouſly combined, placed, and reiterated, can be eaſily made to compoſe not only his four and twenty words, with their variations, but as many others, as a whole language contains.

CHAP. IX.

NOTWITHSTANDING all, that I have been obliged to ſay to the diſadvantage of the chemical principles, in reference to the explication of qualities, I would not be thought to grant, that the Peripateticks have reaſon to triumph, as if their four elements afforded a better theory of qualities. For, if I had, together with leiſure enough to perform ſuch a taſk, any obligation to undertake it, I preſume, it would not be difficult to ſhew, that the Ariſtotelian doctrine, about particular qualities, is liable to ſome of the ſame objections with the chemical, and to ſome others no leſs conſiderable; and that, to derive all the phænomena their doctrine ought to ſolve, from ſubſtantial forms and real qualities elementary, is to impoſe on us a theory more barren and precarious, than that of the Spagyriſts.

THAT, to derive the particular qualities of bodies from thoſe ſubſtantial forms, whence the ſchools would have them to flow, is but an inſufficient and unfit way of accounting for them, may appear by this, that ſubſtantial forms themſelves are things, whoſe exiſtence many learned philoſophers deny, whoſe theory many of them think incomprehenſible, and the moſt candid and judicious of the Peripateticks themſelves confeſs it to be very abſtruſe; ſo that, from ſuch doubtful and obſcure principles, we can hardly expect clear explications of the nature and phænomena of qualities; not to urge, that the Ariſtotelian definitions, both of qualities in general, and of divers of the more familiar qualities in particular, as heat, cold, moiſture, diaphaneity, &c. are far enough from being clear and well framed, as we elſewhere have occaſion to ſhew.

ANOTHER thing, which makes the ſcholaſtic doctrine of qualities unſatisfactory, is, that it ſeldom ſo much as attempts to teach the manner, how the qualities themſelves, and their effects or operations, are produced. Of this you may elſewhere find an inſtance given in the quality, that is wont to be firſt in the liſt, viz. that of heat; which, though it may intelligibly and probably be explicated by the corpuſcular hypotheſis, yet, in the Peripatetic account, that is given of it, is both too queſtionable and too ſuperficial to give much content to a rational inquirer. And indeed to ſay, that a ſubſtantial form (as that of the fire) acts by a quality, (called heat) whoſe nature it is to produce ſuch an effect, (as to ſoften wax, or harden clay,) ſeems to be no other in ſubſtance, than to ſay, that it produces ſuch an effect by ſome power it has to produce it. But what that power is, and how it operates, is that, which, though we moſt deſire to know, we are left to ſeek. But to proſecute the imperfections of the Peripatetic hypotheſis, were to intrench upon another diſcourſe, where they are more fully laid open. And therefore I ſhall now but lightly glance upon a couple of imperfections, that more particularly relate to the doctrine of qualities.

And firſt, I do not think it a convincing argument, that is wont to be employed by the Ariſtotelians for their elements, as well as by the chemiſts for their principles, that, becauſe this or that quality, which they aſcribe to an element or a principle, is found in this or that body, which they call mixed, therefore it muſt owe that quality to the participation of that principle or element. For, the ſame texture of parts, or other modification of matter, may produce the like quality in the more ſimple and the more compounded body, and they may both ſeparately derive it from the ſame cauſe, and not one from the participation of the other. So water, and earth, and metals, and ſtones, &c. are heavy, upon the account of the common cauſe of gravity, and not becauſe the reſt partake of the earth; as may appear in elementary water, which is as ſimple a body as it, and yet is heavy: ſo water and oil, and exactly dephlegmed ſpirit of wine, and mercury, and alſo metals and glaſs of antimony, and minium or calcined lead, whilſt theſe three are in fuſion, are fluid, being made ſo by the variouſly determined motions of their minute parts, and other cauſes of fluidity, and not by the participation of water, ſince the arid calces of lead and antimony are not like to have retained, in the fire, ſo volatile a liquor as water, and ſince fluidity is a quality, that mercury enjoys in a more durable manner than water itſelf: for that metalline liquor, as alſo ſpirit of wine well rectified, will not be brought to freeze with the higheſt degree of cold of our ſharpeſt winters, though a far leſs degree of cold would make water ceaſe to be fluid, and turn it into ice.

To this I ſhall only add, in the ſecond place, that it is not unpleaſant to ſee, how arbitrarily the Peripateticks derive the qualities of bodies from their four elements, as if, to give an inſtance in the lately named quality, liquidity, you ſhew them exactly dephlegmed ſpirit of wine, and aſk them, whence it has its great fluidneſs, they will tell you, from water, which yet is far leſs fluid than it; and this ſpirit of wine itſelf is much leſs ſo than the flame, into which the ſpirit of wine is eaſily reſoluble. But if you aſk, whence it becomes totally inflammable, they muſt tell you, from the fire; and yet the whole body, at leaſt, as far as ſenſe can diſcover, is fluid, and the whole body becomes flame, (and then is moſt fluid of all;) ſo that fire and water, as contrary as they make them, muſt both be, by vaſt odds, predominant in the ſame body. This ſpirit of wine alſo, being a liquor, whoſe leaſt parts, that are ſenſible, are actually heavy, and compoſe a liquor, which is ſeven or eight hundred times as heavy as air of the ſame bulk, which yet experience ſhews not to be devoid of weight, muſt be ſuppoſed to abound with earthy particles; and yet this ſpirituous liquor may, in a trice, become flame, which they would have to be the lighteſt body in the world.

But, to enlarge on this ſubject, would be to forget, that the deſign of this tract engages me to deal not with the Peripatetic ſchool, but the Spagyrical. To which I ſhall therefore return, and give you this advertiſement about it, that what I have hitherto objected, is meant againſt the more common and received doctrine about the material principles of bodies reputed mixed, as it is wont, by vulgar chemiſts, to be applied to the rendering an account of the qualities of ſubſtances corporeal; and therefore I pretend not, that the paſt objections ſhould conclude againſt other chemical theories, than that, which I was concerned to queſtion. And if adept philoſophers, (ſuppoſing there be ſuch) or any other more than ordinarily intelligent Spagyriſts, ſhall propoſe any particular hypotheſis, differing from thoſe, that I have queſtioned, as their doctrine and reaſons are not yet known to me; ſo I pretend not, that the paſt arguments ſhould conclude againſt them, and am willing to think, that perſons advantaged with ſuch peculiar opportunities, to dive into the myſteries of nature, will be able to give us, if they ſhall pleaſe, a far better account of the qualities of bodies, than what is wont to be propoſed by the generality of chemiſts.

Thus, dear *Pyrophilus*, I have laid before you ſome of the chief imperfections I have obſerved, in the vulgar chemiſts doctrine of qualities; and conſequently I have given you ſome of the chief reaſons, that hinder me from acquieſcing in it. And as my objections are not taken from the ſcholaſtical ſubtleties, nor the doubtful ſpeculations of the Peripateticks, or other adverſaries of the Hermetick philoſophy, but from the nature of things, and from chemical experiments themſelves; ſo, I hope, if any of your ſpagyrical friends have a mind to convince me, he will attempt to do it by the moſt proper way, which is, by actually giving us clear and particular explications, at leaſt, of the grand phaenomena of qualities; which, if he ſhall do, he will find me very ready to acquieſce in a truth, that comes uſhered in, and endeared by ſo acceptable and uſeful a thing, as a philoſophical theory of qualities.

REFLEC-

REFLECTIONS
UPON THE
HYPOTHESIS
OF
ALCALI and ACIDUM.

CHAP. I.

I PRESUME, it will not be difficult to diſcern, that much of what has been ſaid about the imperfection of the vulgar chemical doctrine concerning qualities, may, with eaſy variations, be applied to ſome other hypotheſes, that are of kin to that doctrine, and particularly to their theory, that would derive both the qualities of bodies, and the reſt of the phænomena of nature from what they call acidum and alcali. For though theſe two differences may be met with in a great number and variety of bodies, and conſequently the conſideration of them may frequently enough be of good uſe, (eſpecially to Spagyriſts, and phyſicians, when they are converſant about the ſecondary and, if I may ſo call them, chemical cauſes and operations of divers mixed bodies;) yet I confeſs I cannot acquieſce in this hypotheſis of alcali and acidum, in the latitude, wherein I find it urged and applied by the admirers of it, as if it could be uſefully ſubſtituted in the place of matter and motion.

THE hypotheſis being in a ſort ſubordinate to that of the *tria prima*, in aſcribing to two contrary ſaline principles, what vulgar chemiſts do to their ſalt, ſulphur, and mercury; moſt of the objections we have made againſt the vulgar chemical doctrine, may, as I lately intimated, be applied, by a little variation, to this, and therefore I ſhall need but to touch upon the main things, that keep me from acquieſcing in this hypotheſis.

CHAP. II.

AND firſt, it ſeems precarious to affirm, that in all bodies, or even in all the ſenſible parts of mixeds, acid and alcalizate parts are found; there not having been, that I know, any experimental induction made of particulars any thing near numerous enough to make out ſo great an aſſertion, and in divers bodies, wherein experience is vouched for the inexiſtence of theſe principles, that inexiſtence is indeed proved, not by direct and clear experience, but upon a ſuppoſition, that ſuch and ſuch effects flow from the operations of the aſſumed principles.

SOME Spagyriſts, when they ſee aqua-fortis diſſolve filings of copper, conclude from thence, that the acid ſpirits of the menſtruum meet in the metal with an alcali upon which they work; which is but an unſafe way of arguing, ſince good ſpirit of urine, which they take to be a volatile alcali, and which will make a great conflict with aqua-fortis, will, as I have elſewhere noted, diſſolve filings of copper both readily enough and more genuinely than the acid liquor is wont to do. So, when they ſee the magiſtery of pearl or coral made by dropping oil of tartar into the ſolutions of thoſe bodies made with ſpirit of vinegar, they aſcribe the precipitation to the fixed alcali of the tartar, that mortifies the acidity of the ſpirit of vinegar; whereas the precipitation would no leſs enſue, if, inſtead of alcalizate oil of tartar, we employ that highly acid liquor which they call *oleum ſulphuris per campanam*.

I think alſo it may be doubted, whether thoſe, I reaſon with, are ſo certain as they ſuppoſe, that at leaſt, when they can manifeſtly diſcover an acid, for inſtance, in a body, the operation of that body upon another, which they judge to abound with an alcali, muſt be the effect of a conflict between thoſe two jarring principles, or, if I may ſo call them, duelliſts. For an acid body may do many things, not ſimply as an acid, but on the ſcore of a texture or modification, which endows it with other qualities as well as acidity, whoſe being aſſociated with thoſe other qualities in ſome caſes, may be but accidental to the effect to be produced; ſince by one or more of theſe other qualities the body may act in caſes, where prejudice may make a chemiſt conſider nothing but acidity. Thus when ſome chemiſts ſee an acid menſtruum, as aqua-fortis, ſpirit of ſalt, oil of vitriol, &c. diſſolve iron, they preſently aſcribe the effect to an acidity of the liquors, whereas well dephlegmed urinous ſpirits, which they hold to have a great antipathy to acids, will, as I have tried in ſome of them, readily enough diſſolve crude iron even in the cold. And on the other ſide, mercury will not work on the filings of iron, though this be ſo open a metal, that even weak liquors will do it; and yet if one ſhould urge, that quick-ſilver readily diſſolves gold in amalgamation, he may expect to be told, according to their doctrine, that mercury has in it an occult acid, by which it performs the ſolution; whereas it ſeems much more probable, that mercury has corpuſcles of ſuch a ſhape and ſize, as fit them to inſinuate themſelves into the commenſurate pores they meet with in gold, but make them unfit to enter readily the pores of iron, to which nature has not made them congruous; as on the other ſide the ſaline corpuſcles of aqua-fortis will eaſily find admiſſion into the pores of iron, but not into thoſe of gold, to which they do not correſpond as they do to the others. And when a knife, whoſe blade is touched with a load-ſtone, cuts bread and takes up filings of iron, it does neither of them upon the ſcore of alcali and acidum, but the one upon the viſible ſhape, and the ſtiffneſs of the blade, and the other upon the latent contrivance or change of texture produced by the operation of the load-ſtone in the particles, that compoſe the ſteel.

This may perhaps be farther illuſtrated by adding, that when blue vitriol, being beaten and finely ſearced, makes a white powder, that whiteneſs is a quality, which the powder has not, as being of a vitriolate nature. For rock-cryſtal or Venice-glaſs, being finely beaten, will have the ſame operation on the eye, but it proceeds from the tranſparency of the body and the minuteneſs, multitude, and confuſed ſituation of the corpuſcles, that make up the powder. And therefore, if other bodies be brought by comminution into parts endowed with ſuch mechanical affections, as we have named; theſe aggregates will act upon the organs of ſight, as white bodies.

CHAP. III.

And this leads me to another exception againſt the hypotheſis of the duelliſts, which is, that the framers of it ſeem arbitrarily to have aſſigned provinces or offices to each of their two principles, as the chemiſts do to each of their *tria prima*, and the Peripateticks to each of their four elements. For it is not enough to ſay, that an acid, for inſtance, as ſuch, performs theſe things, and an alcali ſo many others, that they divide the operations and phænomena of nature, or at leaſt (as ſome, more cautious, are content to ſay) of mixed bodies between them; ſince aſſertions of ſuch great moment ought not to be advanced or received without ſufficient proof. And perhaps the very diſtribution of ſalts into acids and alcalies hath ſomewhat of arbitrary in it, ſince others may, without aſſuming much more, take the freedom to diſtribute them otherwiſe, there being not only ſeveral things, wherein acids and alcalies agree, but alſo ſeveral things, wherein ſalts of the ſame denomination widely differ. As for inſtance, ſome alcalies, according to thoſe I reaſon with, are, like ſalt of tartar, fixed, and will endure the violence of the fire; others, like ſalt of urine or harts-horn, are exceedingly fugitive, and will be driven up with a ſcarce ſenſible degree of heat; ſome, as ſalt of tartar, will precipitate the ſolution of ſublimate into an orange-tawny; others, as ſpirit of blood and harts-horn, precipitate ſuch a ſolution into a milky ſubſtance. Oil of tartar will very ſlowly operate upon filings of copper, which ſpirit of urine and harts-horn will readily diſſolve in the fire.

And among acids themſelves the difference is no leſs, if not much greater. Some of them will diſſolve bodies, that others will not, as aqua-fortis will diſſolve ſilver and mercury, but leave gold untouched; or as aqua-regis, though made without ſal-armoniac, that diſſolves gold readily, will diſſolve mercury but ſcurvily, and ſilver not at all. And this may happen, when the menſtruum, that will not diſſolve the body, is reputed much ſtronger than that, which does; as dephlegmed ſpirit of vinegar will diſſolve lead, reduced to minute parts in the cold; which is an effect, that chemiſts are not wont to expect from ſpirit of ſalt. Nay, which is more, one acid will precipitate what another has diſſolved, and contrarily; as ſpirit of ſalt will precipitate ſilver out of ſpirit of nitre. And I found oil of vitriol to precipitate bodies of divers kinds, minerals and others, out of ſome acid menſtruums, particularly ſpirit of vinegar.

To this might be added the properties, peculiar to ſome particular acids, as that ſpirit of nitre or aqua-fortis will diſſolve camphire into an oil, and coagulate common oil into a conſiſtent and brittle ſubſtance like tallow; and, though it will both corrode ſilver, copper, lead, and mercury, and keep them diſſolved, it will quickly let fall almoſt the whole body of tin, very ſoon after it has corroded as much as it can of it. By all which, and ſome other like inſtances, I am induced to queſtion whether the acidum and alcali, we are ſpeaking of, have the ſimplicity, that philoſophy requires in principles; and ſhall be kept from wondering, if others ſhall think it as free for them to conſtitute other principles, as it is for the learned men I reaſon with, to pitch upon acidum and alkali.

And ſome perhaps will be bold to ſay, that, ſince the former of thoſe principles comprehend ſuch a number of bodies, that are, many of them, very differing, and ſome of them directly contrary in their operations, it ſeems a ſlight and not philoſophical account of their nature, to define an acid by its hoſtility to an alcali, which, they will ſay, is almoſt as if one ſhould define a man, by ſaying, that he is an animal, that is at enmity with the ſerpent; or a lion, that he is a fourfooted beaſt, that flies from a crowing cock.

CHAP. IV.

But although one of the chiefeſt conditions, that philoſophers may juſtly require in principles, is, that, being to explain other things, they ſhould be very clear themſelves; yet I do not much wonder, that the definitions given us of acidum and alcali ſhould be but unaccurate and ſuperficial, ſince I find not, that they have themſelves any clear and determinate notion or ſure marks, whereby to know them diſtinctly, without which chemiſts will ſcarce be able to form clear and ſettled notions of them. For to infer, as is uſual, that, becauſe a body diſſolves another, which is diſſoluble by this or that known acid, the ſolvent muſt alſo be acid; or to conclude, that, if a body precipitates a diſſolved metal out of a confeſſedly acid menſtruum, the precipitant muſt be an alcali; to argue thus, I ſay, it is unſecure; ſince, not to repeat what I ſaid lately of copper, I found, that filings of ſpelter will be diſſolved as well by ſome alcalies, (as ſpirit of ſal armoniac) as by acids. And bodies may be precipitated out of acid menſtruums, both by other acids, and by liquors, where there appears not the leaſt alcali: as I have found, that a ſolution of tin-glaſs, made in aqua-fortis, would be precipitated both by ſpirit of ſalt, and by common, or rain water. And as for the other grand way, that chemiſts employ, to diſtinguiſh acids and alcalies, namely by the heat, commotion, and bubbles, that are excited upon their being put together, that may be no ſuch certain ſign, as they preſume, they having indeed a dependence upon particular contextures, and other mechanical affections, that chemiſts are not wont to take any notice of. For almoſt any thing, that is fitted variouſly and vehemently to agitate the minute parts of a body, will produce heat in it; and ſo, though water be neither an acid, nor an alcalizate liquor, yet it would quickly grow very hot, not only with the highly acid oil of vitriol, but (as I have more than once purpoſely tried and found) with the fiery alcalizate ſalt of tartar. And it is to be noted, that neither in the one, nor the other of theſe incaleſcent mixtures, there is produced any ſuch viſible or audible conflict, as, according to the doctrine of the chemiſts I reaſon with, one would expect. And as for the production of bubbles, eſpecially if accompanied with a hiſſing noiſe, neither is that ſuch a certain ſign as chemiſts imagine: for the production of bubbles is not a neceſſary effect, or concomitant of heat excited by conflicts, but depends very much upon the peculiar diſpoſition of bodies put together to extricate, produce, or intercept particles of air, (or ſteams, for the time equivalent to them;) and therefore as oil of vitriol, mixed in a due proportion with fair water, may be brought to make the water too hot to be held in one's hand, without exciting bubbles; ſo I have found, by trials purpoſely made, that alcalizate ſpirit of urine drawn from ſome kinds of quick lime, being mixed with oil of vitriol moderately ſtrong, would produce an intenſe heat, whilſt it produced either no manifeſt bubbles at all, or ſcarce any, though the urinous ſpirit was ſtrong, and in other trials operated like an alcali; and although alſo with ſpirit of urine, made *per ſe* the common way, the oil of vitriol will produce a great hiſſing, and a multitude of conſpicuous bubbles. On the other ſide, I have ſometimes, though not ſo conſtantly, found, that ſome acid ſpirits, eſpecially that of verdigreaſe made *per ſe*, would, when poured upon ſalt of tartar, make a conflict with it, and produce a copious froth, though we obſerved it not to be accompanied with any manifeſt heat. And I elſewhere mention two bodies, upon whoſe putting together, numerous bubbles would, for a long time, and not without noiſe, be generated, and ſucceed one another, though I could perceive no heat all to accompany this tumult.

As for the taſte, which by many is made a great touchſtone, whereby to know acids and alcalies, I conſider, that there is a multitude of mixed bodies, wherein we can ſo little diſcern by the taſte, which of the principles is predominant, that this ſenſe would not oblige one to ſuſpect, much leſs to conclude, there were one grain of either of them to be found there; ſuch bodies are diamonds and rubies, and moſt gems, beſides many ignobler ſtones, and gold, and ſilver, and mercury, and I know not how many other bodies. On the other ſide, there are bodies, that abound with acid or alcalizate ſalts, which either have no taſte, or a quite differing one from that of the chemical principle. As though Venice-glaſs be in great part compoſed of a fixed alcali; yet to the tongue it is inſipid, and cryſtals of lune, and of lead, made with aqua fortis, and containing great ſtore of the acid particles of the menſtruum, have nothing of acidity in the mouth, the latter having a ſaccharine ſweetneſs, and the former an extreme bitterneſs. And even in vegetable ſubſtances, that have a manifeſt taſte, it is not ſo eaſy to know by that, whether it be the acid, or the alcalizate principle, that is predominant in them; as in the eſſential oils of ſpices, and other vegetables. And in the groſs empyreumatical oils of woods, and even in high rectified ſpirit of wine, which therefore ſome will have to be an alcalizate liquor, and others liſt it among acids, though I did not find it neither to be deſtroyed, or much altered, by being put upon coral, or ſalt of tartar, as would happen to an acid menſtruum, nor yet by being digeſted with, and diſtilled from ſea ſalt, as might be probably expected from an alcalizate one: and among thoſe very bodies, which their taſtes perſuade chemiſts to reckon among acids, one may (according to what I formerly noted) obſerve ſo great a difference and variety of reliſhes, that, perhaps, without being too ſevere, I may ſay, that if I were to allow acids to be one principle, it ſhould be only in ſome ſuch metaphyſical ſenſe, as that wherein air is ſaid to be one body, though it conſiſt of the aſſociated effluviums of a multitude of corpuſcles of very differing natures, that agree in very little, ſave in their being minute enough to concur to the compoſition of a fluid aggregate, conſiſting of flying parts. But having dwelt longer than I intended on one objection, it is time, that I proceed to thoſe that remain.

CHAP. V.

ANOTHER particular, I am unſatisfied with in the hypotheſis of alcali and acidum, is, that it is in divers caſes either needleſs or uſeleſs to explain the phænomena of qualities, there being ſeveral of theſe produced, deſtroyed, or altered, where there does not appear any acceſſion, receſs, or change of either of thoſe two principles; as when fluid water by hard beating is turned into conſiſtent froth, and when tranſparent red coral is, barely by being beaten and ſifted finely, changed into a white and opacous powder; and as when a very flexible piece of fine ſilver being hammered is brought to have a briſk ſpring, and after a while will, inſtead of continuing malleable, crack or cleave under the hammer; and as when (to diſpatch and omit other inſtances) a ſufficiently thin leaf of gold, held between the light and the eye, appears green.

ANOTHER thing (of kin to the former,) that I like not in the doctrine of acidum and alcali, is, that though the patrons of it, whilſt they would ſeem to conſtitute but two principles, are fain, as I lately intimated, to make I know not how many differing ſorts of acids, beſides ſome variety of alcalies; yet their principles are too few and narrow, to afford any ſatisfactory explication of the phænomena. For I fear, it will be very difficult for them to give a rational account of gravity, ſpringineſs, light, and emphatical colours, ſounds, and ſome other qualities, that are wont to be called manifeſt; and much more of ſeveral, that are confeſſed to be occult, as electricity, and magnetiſm; in which laſt I ſee not, how the affirming, that there is in the magnet an acid and an alcali, and that theſe two are of contrary natures, will help to explain, how a load-ſtone does, as they ſpeak, attract the ſame end of a poiſed needle, with one of its poles, which it will drive away with the other, and determine that needle, when freely placed, to point north and ſouth, and enable it to communicate by its bare touch the ſame properties, and abundance of other ſtrange ones, to another piece of ſteel. But I forbear to alledge particular examples referable to the ſeveral qualities above-mentioned, whether manifeſt or hidden, becauſe that in great part is already done in our notes about particular qualities, in which it will appear, how little able the employing of alcali and acidum will be to afford us an account of many things. And though I enlarge not here on this objection, yet I take it to be of that importance, that, though there were no other, this were enough to ſhew, that the hypotheſis, that is liable to it, is inſufficient for the explication of qualities; and therefore it will not, I preſume, be thought ſtrange, that I add, that as for thoſe, that would extend this narrow chemical doctrine to the whole object of natural philoſophy, they muſt do more, than I expect they will be able, before they can make me their proſelyte, there being a multitude of phænomena in nature (divers whereof I elſewhere take notice of in reference to the chemiſt's philopſophy) in which what acidum and alkali have to do, I confeſs, I do not underſtand.

CHAP. VI.

THE laſt thing (which comprizes ſeveral others) that ſeems to me a defect in the doctrine of alcali and acidum, is, that divers, if not moſt of thoſe very things, that are pretended to be explicated by them, are not ſatisfactorily explicated, ſome things being taken into the explications, that are either not fundamental enough, or not clearly intelligible, or are chargeable with both thoſe imperfections.

AND firſt I am diſſatisfied with the very fundamental notion of this doctrine, namely a ſuppoſed hoſtility between the tribe of acids and that of alkalies, accompanied, if you will have it ſo, with a friendſhip or ſympathy with bodies belonging to the ſame tribe or family. For I look upon amity and enmity as affections of intelligent beings; and I have not yet found it explained by any, how thoſe appetites can be placed in bodies inanimate and devoid of knowledge, or of ſo much as ſenſe. And I elſewhere endeavour to ſhew, that what is called ſympathy and antipathy between ſuch bodies does, in great part, depend upon the actings of our own intellect, which ſuppoſing in every body an innate appetite to preſerve itſelf both in a defenſive and an offenſive way, inclines us to conclude, that that body, which, though deſignleſly, deſtroys or impairs the ſtate or texture of another body, has an enmity to it, though perhaps a ſlight mechanical change may make bodies, that ſeem extremely hoſtile, ſeem to agree very well and co-operate to the production of the ſame effects. As if the acid ſpirit of ſalt and the volatile alkali (as they will have it) that is commonly called ſpirit of urine be put together, they will, after a ſhort, though fierce conflict, upon a new contexture unite together into a ſalt, little, if at all, differing from ſal armoniac, in which the two reconciled principles will amicably join in cooling of water, diſſolving ſome metalline bodies, and producing divers other effects. And ſo, if upon a ſtrong ſolution of ſalt of pot-aſhes, or of ſalt of tartar, good ſpirit of nitre be dropped in a due proportion, after the heat and tumult and ebullition are over, the acid and the alkalizate ſalts will convene into ſuch a concretion as ſalt-petre, which is taken to be a natural body, either homogeneous, or at leaſt conſiſting of parts, that agree very friendly together, and conſpired to conſtitute the particular kind of ſalt, that chemiſts call nitre.

BUT the ſympathy and antipathy, that is ſaid to be betwixt inanimate bodies, I elſewhere more particularly conſider; and therefore I ſhall now add in the ſecond place, that the explications made of phænomena, according to the doctrine of alkali and acidum, do not, in my apprehenſion, perform what may be juſtly expected from philoſophical explications. It is ſaid indeed, that the acidum working on the alkali, or this upon that, produces

duces the effect propoſed; but that is only to tell us, what is the agent, that operates, and not the manner of the operation, or the means and proceſs, whereby it produces the effect propoſed, and it is this modus, that inquiſitive naturaliſts chiefly deſire to learn. And if it be ſaid, that it is by the mutual hoſtility of the principles, that the effect is produced, it may be anſwered, that beſides that that hoſtility itſelf is not, as we have juſt now obſerved, a thing clear, if ſo much as intelligible; this is ſo general and indeterminate a way of explicating things, as can afford little or no ſatisfaction to a ſearching and cautious naturaliſt, that conſiders how very numerous and very various the phænomena of qualities are.

CHAP. VII.

To clear up and to countenance what I have been now ſaying, I ſhall only take notice of ſome few obvious phænomena of one of the moſt familiar operations wherein acidum and alkali are ſuppoſed to be the grand agents. It is known to the very boys of chemiſts, that aqua regis will diſſolve gold, copper and mercury, and that with theſe metals, eſpecially with the ſecond, it will produce an intenſe degree of heat. If now the cauſe of this heat be demanded, it may be expected, that the patrons of the Duelliſts will anſwer, that it is from the action of the acid ſalts of the menſtruum upon the alkali they meet with in the metals. But not to mention how many things are here preſumed, not proved; nor that I know ſome acid menſtruums, and ſome much more evidently alkalizate bodies than theſe metals are, which yet do not upon their mixtures produce any ſenſible heat; not, I ſay, to mention theſe, it is eaſy to diſcern, that this anſwer names indeed two ſuppoſed efficients of heat, but does not explicate or declare how theſe agents produce that quality, which depends upon a certain vehement and various agitation of the ſingly inſenſible parts of bodies, whether the Duelliſts, or any other, though very differing cauſes, put them into a motion ſo modified. And therefore gold and copper, by bare concuſſion, may be brought to an intenſe degree of heat, without the acceſſion of any acid parts to work upon them. But then further, when we are told, that aqua regis by its acidity working on the metalline alkali makes a diſſolution of the metal; I am told indeed, what they think to be the agent in this change, but not at all ſatisfied how this agent effects it; for, copper being a very hard metal, and gold generally eſteemed by chemiſts the cloſeſt and compacteſt body in nature, I would gladly know, by what power and way ſuch weak, and probably either brittle or flexible bodies, as acid ſalts, are enabled with that force to disjoin ſuch ſolid and cloſely coherent copuſcles, as make up the viſible maſſes of copper and gold, nay, and ſcatter them with that violence, as, perhaps to toſs up multitudes of them into the air. And ſince in the diſſolution of theſe metals there is another phænomenon to be accounted for, as well as the forcing of the parts aſunder, namely the ſuſtentation of the metal in the menſtruum, the chemiſts would have much informed me, if they had well explained, how their acidum and alkali is able to ſuſtain and give fluidity to the corpuſcles of the diſſolved metal, which though it be but copper, is nine times as heavy as a bulk of water equal to it, and if it be gold, is nineteen times heavier than the liquor, that muſt keep it from ſinking; and at leaſt divers times heavier in ſpecie than the ſalts, that are mingled with the aqueous parts, can make the menſtruum compoſed of them both. Whereas trial has aſſured me, that, if a piece of wax, or any other ſuch matter, be made by leſs than the hundredth part heavier than an equal bulk of water, it will, when thoroughly immerſed, fall to the bottom, and reſt there. I might alſo aſk a further queſtion about theſe diſſolutions, as why, whereas aqua regis diſſolves mercury, without being much changed in colour by it, gold retains its own citrinity or yellowneſs in the ſolvent, and the ſolution of copper is of a colour, which being greeniſh-blew is quite differing from that of the metal, that affords it, as well as from that of the ſolvent? And I might recruit theſe with other queries not impertinent, but that theſe may ſuffice (for a ſample) on this occaſion, and allow me to conclude this chapter, by repreſenting one thing, which I would gladly recommend and inculcate to you, namely, that "Thoſe hypotheſes do not a little hinder the progreſs of humane knowledge, that introduce morals and politicks into the explications of corporeal nature, where all things are indeed tranſacted according to laws mechanical."

CHAP. VIII.

I MIGHT eaſily have been more copious in the inſtances annexed to the foregoing animadverſions, but that, being deſirous to be ſhort as well as clear, I purpoſely declined to make uſe of divers others, that ſeemed proper to be employed, and indeed might ſafely enough have been ſo, becauſe thoſe I have mentioned and eſpecially thoſe (which make a great part of them) that are mechanical, are not liable to the ſame exceptions, that I foreſaw might be made to elude the force of the examples I paſſed by. And though I think I could very well make thoſe foreſeen objections appear groundleſs or unſatisfactory; yet that could ſcarce be done without engaging in controverſies, that would prove more tedious than I judged them neceſſary.

And yet, although what I have ſaid in this excurſion be but a part of what I could ſay, I would not be thought to have forgot what I intimated at the beginning of it. For though the reaſons I alledged keep me from acquieſcing in the doctrine of alkali and acidum, as it is propoſed ander the notion of a philoſophical hypotheſis, ſuch as the Carteſian or Epicurean, which are each of them alledged by their embracers to be mechanical, and of a very Catholick extent; yet I deny not, that the conſideration of the Duelliſts (or the two jarring principles of alkali and acidum) may be of good uſe to Spagyriſts and phyſicians, as I elſewhere further declare. Nor do I pretend by the paſt diſcourſe, that queſtions one doctrine of the chemiſts, to beget a general contempt of their notions, and much leſs of their experiments. For the operations of chemiſtry may be miſapplied by the erroneous reaſonings of the artiſts, without ceaſing to be themſelves things of great uſe, as being applicable, as well to the diſcovery or confirmation of ſolid theories, as the production of new phænomena, and beneficial effects. And though I think, that many notions of *Paracelſus* and *Helmont*, and ſome other eminent Spagyriſts, are unſolid, and not worthy the veneration, that their admirers cheriſh for them; yet divers of the experiments, which either are alledged to favour theſe notions, or on other accounts are to be met with among the followers of theſe men, deſerve the curioſity, if not the eſteem, of the induſtrious inquirers into nature's myſteries. And looking upon chemiſtry in groſs as a diſcipline ſubordinate to phyſicks, even mechanical philoſophers may juſtly, in my opinion, think favourably of it, ſince, whatever imperfections, or, if they pleaſe, extravagancies there may be in the principles and explications of *Paracelſus* or other leading artiſts, theſe faults of the theorical part may be ſufficiently compenſated by the utilities, that may be derived from the practical part. And this I am the rather induced to ſay, becauſe the experiments, that chemiſtry furniſhes, may much aſſiſt a naturaliſt to rectify the erroneous theories, that oftentimes accompany them, and even thoſe (miſtakes) that are endeavoured to be evinced by them.

AND (to conclude) chemiſtry ſeems to deal with men, in reference to notions, as it does in reference to metals, aſſiſting wary men to detect the errors, unto which it may have miſled the unwary: for the ſame art, that has taught ſome to impoſe on others, (and perhaps themſelves firſt) by blanching copper, imitating gold, &c. does alſo ſupply ſaymaſters and refiners, with the means, by the cupel, cements, aqua fortis, &c. to examine, whether coins be true or falſe, and diſcover adulterate gold and ſilver to be counterfeit.

EXPE-

EXPERIMENTS
AND
NOTES,
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VOLATILITY.

ADVERTISEMENTS about the EXPERIMENTS and NOTES relating to CHEMICAL QUALITIES.

WHEN, after I had gone through the common operations of chemiſtry, I began to make ſome ſerious reflections on them, I thought it was pity, that inſtruments, that might prove ſo ſerviceable to the advancement of natural philoſophy, ſhould not be more ſtudiouſly and ſkilfully made uſe of to ſo good a purpoſe. I ſaw indeed, that divers of the chemiſts had, by a diligent and laudable employment of their pains and induſtry, obtained divers productions, and lighted on ſeveral phænomena, conſiderable in their kind, and indeed more numerous, than, the narrowneſs and ſterility of their principles conſidered, could well be expected. But I obſerved too, that the generality of thoſe, that buſy themſelves about chemical operations; ſome, becauſe they practiſe phyſick, and others, becauſe they either much wanted, or greedily coveted money, aimed, in their trials, but at the preparation of good medicines for the human body, or to diſcover the ways of curing the diſeaſes or imperfections of metals, without referring their trials to the advancement of natural philoſophy in general; of which moſt of the alchymiſts ſeem to have been ſo incurious, that not only they did not inſtitute experiments for that purpoſe, but overlooked and deſpiſed thoſe undeſigned ones, that occurred to them, whilſt they were proſecuting a preparation of a medicine, or a tranſmutation of metals. The ſenſe I had of this too general omiſſion of the chemiſts, tempted me ſometimes to try, whether I could do any thing towards the repairing of it by handling chemiſtry, not as a phyſician, or an alchymiſt, but as a meer naturaliſt, and ſo by applying chemical operations to philoſophical purpoſes. And, in purſuance of theſe thoughts, I remember I drew up a ſcheme of what I ventured to call a *chemia philoſophica*, not out of any affectation of a ſplendid title, but to intimate, that the chemical operations, there treated of, were not directed to the uſual ſcopes of phyſicians, or tranſmuters of metals, but partly to illuſtrate, or confirm ſome philoſophical theories by ſuch operations; and partly to explicate thoſe operations, by the help of ſuch theories.

BUT before I had made any great progreſs in the purſuit of this deſign, the fatal peſtilence, that raged in *London*, and in many other parts of *England*, in the years 1664 and 65, obliging me, among the reſt, to make ſeveral removes, which put me upon taking new meaſures, and engaging me in other employments of my time, made me ſo long neglect the papers I had drawn up, that, at laſt, I knew not where to find them, (though, I hope, they are not yet miſlaid beyond recovery,) which I was the leſs troubled at, becauſe the great difficulties, to be met with in ſuch an undertaking, did not a little diſcourage me, ſuch a taſk requiring, as well as deſerving, a perſon better furniſhed, than I had reaſon to think myſelf, with abilities, leiſure, chemical experiments, and conveniences, to try as many more, as ſhould appear needful. Beſides, to excite the [illegible] for any, that may hereafter think fit to ſet upon ſuch a work, or, to ſhorten my own labour, if I ſhould ſee cauſe to reſume it myſelf, I was content to throw in, among my notes about other particular qualities, ſome experiments and obſervations about ſome of thoſe, that I have elſewhere called chemical qualities, becauſe it is chiefly by the operations of chemiſts, that men have been induced to take ſpecial notice of them. Of theſe notes I have aſſigned to ſome qualities more, and to ſome fewer, as either the nature or importance of the ſubject ſeemed to require, or my leiſure and other circumſtances would permit. And though I have not here handled the ſubjects they belonged to, as if I intended ſuch a *chemia philoſophica*, as I lately mentioned, becauſe my deſign did not make it neceſſary, but did, perhaps, make it impertinent for me to do ſo; yet, in ſome of the larger notes, about volatility and fixedneſs, and eſpecially about precipitation, I have given ſome little ſpecimens of the theorical part of a philoſophical account of thoſe qualities, or operations, that, I hope, will not be wholly uſeleſs. I know, it may be objected, that I ſhould have employed, for inſtances, ſome more conſiderable experiments, if not arcana; but, though poſſibly I am not altogether unfurniſhed with ſuch, yet, aiming rather to promote philoſophy, than appear a poſſeſſor of elaborate proceſſes, I declined ſeveral experiments, that required either more ſkill, or more time, or more expence, than could be well expected from moſt readers, and choſe rather to employ ſuch experiments, as may be more eaſily or cheaply tried; and, which is mainly to be conſidered, being more ſimple, are more clearly intelligible, and more fit to have notions and theories built upon them; eſpecially conſidering, that the doctrine of qualities being itſelf converſant about ſome of the rudimental parts, if I may ſo call them, of natural philoſophy, it ſeemed unfit to employ intricate experiments, and whoſe cauſes were liable to many diſputes, to ſettle a theory of them. In ſhort, my deſign being to hold a taper, not ſo much to chemiſts, as to the naturaliſts, it was fit I ſhould be leſs ſollicitous to gratify the former, than to inform the latter.

EXPERIMENTS and NOTES, about the MECHANICAL ORIGIN and PRODUCTION of VOLATILITY.

CHAP. I.

As far as I have yet obſerved, the qualifications or attributes, on whoſe account a portion of matter is found to be volatile, are chiefly four; whereof the three former moſt regard the ſingle corpuſcles, as ſuch; and the laſt, the manner of their union in the aggregate or body they make up.

BUT before I enter upon particulars, give me leave to advertiſe you here, once for all, that, in the following notes about volatility and fixedneſs, when I ſpeak of the corpuſcles, or minute parts of a body, I do not mean ſtrictly either the elementary parts, ſuch as earth and water, or the hypoſtatical principles, ſuch as ſalt, ſulphur, or mercury; for theſe things come not here into conſideration: but only ſuch corpuſcles, whether of a ſimple, compounded, or decompounded nature, as have the particles they conſiſt of ſo firmly united, that they will not be totally disjoined, or diſſipated, by that degree of fire or heat, wherein the matter is ſaid to be volatile, or to be fixed. But thoſe combined particles will, in their aggregate, either aſcend, or continue unraiſed *per modum unius* (as they ſpeak) or as one entire corpuſcle. As in a corpuſcle of ſal armoniac, whether it be a natural or factitious thing, or whether it be perfectly ſimilar, or compounded of differing parts, I look upon the entire corpuſcle, as a volatile portion of matter; and ſo I do on a corpuſcle of ſulphur, though experience ſhews, when it is kindled, that it has great ſtore of acid ſalt in it, but which is not extricated by bare ſublimation: and ſo colcothar of vitriol falls under our conſideration, as a fixed body, without enquiring what cupreous or other mineral, and not totally fixed parts, may be united with the earthly ones; ſince the fires, we expoſe it to, do not ſeparate them.

AND this being premiſed in the general, I now proceed to ſome particulars. And firſt, to make a volatile body, the parts ſhould be very ſmall. For, *cæteris paribus*, thoſe, that are ſo, are more eaſily put into motion by the action of the fire, and other agents, and conſequently more apt to be elevated, when, by the determination of the movent, the ſituation of the neighbouring bodies, or other mechanical circumſtances, the agitated corpuſcles can continue their motion with leſs reſiſtance upwards, than any other way, (as either downwards, or horizontally.) And if, as it is highly probable, that, which in light bodies, or, at leaſt, in moſt of them, is wont to paſs for poſitive levity, be but a leſs degree of gravity, than that of thoſe contiguous bodies, that raiſe them; it will happen, that, in very many caſes, (for, I ſay, not in all,) the great proportion of the ſurface of a corpuſcle to its bulk, (which is uſually greater in the leſſer particles,) by making it more apt to be wrought on, either by the air agitated by the fire, or by the effluvia of kindled fuel, or by the impulſe of the ſhaken corpuſcles of the body itſelf, will much facilitate the elevation of ſuch a minute particle, by expoſing a greater portion of it to the action of the agent, as it will oftentimes alſo facilitate the renewed ſuſtentation of ſuch a ſmall body in the air, which reſiſts more the deſcent of particles, whoſe ſurfaces are large, than of others of the ſame gravity and bulk: as a leaf of paper diſplayed will much longer hover in the air, than if it were reduced into a ball or pellet. That this minuteneſs of particles may diſpoſe them to be carried upwards, by the impulſe of other bodies, and that of the agitated air, is very obvious to be obſerved: as we ſee, that horſes in a highway, though they be not able, with the ſtrokes of their feet, to make ſtones, or gravel, or clods of earth fly up, yet they will eaſily raiſe clouds of duſt, oftentimes mingled with the ſmaller grains of ſand. And, where timber is ſawing, the ſame wind, that will not, in the leaſt, move the beams, and ſcarce at all move the chips, will eaſily carry up the ſaw-duſt into the air. And we ſee in our chimneys, that the ſmoke readily aſcends, whilſt even ſmall clods of ſoot, which is but an aggregate of the particles of ſmoke, fall headlong down.

CHAP. II.

THE next qualification requiſite in the corpuſcles of volatile bodies is, that they be not too ſolid or heavy. For if they be ſo, though their bulk be very ſmall, yet, unleſs other circumſtances do much compenſate their weight, it will be very difficult to elevate them, becauſe of the great diſproportion of their ſpecific gravity to that of the air, (which contributes to ſuſtain and even raiſe many ſorts of volatile parts) and to the ſtrength of the igneous effluvia or other agents, that would carry them up. Thus we ſee, that filings of lead or iron, and even minium (which is the calx of lead) though the grains they conſiſt of be very ſmall, will not eaſily be blown up like common duſt, or meal, or other powders made of leſs ponderous materials.

A third qualification to be deſired in the corpuſcles, that ſhould make up a volatile body, is, that they be conveniently ſhaped for motion. For if they be of branched, hooked, or other very irregular or inconvenient figures, they will be apt to be ſtopped and detained by other bodies, or intangled among themſelves, and conſequently very difficult to be carried upwards, in regard that, whilſt they are thus faſtened, either to one another, or to any ſtable body, each ſingle corpuſcle is not only to be conſidered, as having its own peculiar bulk, ſince its coheſion with the other corpuſcle or body, that detains it, makes them fit to be looked upon *per modum unius*; that degree of heat, they are expoſed to, being preſumed uncapable of disjoining them. And this may be one reaſon, why water, though it be ſpecifically heavier than oil, yet is much more eaſily brought to exhale in the form of vapours than is oil, whoſe corpuſcles by the laſting ſtains they leave on cloth, wood, wool, &c. (which water will but tranſiently moiſten, not ſtain) ſeem to be of very intangling figures.

The fourth and laſt qualification requiſite in a volatile body is, that the parts do looſely adhere, or at leaſt be united in ſuch a manner, as does not much indiſpoſe them to be ſeparated by the fire in the form of fumes or vapours.

For he, that conſiders the matter, will eaſily grant, that, if the contexture of the corpuſcles, whereof a body conſiſts, be intricate, or their coheſion ſtrong, their mutual implication, or their adherence to each other, will make one part hinder another from flying ſeparately away, and their conjunction will make them too heavy or unweildy to be elevated together, as intire, though compounded parts. Thus we ſee, that in ſpring, or the beginning of ſummer, a wind, though not faint, is unable to carry off the lighteſt leaves of trees, becauſe they ſtick faſt to the bows and twigs on which they grow, but in Autumn, when that adheſion ceaſes, and the leaves ſit but looſely on, a wind no ſtronger than that they reſiſted before, will with eaſe blow them off, and perhaps carry them up a good way into the air. But here note, that it was not without ſome cauſe, that I added above, that in a fluid body, the parts ſhould at leaſt be united in ſuch a manner, as does not much indiſpoſe them to be ſeparated. For it is not impoſſible, that the parts of a body may, by the figures and ſmoothneſs of the ſurfaces, be ſufficiently apt to be put into motion, and yet be indiſpoſed to admit ſuch a motion as would totally ſeparate them and make them fly up into the air. As, if you take two pieces of very flat and well-poliſhed marble or glaſs, and lay them one upon the other, you eaſily make them ſlide along each others ſurfaces, but not eaſily pull up one of them, whilſt the other continues its ſtation. And when glaſs is in the ſtate of fuſion, the parts of it will eaſily ſlide along each other, as is uſual in thoſe of other fluids, and conſequently change places, and yet the continuity of the whole is not intirely broken, but every corpuſcle does ſomewhere touch ſome other corpuſcle, and thereby maintain the coheſion, that indiſpoſes it for that intire ſeparation accompanied with a motion upwards, that we call avolation. And ſo, when ſaltpetre alone is in a crucible expoſed to the fire, though a very moderate degree of it will ſuffice to bring the ſalt to a ſtate of fuſion, and conſequently to put the corpuſcles, that compoſe it, into a reſtleſs motion; yet a greater degree of heat, than is neceſſary to melt it, will not extricate ſo much as the ſpirits, and make them fly away.

CHAP. III.

THE foregoing doctrine of the volatility of bodies may be as well illuſtrated as applied, if we proceed to deduce from it the general ways of volatiliſation of bodies, or of introducing volatility into an aſſigned portion of matter. For theſe ways ſeem not inconveniently reducible to five, which I ſhall ſeverally mention, though nature and art do uſually employ two or more of them in conjunction. For which reaſon I would not, when I ſpeak of one of theſe ways, be underſtood, as if, excluding the reſt, I meant, that no other concurred with it.

THE firſt of the five ways or means of volatilizing a body is, to reduce it into minute parts, and, *cæteris paribus*, the more minute they are the better.

THAT the bringing a body into very minute parts may much conduce to the volatilizing of it, may be gathered from the vulgar practice of the chemiſts, who, when they would ſublime or diſtil antimony, ſal armoniac, ſea-ſalt, nitre, &c. are wont to beat them to powders to facilitate their receiving a further comminution by the action of the fire. And here I obſerve, that in ſome bodies this comminution ought not to be made only at firſt, but to be continued afterwards. For chemiſts find by experience, though perhaps, without conſidering the reaſon of it, that ſea-ſalt and nitre will very hardly afford their ſpirits in diſtillations, without they be mingled with powdered clay or bole, or ſome ſuch other additament, which uſually twice or thrice exceeds the weight of the ſalt itſelf: although theſe additaments, being themſelves fixed, ſeem unlikely to promote the volatilization of the bodies mixed with them, yet by hindering the ſmall grains of ſalt to melt together into one lump or maſs, and conſequently by keeping them in the ſtate of comminution, they much conduce to the driving up of the ſpirits, or the finer parts of the ſalts by the operation of the fire.

BUT to proſecute a little what I was ſaying of the conduciveneſs of bringing a body into ſmall parts to the volatilization of it, I ſhall add, that in ſome caſes the comminution may be much promoted by employing phyſical, after mechanical, ways; and that, when the parts are brought to ſuch a pitch of exiguity, they may be elevated much better than before. Thus, if you take filings of Mars, and mix them with ſal armoniac, ſome few parts may be ſublimed; but if, as I have done, you diſſolve thoſe filings in good ſpirit of ſalt, inſtead of oil of vitriol, and having coagulated the ſolution, you calcine the greeniſh cryſtals or *vitriolum martis*, that will be afforded, you may with eaſe, and in no long time, obtain a *crocus martis* of very fine parts; ſo that I remember, when we exquiſitely mingled this very fixed powder with a convenient proportion of ſal armoniac, and gradually preſſed it with a competent fire, we were able to elevate at the firſt ſublimation a conſiderable part of it; and adding a like, or ſom what inferior, proportion of freſh ſal armoniac to the *caput mortuum*, we could raiſe ſo conſiderable a part of that alſo, and in it of the crocus, that we thought, if we had had conveniency to purſue the operation, we ſhould, by not many repeated ſublimations, have elevated the whole crocus, which (to hint that, upon the by,) afforded a ſublimate of ſo very aſtringent a taſte, as may make the trial of it in ſtanching of blood, ſtopping of fluxes, and other caſes, where potent aſtriction is deſired, worthy of a phyſician's curioſity.

CHAP. IV.

THE ſecond means to volatilize bodies is, to rub, grind, or otherwiſe reduce their corpuſcles to be either ſmooth, or otherwiſe fitly ſhaped to clear themſelves, or be diſintangled from each other.

BY reaſon of the minuteneſs of the corpuſcles, which keeps them from being ſeparately diſcernible by the eye, it is not to be expected, that immediate and ocular inſtances ſhould be given on this occaſion; but that ſuch a change is to be admitted in the ſmall parts of many bodies, brought to be volatile, ſeems highly probable from the account formerly given of the requiſites or conditions of volatility, whoſe introduction into a portion of matter, will ſcarce be explicated without the intervention of ſuch a change. To this ſecond inſtrument of volatilization, in concurrence with the firſt, may probably be referred the following phænomena: in the two firſt of which there is employed no additional volatile ingredient; and in the fourth, a fixed body is diſpoſed to volatility by the operation of a liquor, though this be carefully abſtracted from it.

1. IF urine freſhly made be put to diſtil, the phlegm will firſt aſcend, and the volatile ſalt will not riſe till that be almoſt totally driven away, and then requires a not inconſiderable degree of fire to elevate it. But, if you putrify or digeſt urine, though in a well-cloſed glaſs-veſſel, for ſeven or eight weeks, that gentle warmth will make the ſmall parts ſo rub againſt, or otherwiſe act upon, one another, that the finer ones of the ſalt, will perhaps, be made more ſlender and light, and however will be made to extricate themſelves ſo far, as to become volatile, and, aſcending in a very gentle heat, leave the greateſt part of the phlegm behind them.

2. So, if muſt, or the ſweet juice of grapes, be diſtilled, before it have been fermented, it is obſerved by chemiſts, and we have tried the like in artificial wine made of raiſins, that the phlegm, but no ardent ſpirit, will aſcend. But when this liquor is reduced to wine by fermentation, which is accompanied with a great and inteſtine commotion of the juſtling parts, hitting and rubbing againſt one another, whereby ſome probably come to be broken, others to be variouſly ground and ſubtilized, the more ſubtile parts of the liquor being extricated, or ſome of the parts being, by theſe operations, brought to be ſubtile, they are qualified to be raiſed by a very gentle heat before the phlegm, and convene into that fugitive liquor, that chemiſts, for its activity, call ſpirit of wine. Nor is it only in the ſlighter inſtances afforded by animals and vegetables, that volatility may be effected by the means lately mentioned; for experience hath aſſured me, that it is poſſible, by an artificial and long digeſtion, wherein the parts have leiſure for

frequent juſtlings and attritions, ſo to ſubtilize and diſpoſe the corpuſcles, even of common ſalt, for volatility, that we could make them aſcend in a moderate fire of ſand, without the help of bole, oil of vitriol, or any volatilizing additament; and, which is more conſiderable, the ſpirit would, in riſing, precede the phlegm, and leave the greateſt part thereof behind it.

THIS inteſtine commotion of parts, capable of producing volatility in the more diſpoſed portions of a body, though it be much more eaſy to be found in liquors, or in moiſt and ſoft bodies, yet I have ſometimes, though rarely, met with it in dry ones. And particularly I remember, that, ſome years ago, having, for trial-ſake, taken muſtard-ſeed, which is a body pregnant with ſubtile parts, and cauſed it to be diſtilled *per ſe* in a retort, I had, as I hoped, (without any more ado,) a great many grains of a clear and figured volatile ſalt at the very firſt diſtillation: which experiment having, for the greater ſecurity, made a ſecond time with the like ſucceſs, I mentioned it to ſome lovers of chemiſtry, as what, I juſtly ſuppoſed, they had not heard of. I leave it to farther enquiry, whether, in a body ſo full of ſpirits, as muſtard-ſeed, the action or re-action of the parts among themſelves, perhaps promoted by juſt degrees of fire, might not ſuffice to make in them a change equivalent in order to volatilization, and the yielding a volatile ſalt, to that, which we have obſerved fermentation and putrefaction to have made in the juice of grapes, urine, and ſome other bodies. How far the like ſucceſs may be expected in other trials, I cannot tell; eſpecially, not having by me any notes of the events of ſome attempts, which that enquiry put me upon: only, I remember in general, that, as ſome trials, I made with other ſeeds, and even with aromatic ones, did not afford me any volatile ſalt; ſo the ſucceſs of other trials made me now and then think, that ſome ſubjects of the vegetable kingdom, whence we are wont to drive over acid ſpirits, but no dry ſalt, may be diſtilled with ſo luckily regulated a heat, as to afford ſomething, though but little, of volatile ſalt; and, that perhaps more bodies would be found to do ſo, were they not too haſtily or violently preſſed by the fire, whereby ſuch ſaline ſchematiſms of the deſired parts of the matter are (by being diſſipated or confounded) deſtroyed or vitiated, as in a ſlow, dextrous, or fortunate way of management would come forth, not in a liquid, but a ſaline form. Of which obſervation, we may elſewhere mention ſome inſtances, and ſhall, before the cloſe of this paper, name one, afforded us by crude tartar.

3. THOUGH ſilver be one of the fixedeſt bodies, that we know of, yet, that it is not impoſſible, but that, chiefly by a change of texture, it may ſtrangely be diſpoſed to volatility, I was induced to think, by what I remember once happened to me. A gentleman of my acquaintance, ſtudious of chemical arcana, having lighted on a ſtrange menſtruum, which he affirmed, and I had ſome cauſe to believe, not to be corroſive, he abſtracted it from ſeveral metals, (for the ſame liquor would ſerve again and again,) and brought me the remainders, with a deſire, that I would endeavour to reduce thoſe of lead and ſilver into the priſtine metals again, which he had, in vain, attempted to do: whereupon, though I found the white calx of lead reducible, yet, when I came to the calx of ſilver, I was not able to bring it into a body; and having, at length, melted ſome lead in a gentle fire, to try whether I could make it ſwallow up the calx, in order to a farther operation, I was not a little ſurprized to find, that this mild heat made the calx of ſilver preſently fly away, and ſublime in the form of a *farina volatilis*, which whitened the neighbouring part of the chimney, as well as the upper part of the crucible.

4. From that, which chemiſts themſelves tell us, I think we may draw a good argument *ad hominem*, to prove, that volatility depends much upon the texture, and other mechanical affections of a body. For divers of thoſe Hermetick philoſophers (as they are called) that write of the elixir, tell us, that when their philoſophick mercury or grand ſolvent, being ſealed up together with a third or fourth part of gold in a glaſs egg, is kept in convenient degrees of fire, the whole matter, and conſequently the gold, will, by the mutual operation of the included ſubſtances, be ſo changed, that not only it will circulate up and down in the glaſs, but, in caſe the digeſtion or decoction ſhould be broken off at a certain inconvenient time, the gold would be quite ſpoiled, being, by the paſt and untimely ended operation, made too volatile to be reducible again into gold: whereas, if the decoction be duly continued unto the end, not only the gold, but all the philoſophical mercury or menſtruum will be turned into a ſulphur, or powder of a wonderfully fixed nature. I know, there are ſeveral Chryſopæans, that ſpeak much otherwiſe of this operation, and tell us, that the gold employed about it muſt be philoſophic gold: but I know too, that there are divers others, (and thoſe too none of the leaſt candid or rational,) that ſpeak of it, as I have done; and that is ſufficient to ground an argument on towards all thoſe, that embrace their doctrine. And, in this caſe, it is conſiderable, that it is not by any ſuperadded additament, that the moſt fixed body of gold is made volatile, but the ſame maſſy matter, conſiſting of gold and philoſophic mercury, is, by the change of texture produced, or occaſioned by the various degrees and operations of fire upon it, brought to be firſt volatile, and then extreamly fixed. And having ſaid this, in reference to one tribe of the modern Spagyriſts, to another of them, the *Helmontians*, I think, I can offer a good argument *ad hominem* from the teſtimony and experiments of the founder of their ſect.

5. THE acute *Helmont*, among other prodigious powers, that he aſcribes to the alkaheſt, affirms, that, by abſtracting it frequently enough, it would ſo change all tangible bodies, and conſequently ſtones and metals, that they might be diſtilled over into liquors equiponderant to the reſpective bodies, that afforded

them, and having all the qualities of rain water; which if they have, I need not tell you, that they muſt be very volatile. And I ſee not how thoſe, that admit the truth of this ſtrange alkaheſtical operation, can well deny, that volatility depends upon the mechanical affections of matter, ſince it appears not, that the alkaheſt does, at leaſt in our caſe, work upon bodies otherwiſe than mechanically. And it muſt be confeſſed, that the ſame material parts of a portion of a corporeal ſubſtance, which, when they were aſſociated and contexed (whether by an archeus, ſeed, form, or what elſe you pleaſe,) after ſuch a determinate manner, conſtituted a ſolid and fixed body, as a flint or a lump of gold; by having their texture diſſolved, and (perhaps after being ſubtilized) by being freed from their former implications, or firm coheſions, may become the parts of a fluid body totally volatile.

CHAP. V.

THE fourth means of making a body volatile is, by aſſociating the particles to be raiſed with ſuch as are more volatile than themſelves, and of a figure fit to be faſtened to them, or are at leaſt apt, by being added to them, to make up, with them, corpuſcles more diſpoſed than they to volatility. This being the grand inſtrument of volatilization, I ſhall ſpend ſomewhat the more time about it. But I ſhall firſt here a little explain the laſt clauſe, (that I may not be obliged to reſume it elſewhere) by intimating, that it is not impoſſible, that the particles of an additament, though not more volatile than thoſe of the body it is mixed with, and perhaps, though not volatile at all, will yet conduce to volatilize the body wherewith it is mingled. For the particles of the additament may be of ſuch figures, and ſo aſſociated with thoſe of the body to be elevated, as in this to enlarge the former pores, or produce new ones, by intercepting little cavities (for they muſt not be great ones) between the particles of a body to be raiſed, and thoſe of the additament. For, by theſe and other ſuch ways of aſſociation, the corpuſcles, reſulting from the combination or coalition of two or more of theſe differing particles, may, without becoming too big and unweildy, become more conveniently ſhaped, or more light in proportion to their bulk, and ſo more eaſily buoyed up and ſuſtained in the air, (as when the lid of a copper box being put on, makes the whole box emerge and ſwim in water, becauſe of the intercepted cavity, though neither of the parts of the box would do ſo,) or otherwiſe more fitted for avolation than the particles themſelves were, before their being joined to thoſe of the additament.

BY two things, chiefly, the corpuſcles of the additament may contribute to the elevation of a body. For, firſt, the parts of the former may be much more diſpoſed for avolation than is neceſſary to their own volatility. As when in the making of ſal armoniac, the ſaline particles of urine and of ſoot, are more fugitive than they need be, to be themſelves ſublimed, and thereby are advantaged to carry up with them the more ſluggiſh corpuſcles, whereof ſea ſalt conſiſts. And next, they may be of figures ſo proper to faſten them well to the body to be elevated, that the more fugitive will not be driven away, or disjoined from the more fixed by ſuch a degree of heat as is ſufficient to raiſe them both together: to which effect the congruity, or figuration, is as well required, as the lightneſs or volatility of the particles of the additament. And therefore ſome of the fugitiveſt bodies, that we know, as ſpirit of wine, camphire, &c. will not volatilize many bodies, which will be elevated by far leſs fugitive additaments; becauſe the corpuſcles of ſpirit of wine ſtick not to thoſe of the body they are mingled with, but, eaſily flying up themſelves, leave thoſe behind them, which they did rather barely touch, than firmly adhere to: whereas far leſs fugacious liquors, if they be endowed with figures, that fit them for a competently firm coheſion with the body they are mingled with, will be able to volatilize it. Of which I ſhall now give you ſome inſtances in bodies, that are very ponderous, or very fixed, or both.

AND I ſhall begin with colcothar, though it being a vitriolate calx, made by a laſting and vehement fire, it is (conſequently) capable of reſiſting ſuch a one. This being exquiſitely ground with an equal weight of ſal armoniac, which is itſelf a ſalt, but moderately volatile, will be in good part ſublimed into thoſe yellow flowers, which we have elſewhere more particularly taught to prepare, under the name of *ens primum veneris*; in which, that many vitriolate corpuſcles of the colcothar are really elevated, you may eaſily find, by putting a grain or two of that reddiſh ſubſtance into a ſtrong infuſion of galls, which will thereby immediately acquire an inky colour.

STEEL alſo, which, to deſerve that name, muſt have endured extraordinary violences of the fire, and greater than is needful to obtain other metals from their mother earth; ſteel itſelf, I ſay, being reduced to filings, and diligently ground with about an equal weight of ſal armoniac, will, if degrees of fire be ſkilfully adminiſtred, (for it is eaſy to err in that point,) without any previous calcination or reduction to a crocus, ſuffer ſo much of the metal to be carried up, as will give the ſal armoniac a notable colour, and an ironiſh taſte.

AND here it will be proper to obſerve, for the ſake of practical chemiſts, that the quantity or proportion of the volatile additament is to be regarded; though not ſo much as its nature, yet more than it is wont to be: and divers bodies, that are thought either altogether unfit for ſublimation, or, at leaſt, uncapable to have any conſiderable portion of them elevated, may be copiouſly enough ſublimed, if a greater proportion of the additament, than we uſually content our ſelves with, be ſkilfully employed. And in the newly-mentioned inſtance of filings of ſteel, if, inſtead of an equal weight of ſal armoniac, the treble weight be taken, and the operation be duly managed, a far greater quantity of the metal may be raiſed, eſpecially if freſh ſal armoniac be carefully ground with the caput mortuum. And ſal armoniac may, perhaps, be compounded with ſuch other bodies, heavier than itſelf, as may qualify it, when it is thus clogged, to elevate ſome congruous bodies better than it would of itſelf alone. And I ſhall venture to add this farther advertiſement, that if, beſides the plenty of the additament, there be a ſufficient fitneſs of its particles to lay hold on thoſe of the body to be wrought on, mineral bodies, and thoſe ponderous enough, may be employed to volatilize other heavy bodies. And I am apt to think, that almoſt, if not more than almoſt, all metals themſelves may by copious additaments and frequent cohobations be brought to paſs through the neck of the retort in diſtillation; and perhaps, if you melt them not with equal parts, but with many parts of regulus of antimony, and then proceed as the hints now given will direct you, you will not find cauſe to deſpiſe what I have been ſaying.

YOU know what endeavours have been, and are ſtill fruitleſly employed by chemiſts to elevate ſo fixed a body, as ſalt of tartar, by additaments. I ſhall not now ſpeak much of the enterprize in general, deſigning chiefly to tell you on this occaſion, that, whereas frequent experience ſhews, that ſal armoniac being abſtracted from ſalt of tartar, not only the ſalt of tartar is left at the bottom, but a good part of the ſal armoniac is left behind with it: I ſuſpected the cauſe might be, that ſal armoniac, by the operation of the alkali of tartar, is reduced into ſea-ſalt, and urinous or fuliginous ſalt, as it was at firſt compoſed of thoſe differing ingredients; and that by this means the volatile ſalt being looſened or diſintangled from the reſt, and being of a very fugacious nature, flies eaſily away itſelf, without ſtaying long enough to take up any other ſalt with it. And therefore, if this analyſis of the ſal armoniac could be prevented, it ſeemed not impoſſible to me, that ſome part of the ſalt of tartar, as well as of colcothar and ſteel, might be carried up by it: and accordingly having cauſed the ingredients to be exceedingly well dried, and both nimbly and carefully mixed, and ſpeedily expoſed to the fire, I have ſometimes had a portion of ſalt of tartar carried up with the ſal armoniac: but this happened ſo very rarely, that I ſuſpected ſome peculiar fitneſs for this work in ſome parcels of ſal armoniac, that are ſcarce but by the effect to be diſcerned from others. But however, what has happened to us may argue the poſſibility of the thing, and may ſerve to ſhew the volatilizing efficacy of ſal armoniac; which is a compound, that I elſewhere recommend, and do it now again, as one of the uſefulleſt productions of vulgar chemiſtry.

AND ſince I have mentioned the volatilization of ſalt of tartar, preſuming your curioſity will make you deſire my opinion about the poſſibility of it, I ſhall propoſe to you a diſtinction, that perhaps you do not expect, by ſaying, that I think there is a great deal of difference between the making a volatile ſalt of tartar, and the making ſalt of tartar volatile. For though this ſeem to be but a nicety, yet really it is none; and it is very poſſible, that a man may from tartar obtain a volatile ſalt, and yet be no wiſe able to volatilize that tartareous ſalt, that has been once by the incineration of the tartar brought to fixed alkali. I have in the Sceptical Chemiſt ſummarily delivered a way, by which both I, and ſome Spagyriſts, that learned it of me, obtained from a mixture of antimony, nitre and crude tartar, a volatile ſalt, which in probability comes from the laſt named of thoſe three bodies; but experience carefully made has aſſured me, that without any additament, by a diſtillation warily and very ſlowly made, (inſomuch that I have ſpent near a week in diſtilling one pound of matter) very clean tartar, or at leaſt the cryſtals of tartar, may, in conveniently ſhaped veſſels, be brought to afford a ſubſtance, that in rectification will aſcend to the upper part of the veſſel, in the form of a volatile ſalt, as if it were of urine or of hart's-horn; of which tartareous ſalt, I keep ſome by me: but this operation requires not only a dexterous, but a patient diſtiller.

BUT now as to the making a fixed alkali of tartar become volatile, I take it to be another, and have found it to be a far more difficult, work; the common proceſſes of performing it being wont to promiſe much more than they can make good; which I may juſtly ſay of ſome other, that private men have vaunted for great arcana, but upon trial have ſatisfied me ſo little, that I have divers times offered pretenders to make ſalt of tartar volatile, that without at all inquiring into their proceſſes, I would lay good wagers, that they could do what they pretended; not only as divers philoſophical Spagyriſts require, without any viſible additament, but by any additament whatever; provided I were allowed to bring the ſalt of tartar myſelf, and to examine the ſucceſs, not by what may appear in the alembic and receiver, but by the weight of what would remain in the bottom. For I have convinced ſome of the more ingenuous artiſts, that the ſalt, that ſublimed, was not indeed the alkali of tartar, but ſomewhat, that was by the operation produced, or rather extricated out of the additaments. But yet I would not be thought to affirm, that it is not poſſible to elevate the fixed ſalt of tartar. For ſometimes I have been able to do it, even at the firſt diſtillation by an artificial additament perhaps more fixed than itſelf; but, though the operation was very grateful to me, as it ſhewed the poſſibility of the thing, yet the paucity of the ſalt ſublimed and other circumſtances, kept me from much valuing it upon any other account. And there are other ways, whereby experience has aſſured me, that ſalt of tartar may be raiſed. And if one of them were not ſo uncertain, that I can never promiſe before hand, that it will at all ſucceed, and the other ſo laborious, difficult and coſtly, that few would attempt or be able to practice it, I ſhould think them very valuable things; ſince by the former way moſt part of the ſalt of tartar was quickly brought over in the form of a liquor, whoſe piercing ſmell was ſcarce tolerable; and by the latter way ſome ſalt of tartar of my own, being put into a retort, and urged but with ſuch a fire as could be given in a portable ſand-furnace, there remained not at the bottom near one half of the firſt weight, the additament having carried up the reſt, partly in the form of a liquor, but chiefly in that of a white ſublimate, which was neither ill-ſcented, nor in taſte corroſive, or alkalizate, but very mild, and ſomewhat ſweetiſh. And I do not much doubt, but that by other ways the fixed alkali of tartar may be elevated, eſpecially if, before it be expoſed to the laſt operation of the fire, it be dexterouſly freed from the moſt of thoſe earthy and viſcuous parts, that I think may be juſtly ſuſpected to clog and bind the truly ſaline ones.

BUT I have too long digreſſed, and therefore ſhall intimate only upon the by, that even the ſpurious ſal tartari volatilized, that is made with ſpirit of vinegar, may, if it be well prepared, make amends for its empyreumatical ſmell and taſte, and may, notwithſtanding them, in divers caſes be of no deſpicable uſe, both as a medicine, and a menſtruum.

CHAP. VI.

BEFORE I draw towards a concluſion of theſe notes about volatility, perhaps it will not be amiſs, to take notice of a phænomenon, which may much ſurprize, and ſometimes diſappoint thoſe, that deal in ſublimations, unleſs they be forewarned of it. For though it be taken for granted, and for the moſt part may juſtly be ſo, that by carefully mingling what is ſublimed with what remains, and reſubliming the mixture, a greater quantity of the body to be ſublimed may be elevated the ſecond time than was the firſt, and the third time than the ſecond, and ſo onwards; yet I have not found this rule always to hold, but in ſome bodies, as particularly in ſome kinds of dulcified colcothar, the ſal armoniac, would at the firſt ſublimation carry up more of the fixed powder, than at the ſecond or third. So that I was by ſeveral trials perſuaded, when I found a very well and highly coloured powder elevated, to lay it by for uſe, and thereby ſave my ſelf the labour of a proſecution, that would not only have proved uſeleſs, but prejudicial. And if I miſremember not, by often repeated cohobations, if I may ſo call them, of ſal armoniac upon crude or mineral antimony, though the ſublimate, that was obtained by the firſt operation, was much of it variouſly, and in ſome places richly coloured; yet afterwards, the ſalt aſcended from time to time paler and paler, leaving the antimony behind it. Which way of making ſome minerals more fixed and fuſible I conceive may be of great uſe in ſome medicinal preparations, though I think it not fit to particularize them in this place: where my chief intent was, to mention the phænomenon itſelf, and invite you to conſider, whether it may be aſcribed to this, that by the reiterated action of the fire, and grinding together of the body to be raiſed, either the corpuſcles of the ſal armoniac, or thoſe of the other body, may have thoſe little hooked or equivalent particles, whereby they take hold of one another, broken or worn off; and whether the indiſpoſedneſs of the colcotharine or antimonial parts to aſcend, may not in ſome caſes be promoted by their having, by frequent attritions, ſo ſmoothed their ſurfaces, that divers of them may cloſely adhere, like pieces of poliſhed glaſs, and ſo make up cluſters too unweildy to be ſo raiſed, as the ſingle corpuſcles they conſiſt of, were. Which change may diſpoſe them to be at once leſs volatile and more fuſible. Which conjectures I mention to excite you to frame better, or at leaſt to make amends for my omiſſion of examining theſe, by, trying whether the ſal armoniac, grown white again, will be as fit as it was at firſt to carry up freſh bodies; and alſo by obſerving the weight of the unelevated part, and employing thoſe other ways of examen, which I ſhould have done, if I had not then made ſublimations for another end, than to clear up the doctrine of volatility.

AND here it may be profitable to ſome chemiſts, though not neceſſary to my ſubject, to intimate, that ſublimations may be uſeful to make very fine comminutions of divers bodies. That thoſe, that are elevated are reduced to a great fineneſs of parts, is obvious to be obſerved in many examples, whence it has been anciently, not abſurdly, ſaid, that ſublimations are the chemiſts peſtles, ſince (as in flowers of ſulphur and antimony) they do really reſolve the elevated bodies into exceeding fine flower, and much finer than peſtles and mortars are wont to bring them to. But that, which I intend in this paragraph, is not a thing ſo obvious, ſince it is to obſerve, that ſometimes even bodies ſo fixed, as not at all to aſcend in ſublimation, may yet be reduced by that operation into powders extremely fine. For exemplifying of which, I ſhall put you in mind, that though Spagyriſts complain much of the difficulty of making a good calx of gold, and of the imperfection of the few ordinary proceſſes preſcribed to make it, (which would be more complained of, but that chemical phyſicians ſeldom attempt to prepare it,) yet we are informed by trial, that by exactly grinding a thick amalgam of gold and mercury with a competent weight, (at leaſt equal to its own) of finely powdered ſulphur, we may, by putting the mixture to ſublime in a conveniently ſhaped glaſs, by degrees of fire obtain a cinaber, that will leave behind it a finer calx of gold, than will be had by ſome far more difficult proceſſes.

BUT it is now time to draw towards a concluſion of our notes about volatility; which quality depends ſo much upon the contexture of the corpuſcles, that are to be raiſed together, that even very ponderous bodies may ſerve for volatilizing additaments, if they be diſpoſed to faſten themſelves ſufficiently to the bodies they are to carry up along with them. For, though lead be, ſave one, the heavieſt ſolid we know of, and though quick-ſilver be the heavieſt body in the world, except gold;

yet trials have aſſured us, that quick-ſilver itſelf being united by amalgamation with a ſmall proportion of lead, will, by a fire, that is none of the violenteſt, and in cloſe veſſels, be made to carry over with it ſome of the lead. As we clearly found by the increaſed weight of the quick-ſilver, that paſſed into the receiver; which, by the way, may make us cautious, how we conclude quick-ſilver to be pure, merely from its having been diſtilled over.

THERE remains but one body more heavy than thoſe I come from naming, and that is gold; which, being alſo of a fixity ſo great, that it is indeed admirable, I do not wonder, that, not only the more wary naturaliſts, but the more ſevere among the chemiſts themſelves, ſhould think it incapable of being volatilized. But yet, if we conſider, how very minute parts gold may be rationally ſuppoſed to conſiſt of, and to be diviſible into, methinks it ſhould not ſeem impoſſible, that, if men could light on volatile ſalts endowed with figures fit to ſtick faſt to the corpuſcles of the gold, they would carry up with them bodies, whoſe ſolidity can ſcarce be more extraordinary, than their minuteneſs is: and, in effect, we have made more than one menſtruum, with which ſome particles of gold may be carried up. But when I employed that, which I recommended to you formerly, under the name of *menſtruum peracutum*, (which conſiſts mainly, and ſometimes only of ſpirit of nitre, ſeveral times drawn from butter of antimony,) I was able, without a very violent fire, in a few hours, to elevate ſo much crude gold, as, in the neck of the retort, afforded me a conſiderable quantity of ſublimate, which I have had red as blood, and whoſe conſiſting partly of gold, manifeſtly appeared by this, that I was able, with eaſe, to reduce that metal out of it.

IN reckoning up the inſtruments of volatilization, we muſt not quite leave out the mention of the air, which I have often obſerved to facilitate the elevation of ſome bodies, even in cloſe veſſels; wherein, though to fill them too full be judged, by many, a compendious practice, becauſe the ſteams have a leſs way to aſcend, yet experience has ſeveral times informed me, that, at leaſt in ſome caſes, they take wrong meaſures, and that (to paſs by another cauſe of their diſappointment) a large proportion of air, purpoſely left in the veſſels, may more than compenſate the greater ſpace, that is to be aſcended by the vapours or exhalations of the matter, that is to be diſtilled or ſublimed. And if, in cloſe veſſels, the preſence of the air may promote the aſcenſion of bodies, it may well be expected, that the elevation of divers of them may be furthered, by being attempted in open veſſels, to which the air has free acceſs. And if we may give any credit to the probable relations of ſome chemiſts, the air does much contribute to the volatilization of ſome bodies, that are barely, though indeed for no ſhort time, expoſed to it. But the account, on which the air, by its bare preſence, or peculiar operations, conduces to the volatilization of ſome bodies, is a thing very difficult to be determined, without having recourſe to ſome notions about gravity and levity, and of the conſtitution of the corpuſcles, that compoſe the air; which I take to be both very numerous, and no leſs various. And therefore I muſt not, in theſe occaſional notes, launch out into ſuch a ſubject, though, for fear I ſhould be blamed for too much ſlighting my old acquaintance the air, I durſt not quite omit the power it has to diſpoſe ſome bodies to volatility.

A moderate attention may ſuffice, to make it be diſcerned, that, in what hath been hitherto delivered, I have, for the moſt part, conſidered the ſmall portions of matter, to be elevated in volatilization, as entire corpuſcles: and therefore it may be now pertinent, to intimate in a line or two, that there may be alſo caſes, wherein a kind of volatilization, improperly ſo called, may be affected, by making uſe of ſuch additaments, as break off, or otherwiſe divide the particles of the corpuſcles to be elevated, and by adhering to, and ſo clogging one of the particles, to which it proves more congruous, enable the other, which is now brought to be more light, or diſengaged, to aſcend. This may be illuſtrated by what happens, when ſal armoniac is well ground with lapis calaminaris, or with ſome fixed alkali, and then committed to diſtillation: for the ſea-ſalt, that enters the compoſition of the ſal armoniac, being detained by the ſtone or the alkali, there is a divorce made between the common ſalt and the urinous and fuliginous ſalts, that were incorporated with it, being now diſengaged from it, are eaſily elevated. I elſewhere mention, that I have obſerved, in man's urine, a kind of native ſal armoniac, much leſs volatile than the fugitive, that is ſublimed from man's blood, hartſhorn, &c. and therefore ſuppoſing, that a ſeparation of parts may be made by an alkali, as well in this ſalt, as in the common factitious ſal armoniac, I put to freſh urine a convenient proportion (which was a plentiful one) of ſalt of pot-aſhes, (that being then at hand,) and diſtilling the liquor, it yielded, according to expectation, a ſpirit more volatile than the phlegm, and of a very piercing taſte; which way of obtaining a ſpirit without any violence of fire, and without either previouſly abſtracting the phlegm, (as we are fain to do in freſh urine) or tediouſly waiting for the fermentation of ſtale urine, I taught ſome chemiſts, becauſe of the uſefulneſs of ſpirit of urine; which being obtained this innocent way, would probably be employed with much leſs ſuſpicion of corroſiveneſs, than if in the operation I had made uſe of quick-lime. Another illuſtration of what I was not long ſince ſaying, may be fetched from the experiment of making ſpirit of nitre, by mixing ſalt-petre with oil of vitriol, and diſtilling them together: for the oil does ſo divide or break the corpuſcles of the nitre, that the now diſpoſed particles of that ſalt, which amount to a great portion of the whole, will be made eaſily enough to aſcend, even with a moderate fire of ſand, and ſometimes without any fire at all, in the form of ſpirits, exceeding unquiet, ſubtle, and apt to ſmoke away.

To which inſtances of this imperfect kind of volatilization more might be added, but that you may well think, I have detained you but too long already with indigeſted notes about one quality.

CHAP. VII.

THE laſt means of volatilizing bodies is, the operation of the fire or ſome other actual heat: but of this, which is obvious, it would be ſuperfluous to diſcourſe. Only this I ſhall intimate, that there may be bodies, which, in ſuch degrees of fire, as are wont to be given in the vulgar operations of chemiſts, will not be elevated, which yet may be forced up by ſuch violent and laſting fires, as are employed by the melters of ores, and founders of guns, and ſometimes by glaſs-makers. And, on this conſideration, I ſhall here obſerve to you, ſince I did not do it at my entrance on theſe notes, that chemiſts are wont to ſpeak, and I have accordingly been led to treat of volatility and fixity, in a popular ſenſe of thoſe terms. For, if we would conſider the matter more ſtrictly, I preſume we ſhould find, that volatility and fixity are but relative qualities, which are to be eſtimated, eſpecially the former of them, by the degree of fire, to which the body, whereto we aſcribe one or other of thoſe qualities, is expoſed; and therefore it is much more difficult, than men are aware of, to determine accurately, when a body ought to be accounted volatile, and when not; ſince there is no determinate degree of heat agreed on, nor indeed eaſy to be deviſed, that may be as a ſtandard, whereby to meaſure volatility and fixedneſs: and it is obvious, that a body, that remains fixed in one degree of fire, may be forced up by another. To which may be added, agreeably to what I lately began to obſerve, that a body may paſs for abſolutely fixed among the generality of chemiſts, and yet be unable to perſevere in the fires of founders and glaſs-makers: which brings into my mind, that not having obſerved, that chemiſts have examined the fixity of other bodies, than metalline ones, by the cupel, I had the curioſity to put dry ſalt of tartar upon it, and found, as I expected, that, in no long time, it manifeſtly waſted in ſo vehement a heat, wherein alſo the air came freely at it, (though quick-lime, handled after the ſame way, loſt not of its weight; and having well mixed one ounce of good ſalt of tartar with treble its weight of tobacco-pipe clay, we kept them but for two, or, at moſt, three hours, in a ſtrong fire; yet, the crucible being purpoſely left uncovered, we found the ſalt of tartar ſo waſted, that the remaining mixture (which was not fluxed) afforded us not near a quarter of an ounce of ſalt. And indeed I ſcarce doubt, but that in ſtrictneſs divers of thoſe bodies, that paſs for abſolutely fixed, are but ſemi-fixed, or, at leaſt, but comparatively and relatively fixed, that is, in reference to ſuch degrees of fire, as they are wont to be expoſed to in diſtillations, ſublimations, &c. of chemiſts; not ſuch as are given in the raging fires of founders and glaſs-makers. And perhaps, even the fires of glaſs-makers, and ſay-maſters themſelves, are not the moſt intenſe, that may poſſibly be made in a ſhort time, provided there be but ſmall portions of matter to be wrought on by them. And, in effect, I know very few bodies, beſides gold, that will perſevere totally fixed in the vehementeſt degrees of fire, that trials have made me acquainted with. And I elſewhere tell you, that, though tin, in our chemical reverberatories themſelves, is wont to be reduced but into a calx, that is reputed very fixed; yet in thoſe intenſe fires, that a virtuoſo of my acquaintance uſes in his tin-mines, there is not ſeldom found quantities of tin carried up to a notable height in the form of a whitiſh powder, which, being in good maſſes, forced off from the places to which it had faſtened itſelf, does, by a ſkilful reduction, yield many a pound weight of good malleable metal, which ſeemed to me to be rather more, than leſs, fine than ordinary tin.

POSTSCRIPT,

Relating to page 612, and here annexed for their ſakes, who have a mind to repeat the experiment there delivered, that ſo they may know the quantities employed in it.

WITH two parts of this crocus, we ground very well three parts of ſal armoniac; and having ſublimed them in a ſtrong fire, we took off the high coloured ſublimate, and put in either an equal weight, or a weight exceeding it by half, to the caput mortuum, we found, after the ſecond ſublimation, which was alſo high coloured, that, of an ounce of crocus, we had raiſed ſix drams, that is, three quarters of the whole weight.

EXPERIMENTAL NOTES
OF THE
Mechanical ORIGIN or PRODUCTION
OF
FIXEDNESS.

CHAP. I.

FIXITY being the oppoſite quality to volatility, what we have diſcourſed about the latter, will make the nature of the former more eaſily underſtood, and upon that account allow me to make ſomewhat the quicker diſpatch of what I have to ſay of it.

THE qualifications, that conduce moſt to the fixity of a portion of matter, ſeem to be theſe.

FIRST, the groſſneſs, or the bulk of the corpuſcles it conſiſts of. For, if theſe be too big, they will be too unweildy, and unapt to be carried up into the air by the action of ſuch minute particles as thoſe of the fire, and will alſo be unfit to be buoyed up by the weight of the air; as we ſee, that vapours, whilſt they are ſuch, are ſmall enough to ſwim in the air, but can no longer be ſuſtained by it, when they convene into drops of rain, or flakes of ſnow. But here it is to be obſerved, that, when I ſpeak of the corpuſcles, that a fixed body conſiſts of, I mean not either its elementary or its hypoſtatical principles, as ſuch, but only thoſe very little maſſes or cluſters of particles, of what kind ſoever they be, that ſtick ſo firmly to one another, as not to be diviſible and diſſipable by that degree of fire, in which the body is ſaid to be fixed; ſo that each of thoſe little concretions, though it may itſelf be made up of two, three, or more particles of a ſimpler nature, is conſidered here *per modum unius*, or as one entire corpuſcle. And this is one qualification conducive to the fixedneſs of a body.

THE next is the ponderouſneſs, or ſolidity of the corpuſcles it is made up of. For if theſe be very ſolid, and (which ſolid and compact bodies uſually are) of a conſiderable ſpecifick gravity, they will be too heavy to be carried up by the effluvia or the action of the fire, and their ponderouſneſs will make them as unweildy, and indiſpoſed to be elevated by ſuch agents, as the groſſneſs of their bulk would make bigger corpuſcles, but of a proportionably inferior ſpecific weight. On which account, the calces of ſome metals and minerals, as gold, ſilver, &c. though, by the operation of ſolvents, or of the fire, or of both, reduced to powders exceedingly ſubtile, will reſiſt ſuch vehement fires, as will eaſily drive up bigger, but leſs heavy and compact corpuſcles, than thoſe calces conſiſt of.

THE third qualification, that conduces to the fixity of a body, belongs to its integral parts, not barely as they are ſeveral parts of it, but as they are aggregated or contexed into one body. For, the qualification, I mean, is the ineptitude of the component corpuſcles for avolation, by reaſon of their branchedneſs, irregular figures, crookedneſs, or other inconvenient ſhape, which entangles the particles among one another, and makes them difficult to be extracted; by which means, if one of them do aſcend, others, wherewith it is complicated, muſt aſcend with it; and, whatever be the account, on which divers particles ſtick firmly together, the aggregate will be too heavy or unweildy to be raiſed. Which I therefore take notice of, becauſe that, though uſually it is on the roughneſs and irregularity of corpuſcles, that their coheſion depends; yet it ſometimes happens, that the ſmoothneſs and flatneſs of their ſurfaces makes them ſo ſtick together, as to reſiſt a total divulſion; as may be illuſtrated by what I have ſaid of the coheſion of poliſhed marbles, and the plates of glaſs, and by the fixity of glaſs itſelf in the fire.

FROM this account of the cauſes or requiſites of fixity, may be deduced the following means of giving or adding fixation to a body, that was before either volatile, or leſs fixed. Theſe means may be reduced to two general heads; firſt, the action of the fire, as the parts of the body, expoſed to it are thereby made to operate variouſly on one another. And next, the aſſociation of the particles of a volatile body with thoſe of ſome proper additament: which term [of proper] I rather employ than that, one would expect [of fixed;] becauſe it will ere long appear, that, in certain caſes, ſome volatile bodies may more conduce to the fixation of other volatile bodies, than ſome fixed ones do. But theſe two inſtruments of fixation being but general, I ſhall propoſe four or five more particular ones.

CHAP. II.

AND firſt, in ſome caſes it may conduce to fixation, that, either by an additament, or by the operation of the fire, the parts of a body be brought to touch each other in large portions of their ſurfaces. For, that from ſuch a contact, there will follow ſuch a mutual coheſion, as will, at leaſt, indiſpoſe the touching corpuſcles to ſuffer a total divulſion, may appear probable from what we lately noted of the coheſion of pieces of marble and glaſs, and from ſome other phænomena belonging to the hiſtory of firmneſs, from which we may properly enough borrow ſome inſtances, at leaſt, for illuſtration, in the doctrine of fixedneſs, in regard, that uſually, though not always, the ſame things, that make a body firm, give it ſome degree of fixity, by keeping it from being diſſipated by the wonted degrees of heat, and agitation it meets with in the air. But, to return to the contact we were ſpeaking of, I think it not impoſſible, (though you may perhaps think it ſtrange) that the bare operation of the fire may, in ſome caſes, procure a coheſion among the particles, (and conſequently make them more fixed) as well as in others disjoin them, and thereby make them more volatile. For, as in ſome bodies, the figures and ſizes of the corpuſcles may be ſuch, that the action of the fire may rub or tear off the little beards or hooks, or other particles, that entangle them, and by that means make it more eaſy for the corpuſcles to be diſengaged and fly upwards; ſo, in other bodies, the ſize and ſhape of the corpuſcles may be ſuch, that the agitation, cauſed by the fire, may rub them one againſt the other, ſo as by mutual attrition to grind, as it were, their ſurfaces, and make them ſo broad and ſmooth, if not alſo ſo flat, as that the contact of the corpuſcles ſhall come to be made according to a large portion of their ſuperficies, from whence will naturally follow a firm coheſion. Which I ſhall illuſtrate by what we may obſerve among thoſe, that grind glaſſes for teleſcopes and miſcroſcopes. For, theſe artificers, by long rubbing a piece of glaſs againſt a metalline diſh, or concave veſſel, do, by this attrition, at length bring the two bodies to touch one another in ſo many parts of their congruous ſurfaces, that they will ſtick firmly to one another, ſo as ſometimes to oblige the workman to uſe violence to diſjoin them. And this inſtance (which is not the ſole I could alledge) may ſuffice to ſhew, how a coheſion of corpuſcles may be produced by the mutual adaptation of their congruous ſurfaces. And if two groſſer corpuſcles, or a greater number of ſmaller, be thus brought to ſtick together, you will eaſily believe, their aggregate will prove too heavy or unwieldy for avolation. And to ſhew, that the fire may affect a levigation in the ſurfaces of ſome corpuſcles, I have ſometimes cauſed minium, and ſome other calces, that I judged convenient, to be melted for a competent time, in a vehement fire conveniently adminiſtred; whereby, according to expectation, that, which was before a dull and incoherent powder, was reduced into much groſſer corpuſcles, multitudes of whoſe grains appeared ſmooth, glittering, and almoſt ſpecular, like thoſe of fine litharge of gold; and the maſſes, that theſe grains compoſed, were uſually ſolid enough, and of difficult fuſion. And when we make glaſs of lead *per ſe*, (which I elſewhere teach you how to do) it is plain, that the particles of the lead are reduced to a great ſmoothneſs; ſince, whereſoever you break the glaſs, the ſurfaces, produced at the crack, will not be jagged, but ſmooth, and conſiderably ſpecular. Nor do I think it impoſſible, that even, when the fire does not make any great attrition of the corpuſcles of the body to be fixed, it may yet occaſion their ſticking together, becauſe by long tumbling them up and down in various manners, it may at length, after multitudes of revolutions, and differing occurſions, bring thoſe of their ſurfaces together, which, by reaſon of their breadth, ſmoothneſs, or congruity of figure, are fit for mutual coheſion; and when once they come to ſtick, there is no neceſſity, that the ſame cauſes, that were able to make them paſs by one another, when their contact was but according to an inconſiderable part of their ſurfaces, ſhould have the ſame effect now, when their contact is full; though perhaps, if the degree of fire were much encreaſed, a more vehement agitation would ſurmount this coheſion, and diſſipate again theſe cluſters of coaleſcent corpuſcles.

THESE conjectures will, perhaps, appear leſs extravagant, if you conſider what happens in the preparation of quickſilver precipitated *per ſe*. For there running mercury, being put into a conveniently ſhaped glaſs, is expoſed to a moderate fire for a conſiderable time: (for I have ſometimes found ſix or ſeven weeks to be too ſhort a one.) In this degree of fire the parts are variouſly tumbled, and made many of them to aſcend, till convening into drops on the ſides of the glaſs, their weight carries them down again; but, at length, after many mutual occurſions, if not alſo attritions, ſome of the parts begin to ſtick together in the form of a red powder, and then more and more mercurial particles are faſtened to it, till at length all, or by much the greater part of the mercury, is reduced into the like præcipitate, which, by this coheſion of the parts, being grown more fixed, will not, with the ſame degree of heat, be made to riſe and circulate, as the mercury would before; and yet, as I elſewhere note, I have found, by trial, that, with a greater and competent degree of heat, this præcipitate *per ſe*, would, without the help of any volatilizing additament, be eaſily reduced into running mercury again. Chemiſts and phyſicians, who agree in ſuppoſing this precipitate to be made without any additament, will, perchance, ſcarce be able to give a more likely account of the conſiſtency and degree of fixity, that is obtained in the mercury; in which, ſince no body is added to it, there appears not to be wrought any but a mechanical change. And though, I confeſs, I have not been without ſuſpicions, that in philoſophical ſtrictneſs this præcipitate may not be made *per ſe*, but that ſome penetrating igneous particles, eſpecially ſaline, may have aſſociated themſelves with the mercurial corpuſcles; yet even upon this ſuppoſition it may be ſaid, that theſe particles contribute to the effect, that is produced, but by facilitating or procuring, by their opportune interpoſition, the mutual coheſion of corpuſcles, that would not otherwiſe ſtick to one another.

PERHAPS it will not be altogether impertinent to add on this occaſion, that, as for the generality of chemiſts, as well others, as Helmontians, that aſſert the tranſmutation of all metals into gold by the philoſopher's ſtone, methinks, they may grant it to be probable, that a new and fit contexture of the parts of a volatile body may, eſpecially by procuring a full contact among them, very much contribute to make it highly fixed. For to omit what is related by leſs credible authors, it is averred, upon his own trial, by *Helmont*, who pretended not to the elixir, that a grain of the powder, that was given him, tranſmuted a pound (if I miſremember not) of running mercury; where the proportion of the elixir to the mercury was ſo inconſiderable, that it cannot reaſonably be ſuppoſed, that every corpuſcle of the quick-ſilver, that before was volatile, was made extremely fixed, merely by its coalition with a particle of the powder, ſince, to make one grain ſuffice for this coalition, the parts it muſt be divided into muſt be ſcarce conceiveably minute, and therefore each ſingle part not likely to be fixed itſelf, or at leaſt more likely to be carried up by the vehemently agitated mercury, than to reſtrain that from avolation; whereas, if we ſuppoſe the elixir to have made ſuch a commotion among the corpuſcles of the mercury, as (having made them perhaps ſomewhat change their figure, and expelled ſome inconvenient particles,) to bring them to ſtick to one another, according to very great portions of their ſurfaces, and entangle one another, it will not be diſagreeable to the mechanical doctrine of fixity, that the mercury ſhould endure the fire as well as gold, on the ſcore of its new texture, which, ſuppoſing the ſtory true, appears to have been introduced, by the new colour, ſpecifick gravity, indiſſolubleneſs in aqua fortis, and other qualities, wherein gold differs from mercury, eſpecially malleableneſs, which, according to our notes about that quality uſually requires, that the parts, from whoſe union it reſults, be either hooked, branched, or otherwiſe adapted and fitted to make them take faſt hold of one another, or ſtick cloſe to one another. And ſince, in the whole maſs of the factitious gold, all, ſave one grain, muſt be materially the ſame body, which, before the projection was made, was quick-ſilver, we may ſee, how great a proportion of volatile matter may, by an inconſiderable quantity of fixing additament, acquire ſuch a new diſpoſition of its parts, as to become moſt fixed. And however, this inſtance will agree much better with the mechanical doctrine about fixity, than with that vulgar opinion of the chemiſts, (wherewith it will not at all comply,) that if, in a mixture, the volatile part do much exceed the fixed, it will carry up that, or at leaſt, a good portion thereof, with it; and on the contrary. But though this rule holds in many caſes, where there is no peculiar indiſpoſition to the effect, that is aimed at; yet if the mechanical affections of the bodies be ill ſuited to ſuch a purpoſe, our philoſophical experiment manifeſtly proves, that the rule will not hold, ſince ſo great a multitude of grains of mercury, inſtead of carrying up with them one grain of the elixir, are detained by it in the ſtrongeſt fire. And thus much for the firſt way of fixing volatile bodies.

CHAP. III.

THE ſecond way of producing fixity is by expelling, breaking, or otherwiſe diſabling thoſe volatile corpuſcles, that are too indiſpoſed to be fixed themſelves, or are fitted to carry up with them ſuch particles, as would not, without their help, aſcend. That the expulſion of ſuch parts is a proper means to make the aggregate of thoſe, that remain more fixed, I preſume you will not put me ſolicitouſly to prove; and we have a manifeſt inſtance of it in ſoot, where, though many active parts were by the violence of the fire and current of the air carried up together by the more volatile parts; yet, when ſoot is well diſtilled in a retort, a competent time being given for the extricating and avolation of the other parts, there will at the bottom remain a ſubſtance, that will not now fly away, as it formerly did. And here let me obſerve, that the receſs of the fugitive corpuſcles may contribute to the fixation of a body, not barely becauſe the remaining matter is freed from ſo many unfixed, if not alſo volatilizing parts; but, as it may often happen, that upon their receſs the pores or intervals, they left behind them, are filled up with more ſolid or heavy matter, and the body becomes, as more homogeneous, ſo more cloſe and compact. And whereas I intimated, that, beſides the expulſion of unfit corpucles, they may be otherwiſe diſabled from hindering the fixation of the maſs they belong to, I did it, becauſe it ſeems very poſſible, that in ſome caſes they may, by the action of the fire, be ſo broken, as with their fragments to fill up the pores or intervals of the body they appertained to; or may make ſuch coalitions with the particles of a convenient additament, as to be no impediment to the fixity of the whole maſs, though they remain in it. Which poſſibly you will think may well happen, when you ſhall have peruſed the inſtances annexed to the fourth way of fixing bodies.

The third means of fixing, or leſſening the volatility of bodies, is by preſerving that reſt among the parts, whoſe contrary is neceſſary to their volatilization. And this may be done by preventing or checking that heat, or other motion, which external agents ſtrive to introduce into the parts of the propoſed body. But this means tending rather to hinder the actual avolation of a portion of matter, or, at moſt, procure a temporary abatement of its volatility, than to give it a ſtable fixity, I ſhall not any longer inſiſt on it.

The fourth way of producing fixity in a body, is by putting to it ſuch an appropriated additament, whether fixed or volatile, that the corpuſcles of the body may be put among themſelves, or with thoſe of the additament, into a complicated ſtate, or entangled contexture. This being the uſual and principal way of producing fixity, we ſhall dwell ſomewhat the longer upon it, and give inſtances of ſeveral degrees of fixation. For, though they do not produce that quality in the ſtricteſt acceptation of the word, fixity; yet it is uſeful in our preſent enquiry, to take notice, by what means that volatility comes to be gradually abated, ſince that may facilitate our underſtanding, how the volatility of a body comes to be totally abated, and conſequently the body to be fixed.

CHAP. IV.

AND firſt, we find, that a fixed additament, if its parts be conveniently ſhaped, may eaſily give a degree of fixity to a very volatile body. Thus ſpirit of nitre, that will of itſelf eaſily enough fly away in the air, having its ſaline particles aſſociated with thoſe of fixed nitre, or fals of tartar, will with the alcali compoſe a ſalt of a nitrous nature, which will endure to be melted in a crucible, without being deprived even of its ſpirits. And I have found, that the ſpirits of nitre, that abound in aqua-fortis, being concoagulated with the ſilver they corrode, though one would not expect, that ſuch ſubtile corpuſcles ſhould ſtick faſt to ſo compact and ſolid a body as ſilver; yet cryſtals produced by their coalition, being put into a retort, may be kept a pretty while in fuſion, before the metal will let go the nitrous ſpirits. When we poured oil of vitriol upon the calx of vitriol, though many phlegmatick and other ſulphureous particles were driven away by the excited heat; yet the ſaline parts, that combined with the fixed ones of the colcothar, ſtuck faſt enough to them, not to be eaſily driven away. And if oil of vitriol be in a due proportion dropped upon ſalt of tartar, there reſults a *tartarum vitriolatum*, wherein the acid and alkalizate parts cohere ſo ſtrongly, that it is not an ordinary degree of fire will be able to disjoin them. Inſomuch, that divers chemiſts have (though very erroneouſly) thought this compounded ſalt to be indeſtructible. But a leſs heavy liquor than the ponderous oil of vitriol may, by an alkali, be more ſtrongly detained, than that oil itſelf; experience having aſſured me, that ſpirit of ſalt being dropped to ſatiety upon a fixed alkali, (I uſed either that of nitre or of tartar,) there would be made ſo ſtrict an union, that, having, without additaments, diſtilled the reſulting ſalt with a ſtrong and laſting fire, it appeared not at all conſiderably to be wrought upon, and was not ſo much as melted.

But it is not the bare mixture or commiſtion of volatile particles with fixed ones, (yea though the former be predominant in quantity,) that will ſuffice to elevate the latter. For, unleſs the figures of the latter be congruous and fitted to faſten to the other, the volatile parts will fly away in the heat, and leave the reſt as fixed as before: as when ſand or aſhes are wetted or drenched with water, they quickly part with that water, without parting with any degree of their fixity. But on the other ſide, it is not always neceſſary, that the body, which is fitted to deſtroy, or much abate the volatility of another ſubſtance, ſhould be itſelf fixed. For, if there be a ſkilful or lucky coaptation of the figures of the particles of both the bodies, theſe particles may take ſuch hold of one another, as to compoſe corpuſcles, that will neither by reaſon of their ſtrict union be divided by heat; nor by reaſon of their reſulting groſſneſs be elevated even by a ſtrong fire, or at leaſt by ſuch a degree of heat, as would have ſufficed to raiſe more indiſpoſed bodies than either of the ſeparate ingredients of the mixture. This obſervation, if duly made out, does ſo much favour our doctrine about the mechanical origin of fixation, and may be of ſuch uſe, not only to chemiſts, in ſome of their operations, but to philoſophers, in aſſigning the cauſes of divers phænomena of nature, that it may be worth while to exemplify it by ſome inſtances.

The firſt whereof I ſhall take from an uſual practice of the chemiſts themſelves: which I the rather do, to let you ſee, that ſuch known experiments are too often over-looked by them, that make them, but yet may hint or confirm theories to thoſe, that reflect on them. The inſtance, I here ſpeak of, is that, which is afforded by the vulgar preparation of bezoardicum minerale. For, though the rectified butter, or oil of antimony, and the ſpirit of nitre, that are put together to make this white præcipitate, are both of them diſtilled liquors; yet the copious powder, that reſults from their union, is, by that union of volatile parts, ſo far fixed, that, after they have edulcorated it with water, they preſcribe the calcining of it in a crucible for five or ſix hours: which operation it could not bear, unleſs it had attained to a conſiderable fixation. This diſcourſe ſuppoſes with the generality of chemiſts, that the addition of a due quantity of ſpirit of nitre is neceſſary to be employed in making the bezoardicum minerale. But if it be a true obſervation, which is attributed to the learned *Guntherus Billichius*, (but which I had no furnace at hand to examine, when I heard of it,) if, I ſay, it be true, that a bezoardicum minerale may be obtained, without ſpirit of nitre, barely by a ſlow evaporation, made in a glaſs-diſh, of the more fugitive parts of the oil of antimony; this inſtance will not indeed be proper in this place, but yet will belong to the ſecond of the foregoing ways of introducing fixity. I proceed now to alledge other particulars, in favour of the above-mentioned obſervation.

IF you take ſtrong ſpirit of ſalt, that, when the glaſs is unſtopped, will ſmoke of itſelf in the cold air, and ſatiate it with the volatile ſpirit of urine, the ſuperfluous moiſture being abſtracted, you will obtain by this preparation (which, you may remember, I long ſince communicated to you, and divers other virtuoſi,) a compounded ſalt, ſcarce, if at all, diſtinguiſhable from ſal armoniac, and which will not, as the ſalts it conſiſts of will do, before their coalition, eaſily fly up of itſelf into the air, but will require a not deſpicable degree of fire to ſublime it.

OF theſe ſemivolatile compoſitions of ſalt I have made, and elſewhere mentioned, others, which I ſhall not here repeat, but paſs on to other inſtances, pertinent to our preſent deſign.

I lately mentioned, that the volatility of the ſpirits of nitre may be very much abated, by bringing them to coagulate into cryſtals, with particles of corroded ſilver; but I ſhall now add, that I gueſſed, and by trial found, that theſe nitrous ſpirits may be made much more fixed by the addition of the ſpirit of ſalt, which, if it be good, will of itſelf ſmoke in the air. For, having diſſolved a convenient quantity of cryſtals of ſilver in diſtilled water, and precipitated them, not with a ſolution of ſalt, but the ſpirit of ſalt; the phlegm being abſtracted, and ſome few of the looſer ſaline particles; though the remaining maſs were preſſed with a violent fire, that kept the retort red-hot for a good while; yet the nitrous and ſaline ſpirits would by no means be driven away from the ſilver, but continued in fuſion with it; and when the maſs was taken out, theſe ſpirits did ſo abound in it, that it had no appearance of a metal, but looked rather like a thick piece of horn.

THE next inſtance I ſhall name is afforded us by that kind of turbith, which may be made by oil of vitriol, inſtead of the aqua-fortis employed in the common turpethum minerale. For, though oil of vitriol be a diſtilled liquor, and mercury a body volatile enough; yet, when we abſtracted four or five parts of oil of vitriol from one of quick-ſilver, (eſpecially if the operation were repeated,) and then waſhed off as much as we could of the ſaline particles of the oil of vitriol; yet thoſe, that, remained adhering to the mercury made it far more fixed, than either of the liquors had been before, and enabled it even in a crucible, to endure ſuch a degree of fire, before it could be driven away, as, I confeſs, I ſomewhat wondered at. The like turbith may be made with oil of ſulphur *per campanam*. But this is nothing to what *Helmont* tells us of the operation of his alkaheſt, where he affirms, that that menſtruum, which is volatile enough, being abſtracted from running mercury, not only coagulates it, but leaves it fixed, ſo that it will endure the brunt of fires actuated by bellows, (*omnem follium ignem.*) If this be certain, it will not be a ſlender proof, that fixity may be mechanically produced; and however, the argument will be good in reference to the Helmontian Spagyriſts. For if, as one would expect, there do remain ſome particles of the menſtruum with thoſe of the metal, it will not be denied, that two volatile ſubſtances may perfectly fix one another. And if, as *Helmont* ſeems to think, the menſtruum be totally abſtracted, this ſuppoſition will the more favour our doctrine about fixity; ſince, if there be no material additament left with the quick-ſilver, the fixation cannot ſo reaſonably be aſcribed to any thing, as to ſome new mechanical modification, and particularly to ſome change of texture introduced into the mercury itſelf.

AND that you may think this the leſs improbable, I will now proceed to ſome inſtances, whereof the firſt ſhall be this; that, having put a mixture made of a certain proportion of two dry, as well as volatile bodies, (viz. ſal armoniac, and flower or very fine powder of ſulphur,) to half its weight of common running mercury, and elevated this mixture three or four times from it, (in a conveniently ſhaped, and not over-wide glaſs) the mercury, that lay in the bottom, in the form of a ponderous and ſomewhat purpliſh powder, was, by this operation, ſo fixed, that it long endured a ſtrong fire, which at length was made ſo ſtrong, that it melted the glaſs, and kept it melted, without being ſtrong enough to force up the mercury: which, by ſome trials, not ſo proper to be here mentioned, ſeemed to have its ſalivating and emetick powers extraordinarily infringed, and ſometimes quite ſuppreſſed. But this only upon the bye. In all the other inſtances, (wherewith I ſhall conclude theſe notes,) I ſhall employ one menſtruum, oil of vitriol, and ſhew you the efficacy of it in fixing ſome parts of volatile bodies with ſome parts of itſelf; by which examples it may appear, that a volatile body may not only leſſen the volatility of another body, as in the lately mentioned caſe of our ſpirituous ſal armoniac; but that two ſubſtances, that apart were volatile, may compoſe a third, that will not only be leſs volatile, but conſiderably, if not altogether, fixed

WE mixed then, by degrees, about equal parts of oil of vitriol and oil of turpentine: and though each of them ſingle, eſpecially the latter, will aſcend with a moderate fire in a ſand-furnace; yet, after the diſtillation was ended, we had a conſiderable quantity, ſometimes, if I miſremember not, a fifth or ſixth part, of a caput mortuum, black as a coal, and whereof a great part was of a ſcarce to be expected fixedneſs in the fire.

TO give a higher proof of the diſpoſition, that oil of vitriol has to let ſome of its parts grow fixed by combination with thoſe of an exceeding volatile additament, I mixed this liquor with an equal or double weight of highly rectified ſpirit of wine, and not only after, but ſometimes without, previous digeſtion, I found, that the fluid parts of the mixture being totally abſtracted, there would remain a pretty quantity of a black ſubſtance ſo fixed, as to afford juſt cauſe of wonder.

And becauſe camphire is eſteemed the moſt fugitive of conſiſtent bodies, in regard that, being but laid in the free air, without any help of the fire, it will fly all away; I tried, what oil of vitriol, abſtracted from camphire, would do; and found at the bottom of the retort a greater quantity, than one would expect, of a ſubſtance as black as pitch, and almoſt as far from the volatility, as from the colour of camphire, though it appeared not, that any of the gum had ſublimed into the neck of the retort.

From all which inſtances it ſeems manifeſtly enough to follow, that in many caſes there needs nothing to make aſſociated particles, whether volatile or not, become fixed, but either to implicate or entangle them among themſelves, or bring them to touch one another, according to large portions of their ſurfaces, or by both theſe ways conjointly, or by ſome others, to procure the firm cohæſion of ſo many particles, that the reſulting corpuſcles be too big or heavy to be, by the degree of fire, wherein they are ſaid to be fixed, driven up into the air.

EXPERIMENTS
AND
NOTES,
ABOUT THE
MECHANICAL ORIGIN or PRODUCTION
OF
CORROSIVENESS
AND
CORROSIBILITY.

SECTION I.

About the Mechanical Origin of CORROSIVENESS.

I DO not, in the following notes, treat of corroſiveneſeſs in their ſtrict ſenſe of the word, who aſcribe this quality only to liquors, that are notably acid or ſour, ſuch as aqua fortis, ſpirir of ſalt, vinegar, juice of lemons, &c. but, that I may not be obliged to overlook urinous, oleous, and divers other ſolvents, or to coin new names for their differing ſolutive powers, I preſume to employ corroſiveneſs in a greater latitude, ſo as to make it almoſt equivalent to the ſolutive power of liquors, referring other menſtruums to thoſe, that are corroſive or fretting, (though not always as to the moſt proper, yet) as to the principal and beſt known ſpecies; which I the leſs ſcruple here to do, becauſe I have * elſewhere more diſtinctly enumerated and ſorted the ſolvents of bodies.

The attributes, that ſeem the moſt proper to qualify a liquor to be corroſive, are all of them mechanical, being ſuch as are theſe, that follow:

First, that the menſtruum conſiſt of, or abound with corpuſcles not too big to get in at the pores or commiſſures of the body to be diſſolved; nor yet be ſo very minute, as to paſs through them, as the beams of light do through glaſs; or to be unable, by reaſon of their great ſlenderneſs and flexibility, to disjoin the parts they invade.

Secondly, that theſe corpuſcles be of a ſhape fitting them to inſinuate themſelves, more or leſs, into the pores or commiſſures above-mentioned, in order to the diſſociating of the ſolid parts.

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* This refers to an eſſay of the author's about the uſefulneſs of chemiſtry to, &c.

THIRDLY, that they have a competent degree of ſolidity to disjoin the particles of the body to be diſſolved; which ſolidity of ſolvent corpuſcles is ſomewhat diſtinct from their bulk, mentioned in the firſt qualification; as may appear, by comparing a ſtalk of wheat and a metalline wire of the ſame diameter, or a flexible wand of oſier, of the bigneſs of one's little finger, with a rigid rod of iron of the ſame length and thickneſs.

FOURTHLY, that the corpuſcles of the menſtruum be agile and advantaged for motion, (ſuch as is fit to disjoin the parts of the invaded body) either by their ſhape, or their minuteneſs, or their fitneſs to have their action befriended by adjuvant cauſes; ſuch as may be (firſt) the preſſure of the atmoſphere, which may impel them into the pores of bodies not filled with a ſubſtance ſo reſiſting as common air: as we ſee, that water will, by the prevalent preſſure of the ambient, whether air or water, be raiſed to the height of ſome inches in capillary glaſſes, and in the pores of ſpunges, whoſe conſiſtent parts, being of eaſier ceſſion than the ſides of glaſs-pipes, thoſe pores will be enlarged, and conſequently thoſe ſides disjoined, as appears by the dilatation and ſwelling of the ſpunge: and (ſecondly) the agitation, that the intruding corpuſcles may be fitted to receive in thoſe pores or commiſſures, by the tranſcurſion of ſome ſubtile aetherial matter; or by the numerous knocks and other pulſes of the ſwimming or tumbling corpuſcles of the menſtruum itſelf, (which, being a fluid body, muſt have its ſmall parts perpetually and variouſly moved) whereby the engaged corpuſcles, like ſo many little wedges and leavers, may be enabled to wrench open, or force aſunder the little parts between which they have inſinuated themſelves. But I ſhall not here proſecute this theory, (which, to be handled fully, would require a diſcourſe apart) ſince theſe conjectures are propoſed but to make it probable in the general, that the corroſiveneſs of bodies may be deduced from mechanical principles: but whether beſt from the newly propoſed ones, or any other, need not be anxiouſly conſidered in theſe notes, where the things mainly intended and relied on are the experiments and phænomena themſelves.

EXPERIMENT I.

IT is obvious, that, though the recently expreſſed juice of grapes be ſweet, whilſt it retains the texture, that belongs to it, as it is new, (eſpecially, if it be made of ſome ſorts of grapes, that grow in hot regions;) yet, after fermentation, it will, in tract of time, as it were ſpontaneouſly, degenerate into vinegar. In which liquor, to a multitude of the more ſolid corpuſcles of the muſt, their frequent and mutual attritions may be ſuppoſed to have given edges like thoſe of the blades of ſwords or knives; and in which, perhaps, the confuſed agitation, that preceded, extricated, or, as it were, unſheathed ſome acid particles, that (derived from the ſap of the vine, or, perchance, more originally from the juice of the earth,) were at firſt in the muſt, but lay concealed, and, as it were, ſheathed among the other particles, wherewith they were aſſociated, when they were preſſed out of the grapes. Now this liquor, that by the forementioned, or other like mechanical changes, is become vinegar, does ſo abound with corpuſcles, which, on the account of their edges, or their otherwiſe ſharp and penetrative ſhape, are acid and corroſive, that the better ſort of it will, without any preparation, diſſolve coral, crab's-eyes, and even ſome ſtones, lapis ſtellaris in particular, as alſo minium, or the calx of lead, and even crude copper, as we have often tried. And not only the diſtilled ſpirit of it will do thoſe things more powerfully, and perform ſome other things, that mere vinegar cannot; but the ſaline particles, wont to remain after diſtillation, may, by being diſtilled and cohobated *per ſe*, or by being ſkilfully united with the foregoing ſpirit, be brought to a menſtruum of no ſmall efficacy in the diſſolution, and other preparations of metalline bodies, too compact for the mere ſpirit itſelf to work upon.

FROM divers other ſweet things alſo may vinegar be made; and even of honey, ſkilfully fermented with a ſmall proportion of common water, may be made a vinegar ſtronger than many of the common wine-vinegars; as has been affirmed to me by a very candid phyſician, who had occaſion to deal much in liquors.

EXPERIMENT II.

NOT only ſeveral dry woods, and other bodies, that moſt of them paſs for inſipid, but honey and ſugar themſelves afford by diſtillation acid ſpirits, that will diſſolve coral, pearls, &c. and will alſo corrode ſome metals and metalline bodies themſelves; as I have often found by trial. So that the violent operation of the fire, that deſtroys what they call the form of the diſtilled body, and works, as a mechanical agent, by agitating, breaking, diſſipating, and under a new conſtitution reaſſembling the parts, procures for the diſtiller an acid corroſive menſtruum; which, whether it be brought to paſs by making the corpuſcles rub one another into the figure of little ſharp blades, or by ſplitting ſome ſolid parts into ſharp or cutting corpuſcles, or by unſheathing, as it were, ſome parts, that, during the former texture of the body, did not appear to be acid; or whether it be rather effected by ſome other mechanical way, may in due time be further conſidered.

EXPERIMENT III.

IT is obſerved by refiners, goldſmiths and chemiſts, that aqua fortis and aqua regia, which are corroſive menſtruums, diſſolve metals, the former of them ſilver, and the latter gold, much more ſpeedily and copiouſly, when an external heat gives their inteſtine motions a new degree of vehemency or velocity, which is but a mechanical thing; and yet this ſuperadded meaſure of agitation is not only in the abovementioned inſtances a powerfully aſſiſtant cauſe in the ſolutions made by the lately mentioned corroſive liquors, but is that, without which ſome menſtruums are not wont ſenſibly to corrode ſome bodies at all, as we have tried in keeping quick-ſilver in three or four times its weight of oil of vitriol; ſince in this menſtruum I found not the mercury to be diſſolved, or corroded, though I kept it a long time in the cold: whereas, when the oil of vitriol was excited by a convenient heat, (which was not faint) it corroded the mercury into a fine white calx or powder, which by the affuſion of fair water, would be preſently turned into a yellowiſh calx of the colour and nature of a turbith. I remember alſo, that having, for trial's ſake, diſſolved in a weak ſpirit of ſalt a fourth part of weight of fine cryſtals of nitre, we found, that it would not in the cold (at leaſt during a good while, that we waited for its operation) diſſolve leaf-gold; but when the menſtruum was a little heated at the fire, the ſolution proceeded readily enough. And in ſome caſes, though the external heat be but ſmall, yet there may intervene a briſk heat, and much cooperate in the diſſolution of a body; as for inſtance, of quick-ſilver in aqua fortis. For it is no prodigy to find, that when a full proportion of that fluid metal has been taken, the ſolution, though at firſt altogether liquid, and as to ſenſe uniform, comes to have, after a while, a good quantity of coagulated or cryſtallized matter at the bottom, of which the cauſe may be, that in the very act of corroſion there is excited an intenſe degree of heat, which conferring a new degree of agitation to the menſtruum, makes it diſſolve a good deal more, than afterwards, when the conflict is over, it is able to keep up.

EXPERIMENT IV.

WE have obſerved alſo, that agitation does in ſome caſes ſo much promote the diſſolutive power of ſaline bodies, that though they be not reduced to that ſubtilty of parts, to which a ſtrong diſtillation brings them; yet they may in their groſſer and cruder form have the power to work on metals; as I elſewhere ſhew, that by barely boiling ſome ſolutions of ſalts of a convenient ſtructure, as nitre, ſal armoniac, &c. with foliated gold, ſilver, &c. we have corroded theſe metals, and can diſſolve ſome others. And by boiling crude copper (in filings) with ſublimate and common water, we were able, in no long time, to make a ſolution of the metal.

EXPERIMENT V.

SOMETIMES alſo, ſo languid an agitation, as that, which ſeems but ſufficient to keep a liquor in the ſtate of fluidity, may ſuffice to give ſome dry bodies a corroding power, which they could not otherwiſe exerciſe; as in the way of writing one's name (or a motto) upon the blade of a knife with common ſublimate: for, if having very thinly overlaid which ſide you pleaſe with bees-wax, you write with a bodkin, or ſome pointed thing upon it; the wax being thereby removed from the ſtrokes made by the ſharp body, it is eaſy to etch with ſublimate; ſince you need but ſtrew the powder of it upon the place bared of the wax, and wet it well with mere common water; for ſtrong vinegar is not neceſſary. For, after a while, all the parts of the blade, that ſhould not be fretted, being protected by the caſe or film of wax, the ſublimate will corrode only where way has been made for it by the bodkin, and the letters will be more or leſs deeply engraven (or rather etched) according to the time the ſublimate is ſuffered to lie on. And if you aim only at a legible impreſſion, a few minutes of an hour (as four or five) may ſerve the turn.

EXPERIMENT VI.

THIS brings into my mind an obſervation I have ſometimes had occaſion to make, that I found more uſeful than common; and it is, that divers bodies, whether diſtilled or not diſtilled, that are not thought capable of diſſolving other bodies, becauſe in moderate degrees of heat they will not work on them, may yet, by intenſe degrees of heat, be brought to be fit ſolvents for them. To which purpoſe I remember, that having a diſtilled liquor, which was rather ſweet to the taſte, than either acid, lixiviate, or urinous, though for that reaſon it ſeemed unfit to work on pearls, and accordingly did not diſſolve them in a conſiderable time, wherein they were kept with it in a more than ordinarily warm digeſtion; yet the glaſs being for many hours (amounting perhaps to ſome days) kept in ſuch an heat of ſand as made the liquor boil, we had a diſſolution of pearls, that uniting with the menſtruum, made it a very valuable liquor. And though the ſolvents of crude gold, wont to be employed by chemiſts, are generally diſtilled liquors, that are acid, and in the lately mentioned ſolvent, made of crude ſalts and common water, acidity ſeemed to be the predominant quality (which makes the uſe of ſolutions made in aqua regia, &c. ſuſpected by many phyſicians and chemiſts;) yet fitly choſen alcalizate bodies themſelves, as repugnant as they uſe to be to acids, without the help of any liquor, will be enabled, by a melting fire, in no long time, to penetrate and tear aſunder the parts even of crude gold; ſo that it may afterwards be eaſily taken up in liquors, that are not acid, or even by water itſelf.

EXPERIMENT VII.

THE tract about ſalt-petre, that gave occaſion to theſe annotations, may furniſh us with an eminent inſtance of the production of ſolvents. For, though pure ſalt-petre itſelf, when diſſolved in water, is not obſerved to be a menſtruum for the ſolution of the metals hereafter to be named, or ſo much as of coral itſelf; yet when, by a convenient diſtillation, its parts are ſplit, if I may ſo ſpeak, and by attrition, or other mechanical ways of working on them, reduced to the ſhapes of acid and alcalizate ſalts, it then affords two ſorts of menſtruums, of very differing natures, which, betwixt them, diſſolve or corrode a great number and variety of bodies; as the ſpirit of nitre, without addition, is a ſolvent for moſt metals, as ſilver, mercury, copper, lead, &c. and alſo divers mineral bodies, as tin-glaſs, ſpelter, lapis calaminaris, &c. and the fixed ſalt of nitre operates upon ſulphureous minerals, as common ſulphur, antimony, and divers other bodies, of which I elſewhere make mention.

EXPERIMENT VIII.

BY the former trials it has appeared, that the encreaſe of motion, in the more penetrating corpuſcles of a liquor, contributes much to its ſolutive power; and I ſhall now add, that the ſhape and ſize, which are mechanical affections, and ſometimes alſo the ſolidity of the ſame corpuſcles does eminently concur to qualify a liquor to diſſolve this or that particular body. Of this, even ſome of the more familiar practices of chemiſts may ſupply us with inſtances. For there is no account ſo probable as may be given upon this ſuppoſition, why aqua fortis, which will diſſolve ſilver, without meddling with gold, ſhould, by the addition of a fourth part of its weight of ſal armoniac, be turned into aqua regia, which, without meddling with ſilver, will diſſolve gold. But there is no neceſſity of having recourſe to ſo groſs and compounded a body as ſal armoniac, to enable aqua fortis to diſſolve gold: for, the ſpirit of common ſalt alone, being mingled in a due proportion, will ſuffice for that purpoſe. Which (by the way) ſhews, that the volatile ſalt of urine and ſoot, that concur to the making up of ſal armoniac, are not neceſſary to the diſſolution of gold, for which a ſolvent may be made with aqua fortis and crude ſea ſalt. I might add, that the mechanical affections of a menſtruum, may have ſuch an intereſt in its diſſolutive power, that even mineral or metalline corpuſcles may become uſeful ingredients of it, though, perhaps, it be a diſtilled liquor; as might be illuſtrated by the operations of ſome compounded ſolvents, ſuch as is the oil of antimony made by repeated rectifications of what chemiſts call its butter, which, whatever ſome ſay to the contrary, does much abound in antimonial ſubſtance.

EXPERIMENT IX.

BUT I ſhall return to our aqua regia, becauſe the mention I had occaſion to make of that ſolvent, brought into my mind what I deviſed, to make it probable, that a ſmaller change, than one would lightly imagine, of the bulk, ſhape, or ſolidity of the corpuſcles of a menſtruum, may make it fit to diſſolve a body it would not work on before. And this I the rather attempted, becauſe the warier ſort of chemiſts themſelves are very ſhy of the inward uſe or preparations made of gold by the help of aqua fortis, becauſe of the odious ſtink they find, and the venenoſity they ſuſpect in that corroſive menſtruum: whereas ſpirit of ſalt we look upon as a much more innocent liquor, whereof, if it be but diluted with fair water, or any ordinary drink, a good doſe may be ſafely given inwardly, though it have not wrought upon gold, or any other body, to take off its acrimony. But, whether or no this prove of any great uſe in phyſick, wherein, perhaps, if any quantity of gold be to be diſſolved, a greater proportion of ſpirit of nitre would be needed; the ſucceſs will not be unfit to be mentioned, in reference to what we were ſaying of ſolvents. For, whereas we find not, that our ſpirit of ſalt here, in *England*, will at all diſſolve crude gold, we found, that by putting ſome leaf-gold into a convenient quantity of good ſpirit of ſalt, when we had dropped in ſpirit of nitre, (ſhaking the glaſs at each drop) till we perceived, that the mixture was juſt able, in a moderate heat, to diſſolve the gold, we found, that we had been obliged to employ but after the rate of twelve drops of the latter liquor to an ounce of the former; ſo that, ſuppoſing each of theſe drops to weigh a grain, the fortieth part of ſpirit of nitre being added, ſerved to turn the ſpirit of ſalt into a kind of aqua regia. But to know the proportion otherwiſe than by gueſs, we weighed ſix other drops of the ſame ſpirit of ſalt, and found them to amount not fully to three grains and an half: whence it appeared, that we added but about a ſeventieth part of the nitrous ſpirit to that of ſalt.

THE experiments, that have been hitherto recited, relate chiefly to the production of corroſive menſtruums; and therefore I ſhall now add an account of a couple of trials, that I made manifeſtly to leſſen, or quite to deſtroy corroſiveneſs in liquors very conſpicuous for that quality.

EXPERIMENT X.

WHEREAS one of the moſt corroſive menſtruums, that is yet known, is oil of vitriol, which will fret in pieces both divers metals and minerals, and a great number and variety of animal and vegetable bodies; yet if you digeſt with it, for a while only, an equal weight of highly-rectified ſpirit of wine, and afterwards diſtil the mixture very warily, (for elſe the experiment may very eaſily miſcarry,) you may obtain a pretty deal of liquor not corroſive at all, and the remaining ſubſtance will be reduced partly into a liquor, which, though acid, is not more ſo than one part of good oil of vitriol will make ten times as much common water, by being well mingled with it; and partly into a dry ſubſtance, that has ſcarce any taſte at all, much leſs a corroſive one.

EXPERIMENT XI.

AND though good aqua fortis be the moſt generally employed of corroſive menſtruums, as being capable of diſſolving or corroding, not only many minerals, as tin-glaſs, antimony, zink, &c. but all metals, except gold, (for, though it make not a permanent ſolution of crude tin, it quickly frets the parts aſunder, and reduces it to an immalleable ſubſtance;) yet, to ſhew how much the power of corroding may be taken away, by changing the mechanical texture of a menſtruum, even without ſeeming to deſtroy the fretting ſalts, I practiſed (and communicated to divers virtuoſi) the following experiment, elſewhere mentioned to other purpoſes.

We took equal parts of good aqua fortis, and highly dephlegmed ſpirit of wine, and having mingled them warily, and by degrees, (without which caution the operation may prove dangerous) we united them by two or three diſtillations of the whole mixture; which afterwards we found not to have the leaſt fretting taſte, and to be ſo deprived of its corroſive nature, that it would not work upon ſilver, though by precipitation, or otherwiſe, reduced to very ſmall parts; nay, it would ſcarce ſenſibly work, in a good while, on filings of copper, or upon other bodies, which mere vinegar, or, perhaps, rheniſh wine, will corrode. Nay, I remember, that, with another ſpirit, (that was not urinous) and afterwards, with alkohol of wine, we ſhewed a more ſurpriſing ſpecimen of the power of either deſtroying, or debilitating, the corroſiveneſs of a menſtruum, and checking its operation. For, having cauſed a piece of copper plate to be put into one ounce of aqua fortis, when this liquor was eagerly working upon the metal, I cauſed an ounce of the alkohol of wine, or the other ſpirit to be poured, (which it ſhould warily be) upon the agitated mixture; whoſe efferveſcence, at the firſt inſtant, ſeemed to be much encreaſed, but preſently after was checked, and the corroſivenſs of the menſtruum being ſpeedily diſabled or corrected, the remaining copper was left undiſſolved at the bottom.

Nor are theſe the only acid menſtruums, that I have, many years ſince, been able to correct by ſuch a way: for I applied it to others, as ſpirit of nitre, and even aqua regis itſelf; but it has not an equal operation upon all, and leaſt of all (as far as I can remember) upon ſpirit of ſalt; as, on the other ſide, ſtrong ſpirit of nitre was the menſtruum, upon which its effects were the moſt ſatisfactory.

Most of the chemiſts pretend, that the ſolutions of bodies are performed by a certain cognation and ſympathy between the menſtruum and the body it is to work upon. And it is not to be denied, that, in divers inſtances, there is, as it were, a conſanguinity between the menſtruum and the body to be diſſolved; as when ſulphur is diſſolved by oils, whether expreſſed or diſtilled: but yet, as the opinion is generally propoſed, I cannot acquieſce in it, partly, becauſe there are divers ſolutions and other phænomena, where it will not take place; and partly, becauſe, even in thoſe inſtances, wherein it is thought moſt applicable, the effect ſeems to depend upon mechanical principles.

EXPERIMENT XII.

And firſt, it will be difficult to ſhew, what conſanguinity there is between ſal gem, and antimony, and iron, and zink, and bread, and camphire, and lapis calaminaris, and fleſh of divers kinds, and oiſter-ſhells, and hartſhorn, and chalk, and quick-lime; ſome of which belong to the vegetable, ſome to the mineral, and ſome to the animal kingdom; and yet all of them, and divers others, as I have tried, may, even without the aſſiſtance of external heat, be diſſolved or corroded by one ſingle mineral menſtruum, oil of vitriol. And, which is not to be neglected on this occaſion, ſome of them may be bodies, ſuppoſed, by chemiſts, to have an antipathy to each other, in point of corroſion, or diſſolution.

EXPERIMENT XIII.

I Obſerve alſo, that a diſſolution may be made of the ſame body by menſtruums, to which the chemiſts attribute (as I juſt now obſerved they did to ſome bodies) a mutual antipathy, and which therefore are not like to have a ſympathy with the ſame third body; as I found by trial, that both aqua fortis, and ſpirit of urine, upon whoſe mixture there enſues a conflict, with a great efferveſcence, will, each of them apart, readily diſſolve crude zink, and ſo each of them will the filings of copper. Not to mention, that pure ſpirit of wine, and oil of vitriol, as great a difference as there is between them, in I know not how many reſpects, and as notable a heat as will enſue upon their commixture, will each of them diſſolve camphire; to which may be added, other inſtances of the like nature. As for what is commonly ſaid, that oils diſſolve ſulphur, and ſaline menſtruums metals, becauſe (as they ſpeak) *ſimile ſimili gaudet*; I anſwer, that, where there is any ſuch ſimilitude, it may be very probably aſcribed, not ſo much with the chemiſts, that favour *Ariſtotle*, to the eſſential forms of the bodies, that are to work on each other; nor, with the mere chemiſts, to their ſalt, or ſulphur, or mercury, as ſuch; but to the congruity between the pores and figures of the menſtruum, and the body diſſolved by it, and to ſome other mechanical affections of them.

EXPERIMENT XIV.

For ſilver, for example, not only will be diſſolved by nitre, which they reckon a ſalt, but be amalgamed with, and conſequently diſſolved by quickſilver, and alſo by the operation of brimſtone, be eaſily incorporated with that mineral, which chemiſts are wont to account of ſo oleaginous a nature, and inſoluble in aqua fortis.

EXPERIMENT XV.

And as for thoſe diſſolutions, that are made with oily and inflammable menſtruums, of common ſulphur and other inflammable bodies, the diſſolution does not make for them ſo clearly as they imagine. For, if ſuch menſtruums operate, as is alledged, upon the account of their being, as well as the bodies they work upon, of a ſulphureous nature, whence is it, that highly rectified ſpirit of wine, which, according to them, muſt be of a moſt ſulphureous nature, ſince, being ſet on fire, it will flame all away, without leaving one drop behind it, will not (unleſs, perhaps, after a tedious while) diſſolve even flowers of brimſtone, which eſſential, as well as expreſſed oils, will eaſily take up; as ſpirit of wine itſelf alſo will do almoſt in a trice, if, (as we ſhall ſee anon) by the help of an alkali, the texture of the brimſtone be altered, though the only thing, that is added to the ſulphur, being an incombuſtible ſubſtance, is nothing near of ſo ſulphureous a nature, as the flowers, and need have no conſanguinity upon the ſcore of its origin with ſpirit of wine, as it is alledged, that ſalt of tartar has; ſince I have tried, that fixed nitre, employed inſtead of it, will do the ſame.

EXPERIMENT XVI.

THE mention of nitre brings into my mind, that the ſalt-petre, being wont to be looked upon, by chemiſts, as a very inflammable body, ought, according to them, to be of a very ſulphureous nature; yet we find not, that it is in chemical oils, but in water, readily diſſolved. And whereas chemiſts tell us, that the ſolutions of alkalies, ſuch as ſalt of tartar, or of pot-aſhes in common oils, proceed from the great cognition between them, I demand, whence it happens, that ſalt of tartar will, by boiling, be diſſolved in the expreſſed oil of almonds, or of olives, and be reduced with it to a ſoapy body, and that yet, with the eſſential oil of juniper or anniſeeds, &c. where what they call the ſulphur is made pure and penetrant, being freed from the earthy, aqueous and feculent parts, which diſtillation diſcovers to be in the expreſſed oils, you may boil ſalt of tartar twenty times as long, without making any ſoap of them, or perhaps any ſenſible ſolution of the alkali. And chemiſts know, how difficult it is, and how unſucceſsfully it is wont to be attempted, to diſſolve pure ſalt of tartar in pure ſpirit of wine, by digeſting the not peculiarly prepared ſalt in the cognate menſtruum. I will not urge, that, though the moſt conſpicuous mark of ſulphur be inflammability, and is in an eminent degree to be found in oil, as well as ſulphur; yet an alkali and water, which are neither ſingly, nor united inflammable, will diſſolve common ſulphur.

EXPERIMENT XVII.

BUT, to make it probable againſt the chemiſts, (for I purpoſe it but as an argument *ad hominem*) that the ſolution of ſulphur in expreſſed oils depends upon ſomewhat elſe beſides the abundance of the ſecond principle, in both the bodies; I will add to what I ſaid before, an affirmation of divers chemical writers themſelves, who reckon aqua regis, which is plainly a ſaline menſtruum, and diſſolves copper, iron, coral, &c. like acid liquors, among the ſolvents of ſulphur, and by that power, among other things, diſtinguiſh it from aqua fortis. And, on the other ſide, if there be a congruity betwixt an expreſſed oil and another body, though it be ſuch as, by its eaſy diſſolubleneſs in acid ſalts, chemiſts ſhould pronounce to be of a ſaline nature, an expreſſed oil will readily enough work upon it; as I have tried, by digeſting even crude copper, in filings, with oil of ſweet almonds, which took up ſo much of the metal, as to be deeply coloured thereby, as if it had been a corroſive liquor: nay, I ſhall add, that, even with milk, as mild a liquor as it is, I have found, by trial, that, without the help of fire, a kind of diſſolution may, though not in few hours, be made of crude copper, as appeared by the greeniſh blue colour the filings acquired, when they had been well drenched in the liquor, and left for a certain time in the veſſel, where the air had very free acceſs to them.

EXPERIMENT XVIII.

BESIDES the argument *ad hominem*, newly drawn from aqua regia, it may be proper enough to urge another of the ſame kind, upon the generality of the Helmontians and Paracelſians, who admit what the heads of their ſects delivered concerning the operations of the alkaheſt. For whereas it is affirmed, that this irreſiſtible menſtruum will diſſolve all tangible bodies here below, ſo as they may be reduced into inſipid water; as, on the one ſide, it will be very hard to conceive, how a ſpecificated menſtruum, that is determined to be either acid, or lixiviate, or urinous, &c. ſhould be able to diſſolve ſo great a variety of bodies of differing, and perhaps contrary natures, in ſome whereof acids, in others, lixiviate ſalts, and in others, urinous are predominant; ſo, on the other ſide, if the alkaheſt be not a ſpecificated menſtruum, it will very much diſfavour the opinion of the chemiſts, that will have ſome bodies diſſoluble only by acids as ſuch, others by fixed alkalies, and others again by volatile ſalts; ſince a menſtruum, that is neither acid, lixiviate, nor urinous, is able to diſſolve bodies, in ſome of which one, and in others, another of thoſe principles is predominant: ſo that, if a liquor be conveniently qualified, it is not neceſſary, that it ſhould be either acid to diſſolve pearl or coral, or alkalizate to diſſolve ſulphur. But upon what mechanical account an analyzing menſtruum may operate, is not neceſſary to be here determined. And I elſewhere offer ſome thoughts of mine about it.

EXPERIMENT XIX.

IF we duly reflect upon the known proceſs, that chemiſts are wont to employ in making mercurius dulcis, we ſhall find it very favourable to our hypotheſis. For though we have already ſhewn in the fifth experiment, and it is generally confeſſed, that common ſublimate made of mercury is a highly corroſive body; yet, if it be well ground with near an equal weight of quickſilver, and be a few times ſublimed, (to mix them the more exactly) it will become ſo mild, that it will not ſo much as taſte ſharp upon the tongue; ſo that chemiſts are wont to call it mercurius dulcis: and yet this dulcification ſeems to be performed in a mechanical way. For moſt part of the ſalts, that made the ſublimate ſo corroſive, abide in the mercurius dulcis; but by being compounded with more quickſilver, they are diluted by it, and (which is more conſiderable) acquire a new texture, which renders them unfit to operate, as they did before, when the fretting ſalts were not joined with a ſufficient quantity of the mercury to inhibit their corroſive activity. It may perhaps ſomewhat help us to conceive, how this change may be made, if we imagine, that a company of mere knife-blades be firſt fitted with hafts, which will in ſome regard leſſen their wounding power by covering or caſing them at that end, which is deſigned for the handle; (though their inſertion into thoſe hafts, turning them into knives, makes them otherwiſe the fitter to cut and pierce) and, that each of them be afterwards ſheathed, (which is, as it were, a hafting of the blades too;) for then they become unfit to cut or ſtab, as before, though the blades be not deſtroyed: or elſe we may conceive theſe blades without hafts or ſheaths to be tied up in bundles, or as it were in little faggots with pieces of wood, ſomewhat longer than themſelves, opportunely placed between them. For neither in this new conſtitution would they be fit to cut and ſtab as before. And by conceiving the edge of more or fewer of the blades to be turned inwards, and thoſe, that are not, to have more or leſs of their points and edges to be ſheathed, or otherwiſe covered by interpoſed bodies, one may be helped to imagine, how the genuine effects of the blades may be variouſly leſſened or diverſified. But, whether theſe, or any other like changes of diſpoſition be fancied, it may by mechanical illuſtrations become intelligible, how the corroſive ſalts of common ſublimate may loſe their efficacy, when they are united with a ſufficient quantity of quickſilver in mercurius dulcis: in which new ſtate, the ſalts may indeed, in a chemical phraſe, be ſaid to be ſatiated; but this chemical phraſe does not explicate, how this ſaturation takes away the corroſiveneſs from ſalts, that are ſtill actually preſent in the ſweet mercury. And by analogy to ſome ſuch explication, as the above propoſed, a poſſible account may be rendered, why fretting ſalts do either quite loſe their ſharpneſs, as alkalies, whilſt they are imbodied with ſand in common glaſs; or loſe much of their corroſive acidity, as oil of vitriol does, when with ſteel it compoſes vitriolum martis; or elſe are tranſmuted or diſguiſed by conjunction with ſome corroded bodies of a peculiar texture, as when aqua fortis does with ſilver make an extremely bitter ſalt or vitriol, and with lead one, that is poſitively ſweet, almoſt like common ſaccharum ſaturni.

EXPERIMENT XX.

TO ſhew, how much the efficacy of a menſtruum may depend even upon ſuch ſeemingly ſlight mechanical circumſtances, as one would not eaſily ſuſpect any neceſſity of, I ſhall employ an experiment, which, though the unpractiſed may eaſily fail of making well, yet, when I tried it after the beſt manner, I did it with good ſucceſs. I put then upon lead a good quantity of well rectified aqua-fortis, in which the metal, as I expected, continued undiſſolved; though, if the chemiſts ſay truly, that the diſſolving power of the menſtruum conſiſts, only in the acid ſalts, that it abounds with, it ſeems naturally to follow, that the more abundance of them there is in a determinate quantity of the liquor, it ſhould be the more powerfully able to diſſolve metalline and mineral bodies. And in effect, we ſee, that, if corroſive menſtruums be not ſufficiently dephlegmed, they will not work on divers of them. But, notwithſtanding this plauſible doctrine of the chemiſts, conjecturing, that the ſaline particles, that ſwam in our aqua fortis, might be more thronged together, than was convenient for a body of ſuch a texture of ſaline parts, and ſuch intervals between them, I diluted the menſtruum, by adding to it what I thought fit of fair water, and then found, that the deſired congruity betwixt the agent and the patient emerged, and the liquor quickly began to fall upon the metal, and diſſolve it. And if you would try an experiment to the ſame purpoſe, that needs much leſs circumſpection to make it ſucceed, you may, inſtead of employing lead, reiterate what I elſewhere mention my ſelf to have tried with ſilver, which would not diſſolve in too ſtrong aqua-fortis, but would be readily fallen upon by that liquor, when I had weakened it with common water.

AND this it may ſuffice to have ſaid at preſent of the power or faculty, that is found in ſome bodies of corroding or diſſolving others. Whereof I have not found among the Ariſtotelians, I have met with, ſo much as an offer at an intelligible account. And I the leſs expect the vulgar chemiſts will from their hypoſtatical principles afford us a ſatisfactory one, when, beſides the particulars, that from the nature of the things, and *Helmont*'s writings, have been lately alledged againſt their hypotheſis, I conſider, how ſlight accounts they are wont to give us even of the familiar phænomena of corroſive liquors. For if, for example, you aſk a vulgar chemiſt, why aqua-fortis diſſolves ſilver and copper, it is great odds but he will tell you, it is becauſe of the abundance of fretting ſalt, that is in it, and has a cognation with the ſalts of the metal. And if you aſk him, why ſpirit of ſalt diſſolves copper, he will tell you it is for the ſame reaſon; and yet, if you put ſpirit of ſalt, though very ſtrong, to aqua-fortis, this liquor will not diſſolve ſilver, becauſe upon the mixture the liquors acquire a new conſtitution as to the ſaline particles, by virtue of which the mixture will diſſolve, inſtead of ſilver, gold. Whence we may argue againſt the chemiſts, that the inability of this compounded liquor to work on ſilver does not proceed from its being weakened by the ſpirit of ſalt; as well becauſe, according to them, gold is far the more compact metal of the two, and requires a more potent menſtruum to work upon it, as becauſe this ſame compounded liquor will readily diſſolve copper. And to the ſame purpoſe, with this experiment I ſhould alledge divers others, if I thought this the fitteſt place, wherein I could propoſe them.

SECTION II.

About the Mechanical Origin of CORROSIBILITY.

CORROSIBILITY being the quality, that anſwers corroſiveneſs, he, that has taken notice of the advertiſement I formerly gave about my uſe of the term[*] corroſiveneſs, in theſe notes, may eaſily judge, in what ſenſe I employ the name of the other quality; which, whether you will ſtile it oppoſite or conjugate, for want of a better word, I call corroſibility.

THIS corroſibility of bodies is, as well as their corroſiveneſs, a relative thing; as we ſee, that gold, for inſtance, will not be diſſolved by aqua fortis, but will by aqua regis; whereas ſilver is not ſoluble by the latter of theſe menſtruums, but is by the former. And this relative affection, on whoſe account a body comes to be corrodible by a menſtruum, ſeems to conſiſt chiefly in three things, which all of them depend upon mechanical principles.

OF theſe qualifications, the firſt is, that the body to be corroded be furniſhed with pores of ſuch a bigneſs and figure, that the corpuſcles of the ſolvent may enter them, and yet not be much agitated in them, without giving briſk knocks or ſhakes to the ſolid parts, that make up the walls, if I may ſo call them, of the pores. And it is for want of this condition, that glaſs is penetrated in a multitude of places, but not diſſipated or diſſolved by the incident beams of light, which permeate its pores without any conſiderable reſiſtance; and though the pores and commiſſures of a body were leſs minute, and capable of letting in ſome groſſer corpuſcles, yet if theſe were, for want of ſolidity or rigidneſs, too flexible, or were of a figure incongruous to that of the pores they ſhould enter, the diſſolution would not enſue, as it happens, when pure ſpirit of wine is in the cold put upon ſalt of tartar, or when aqua fortis is put upon powder or ſulphur.

THE ſecond qualification of a corrodible body is, that its conſiſtent corpuſcles be of ſuch a bulk and ſolidity, as does not render them uncapable of being disjoined by the action of the inſinuating corpuſcles of the menſtruum. Agreeable to this, and the former obſervation, is the practice of chemiſts, who oftentimes, when they would have a body to be wrought on by a menſtruum, otherwiſe too weak for it in its crude eſtate, diſpoſe it to receive the action of the menſtruum by previouſly opening it, (as they ſpeak) that is, by enlarging the pores, making a comminution of the corpuſcles, or weakening their coheſion. And we ſee, that divers bodies are brought by fit preparations to be reſoluble in liquors, that would not work on them before. Thus, as was lately noted, lime-ſtone by calcination becomes, in part, diſſoluble in water; and ſome metalline calces will be ſo wrought on by ſolvents, as they would not be by the ſame agents, if the preparation of the metalline or other body had not given them a new diſpoſition. Thus, though crude tartar, eſpecially in lumps, is very ſlowly and difficultly diſſoluble in cold water, yet when it is burnt, it may be preſently diſſolved in that liquor; and thus, though the filings and the calx of ſilver will not be at all diſſolved by common water, or ſpirit of wine; yet if by the interpoſition of the ſaline particles of aqua fortis, the lunar corpuſcles be ſo disjoined, and ſuffer ſuch a comminution as they do in cryſtals of lune, the metal thus prepared and brought with its ſaline additament into a new texture, will eaſily enough diſſolve, not only in water, but, as I have tried, in well rectified ſpirit of wine. And the like ſolubility I have found in the cryſtals of lead, made with ſpirit of verdigreaſe, or good diſtilled vinegar, and in thoſe of copper made with aqua fortis.

THE laſt diſpoſition to corroſibility conſiſts in ſuch a coheſion of the parts, whereof a body is made up, as is not too ſtrict to be ſuperable by the action of the menſtruum. This condition, though of kin to the former, is yet ſomewhat differing from it, ſince a body may conſiſt of parts, either bulky or ſolid, which yet may touch one another in ſuch ſmall portions of their ſurfaces, as to be much more eaſily diſſociable than the minute or leſs ſolid parts of another body, whoſe contact is more full and cloſe, and ſo their coheſion more ſtrict.

BY what has been ſaid, it may ſeem probable, that, as I formerly intimated, the corroſibility of bodies is but a mechanical relation, reſulting from the mechanical affections and contexture of its parts, as they intercept pores of ſuch ſizes and figures, as make them congruous to thoſe of the corpuſcles of the menſtruum, that are to pierce between them, and disjoin them.

THAT the quality, that diſpoſes the body it affects to be diſſolved by corroſive and other menſtruums, does, as hath been declared, in many caſes depend upon the mechanical texture

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[*] See the beginning of the firſt Section.

ture and affections of the body in reference to the menſtruum, that is to work upon it, may be made very probable, by what we are in due place to deliver concerning the pores of bodies and figures of corpuſcles. But yet in compliance with the deſign of theſe notes, and agreeably to my cuſtom on other ſubjects, I ſhall ſubjoin a few experiments on this occaſion alſo.

EXPERIMENT I.

IF we put highly rectified ſpirit of wine upon crude ſulphur, or even flowers of ſulphur, the liquor will lie quietly thereon, eſpecially in the cold, for many hours and days, without making any viſible ſolution of it; and if ſuch exactly dephlegmed ſpirit were put on very dry ſalt of tartar, the ſalt would lie in an undiſſolved powder at the bottom: and yet, if before any liquor be employed, the ſulphur be gently melted, and then the alkali of tartar be by degrees put to it, and incorporated with it; as there will reſult a new texture diſcoverable to the eye by the new colour of the compoſition, ſo there will emerge a diſpoſition, that was not before in either of the ingredients, to be diſſolved by ſpirit of wine; inſomuch, that though the mixture be kept till it be quite cold, or long after too, provided it be carefully ſecured from the acceſs of the air, the ſpirit of wine being put to it, and ſhaken with it, will, if you have gone to work aright, acquire a yellow tincture in a minute of an hour; and perhaps in leſs than half a quarter of an hour a red one, being richly impregnated with ſulphureous particles diſcoverable by the ſmell, taſte, and divers operations.

EXPERIMENT II.

[IT is known to ſeveral chemiſts, that ſpirit of ſalt does not diſſolve crude mercury in the cold; and I remember, I kept them for a conſiderable time in no contemptible heat without finding any ſolution following. But I ſuppoſe, many of them will be gratified by an experiment once mentioned to me by an ingenious German gentleman, namely, that if mercury be precipitated *per ſe*, that is, reduced to a red powder without additament, by the mere operation of the fire, the texture will be ſo changed, that the abovementioned ſpirit will readily diſſolve it; for I found it upon trial to do ſo; nay, ſometimes ſo readily, that I ſcarce remember, that I ever ſaw any menſtruum ſo nimbly diſſolve any metalline body whatſoever.

EXPERIMENT III.

THE former experiment is the more remarkable, becauſe, that though oil of vitriol will in a good heat corrode quickſilver, (as we have already related in the firſt ſection,) yet I remember I kept a precipitate *per ſe* for divers hours in a conſiderable degree of heat, without finding it to be diſſolved or corroded by the menſtruum. And yet having, for trial's ſake, put another parcel of the ſame mercurial powder into ſome aqua fortis, or ſpirit of nitre, there enſued a ſpeedy diſſolution even in the cold.

AND that this diſpoſition to be diſſolved by ſpirit of ſalt, that mercury acquires by being turned into precipitate *per ſe*, that is, by being calcined, is not merely the effect of the operation of the fire upon it, but of ſome change of texture produced by that operation; may be probably argued from hence, that, whereas ſpirit of ſalt is a very proper menſtruum, as I have often tried, for the diſſolving of iron or ſteel; yet, when that metal is reduced by the action of the fire (eſpecially if a kind of vitrification, and an irroration with diſtilled vinegar have preceded) to crocus martis, though it be thereby brought to a very fine powder, yet I found not, that, as ſpirit of ſalt will readily, and with heat and noiſe, diſſolve filings of mars, ſo it would have the ſame or any thing near ſuch an operation upon the crocus; but rather, after a good while, it would leave in the bottom of the glaſs a conſiderable, if not the greateſt part of it ſcarce, if at all, ſenſibly altered. And the menſtruum ſeemed rather to have extracted a tincture; than made an ordinary ſolution; ſince the colour of it was a high yellow or reddiſh, whereas mars, diſſolved in ſpirit of ſalt, affords a green ſolution. Whether by repeated operations with freſh menſtruum further diſſolutions might in time be made, I had not occaſion to try, and it may ſuffice for our preſent purpoſe, that mars, by the operation of the fire, did evidently acquire, not, as mercury had done, a manifeſt facility, but on the contrary, a great indiſpoſition to be diſſolved by ſpirit of ſalt.

To ſecond this experiment, we varied it, by employing, inſtead of ſpirit of ſalt, ſtrong oil of vitriol, which being poured on a little crocus martis made *per ſe*, did not, as that menſtruum is wont do upon filings of crude mars, readily and manifeſtly fall upon the powder with froth and noiſe, but, on the contrary, reſted for divers hours calmly upon it, without ſo much as producing with it any ſenſible warmth.

EXPERIMENT IV.

IT agrees very well with our doctrine about the dependants of the corroſibility of bodies upon their texture, that from divers bodies, whilſt they are in conjunction with others, there reſult maſſes, and thoſe homogeneous as to ſenſe, that are eaſily diſſoluble in liquors, in which a great part of the matter, if it were ſeparated from the reſt, would not be at all diſolved. Thus we ſee, that common vitriol is eaſily diſſolved in mere water; whereas if it be ſkilfully calcined, it will yield ſometimes near half its firſt weight of inſipid colcothar, which not only is not ſoluble in water, but which neither aqua fortis nor aqua regis, though ſometimes they will colour themſelves upon it, are able, as far as I have tried, to make ſolutions of. We ſee likewiſe, that ſimple water will, being boiled for a competent time with hart's-horn, diſſolve it and make a jelly of it: and yet, when we have taken harts-horn throughly calcined to whiteneſs, not only we found, that common water was no longer a fit ſolvent for it, but we obſerved, that when we put oil of vitriol itſelf upon it, a good part of the white powder was even by that corroſive menſtruum left undiſſolved.

EXPERIMENT V.

IN the fifteenth of the foregoing experiments I refer to a way of making the flower or powder of common ſulphur become eaſily diſſoluble, which otherwiſe it is far from being, in highly rectified ſpirit of wine. Wherefore I ſhall now add, that it is quickly performed by gently melting the ſulphur, and incorporating with it by degrees an equal or a greater weight of finely powdered ſalt of tartar, or of fixed nitre. For if the mixture be put warm into a mortar, that is ſo too; and as ſoon as it reduced to powder, but put into a glaſs, and well ſhaken with pure ſpirit of wine, it will, (as perhaps I may have elſewhere obſerved) in a few minutes acquire a yellow colour, which afterwards will grow deeper, and manifeſt itſelf by the ſmell and effects to be a real ſolution of ſulphur; and yet this ſolubleneſs in ſpirit of wine ſeems procured by the change of texture, reſulting from the commixtion of mere ſalt of tartar, which chemiſts know, to their trouble, to be itſelf a body almoſt as difficult as ſulphur to be diſſolved in phlegmleſs ſpirit of wine, unleſs the conſtitution of it be firſt altered by ſome convenient additament. Which laſt words I add, becauſe, though ſpirit of verdigreaſe be a menſtruum, that uſes to come off in diſtillation much more intirely than other acid menſtruums from the bodies it has diſſolved; yet it will ſerve well for an additament to open (as the chemiſts ſpeak) the body of the ſalt of tartar. For this purpoſe I employ ſpirit of verdigreaſe, not made firſt with ſpirit of vinegar, and then of wine, after the long and laborious way preſcribed by *Baſilius* and *Zwelfer*, but eaſily and expeditiouſly by a ſimple diſtillation of crude verdigreaſe of the better ſort. For when you have with this liquor (being, if there be need, once rectified) diſſolved as much good ſalt of tartar, as it will take up in the cold, if you draw off the menſtruum *ad ſiccitatem*, the remaining dry ſalt will be manifeſtly altered in texture even to the eye, and will readily enough, in high rectified ſpirit of wine, afford a ſolution, which I have found conſiderable in order to divers uſes, that concern not our preſent diſcourſe.

EXPERIMENT VI.

TO the conſideration of the followers of *Helmont* I ſhall recommend an experiment of that famous chemiſt's, which ſeems to ſute exceeding well with the doctrine propoſed in this ſection. For he tells us, that, if by a ſubtle menſtruum, to which he aſcribes that power, quickſilver be diveſted (or deprived) of its external ſulphur, as he terms it, all the reſt of the fluid metal, which he wittily enough ſtiles the kernel of mercury, will be no longer corroſible by it. So that upon this ſuppoſition, though common quickſilver be obſerved to be ſo obnoxious to aqua-fortis, that the ſame quantity of that liquor will diſſolve more of it, than of any other metal; yet, if by the deprivation of ſome portion of it the latent texture of the metal be altered, though not (that I remember) the viſible appearance of it; the body, that was before ſo eaſily diſſolved by aqua fortis, ceaſes to be at all diſſoluble by it.

EXPERIMENT VII.

AS for thoſe chemiſts of differing ſects, that agree in giving credit to the ſtrange things, that are affirmed of the operations of the alkaheſt, we may in favour of our doctrine urge them with what is delivered by *Helmont*, where he aſſerts, that all ſolid bodies, as ſtones, minerals, and metals themſelves, by having this liquor duly abſtracted or diſtilled off from them, may be changed into ſalt, equiponderant to the reſpective bodies whereon the menſtruum was put. So, that ſuppoſing the alkaheſt to be totally abſtracted, (as it ſeems very probable to be, ſince the weight of the body, whence it was drawn off, is not altered;) what other change, than of texture, can be reaſonably imagined to have been made in the tranſmuted bodies? and yet divers of them, as flints, rubies, ſapphires, gold, ſilver, &c. that were inſoluble before, ſome of them in any known menſtruums, and others in any but corroſive liquors, come to be capable of being diſſolved in common water.

EXPERIMENT VIII.

IT is a remarkable phænomenon, that ſuits very well with our opinion about the intereſt of mechanical principles in the corroſive power of menſtruums, and the corroſibility of bodies, that we produced by the following experiment: this we purpoſely made to ſhew, after how differing manners the ſame body may be diſſolved by two menſtruums, whoſe minute parts are very differingly conſtituted and agitated. For whereas it is known, that if we put large grains of ſea ſalt into common water, they will be diſſolved therein, calmly and ſilently, without any appearance of conflict; if we put ſuch grains of ſalt into good oil of vitriol, that liquor will fall furiouſly upon them, and produce for a good while a hiſſing noiſe with fumes, and a great ſtore of bubbles, as if a potent menſtruum were corroding ſome ſtubborn metal or mineral. And this experiment I the rather mention, becauſe it may be of uſe to us on divers other occaſions. For elſe it is not the only, though it be the remarkableſt, that I made to the ſame purpoſe.

EXPERIMENT IX.

FOR, whereas aqua fortis, or aqua regis, being poured upon filings of copper, will work upon them with much noiſe and ebullition, I have tried, that ſpirit of ſal armoniac or urine, being put upon the like filings, and left there without ſtopping the glaſs, will quickly begin to work on them, and quietly diſſolve them almoſt as water diſſolves ſugar. To which may be added, that even with oil of turpentine I have, though but ſlowly, diſſolved crude copper; and the experiment ſeemed to favour our conjecture the more, becauſe having tried it ſeveral times, it appeared, that common unrectified oil would perform the ſolution much quicker than that, which was purified and ſubtilized by rectification; which though more ſubtle and penetrant, yet was, it ſeems, on that account leſs fit to diſſolve the metal, than the groſſer oil, whoſe particles might be more ſolid, or more advantageouſly ſhaped, or on ſome other mechanical account better qualified for the purpoſe.

EXPERIMENT X.

TAKE good ſilver, and, having diſſolved it in aqua-fortis, precipitate it with a ſufficient quantity of good ſpirit of ſalt; then having waſhed the calx, which will be very white, with common water, and dried it well, melt it with a moderate fire into a fuſible maſs, which will be very much of the nature of what chemiſts call cornu lunæ, and which they make by precipitating diſſowed ſilver with a bare ſolution of common ſalt made in common water. And whereas both ſpirit of ſalt and ſilver, diſſolved in aqua fortis, will each of them apart readily diſſolve in ſimple water, our luna cornea not only will not do ſo, but is ſo indiſpoſed to diſſolution, that I remember I have kept it in digeſtion, ſome in aqua fortis, and ſome in aqua regia, and that for a good while, and in no very faint degree of heat, without being able to diſſolve it like a metal, the menſtruums having indeed tinged themſelves upon it, but left the compoſition undiſſolved at the bottom.

WITH this inſtance (of which ſort more might be afforded by chemical precipitations) I ſhall conclude what I deſigned to offer at preſent about the corroſibility of bodies, as it may be conſidered in a more general way. For as to the diſpoſition, that particular bodies have of being diſſolved in, or of reſiſting determinate liquors, it were much eaſier for me to enlarge upon that ſubject, than it was to provide the inſtances above recited. And theſe are not ſo few, but that it is hoped they may ſuffice to make it probable, that in the relation betwixt a ſolvent and a body it is to work upon, that, which depends upon the mechanical affections of one or both, is much to be conſidered, and has a great intereſt in the operations of one of the bodies upon the other.

OF THE
MECHANICAL CAUSES
OF
CHEMICAL PRECIPITATION.

ADVERTISEMENT.

THOUGH I ſhall not deny, that, in grammatical ſtrictneſs, precipitation ſhould be reckoned among chemical operations, not qualities; yet I did not much ſcruple to inſert the following diſcourſe among the notes about particular qualities, becauſe many, if not moſt of the phænomena, mentioned in the enſuing eſſay, may be conſidered as depending, ſome of them, upon a power, that certain bodies have to cauſe precipitation, and ſome upon ſuch a diſpoſition, to be ſtruck down by others, as may, if men pleaſe, be called precipitability. And ſo theſe differing affections may, with (at leaſt) tolerable congruity, be referred to thoſe, that we have elſewhere ſtiled chemical qualities.

But though, I hope, I may, in theſe few lines, have ſaid enough concerning the name given to theſe attributes, yet, perhaps, it will be found in time, that the things themſelves may deſerve a larger diſcourſe, than my little leiſure would allow them. For, that is not a cauſeleſs intimation of the importance of the ſubject, wherewith I conclude the following tract; ſince, beſides that, many more inſtances might have been particularly referred to the heads treated of in the enſuing eſſay, there are improper kinds of precipitation, (beſides thoſe mentioned in the former part of the diſcourſe) to which one may not incongruouſly refer divers of the phænomena of nature, as well in the greater as in the leſſer world, whereof either no cauſes at all, or but improper ones, are wont to be given. And, beſides the ſimple ſpirits and ſalts uſually employed by chemiſts, there are many compounded and decompounded bodies, not only factitious, but natural, (and ſome ſuch, as one would ſcarce ſuſpect) that may, in congruous ſubjects, produce ſuch precipitations, as I ſpeak of. And the phænomena and conſequents of ſuch operations may, in divers caſes, prove conducive both to the diſcovery of phyſical cauſes, and the production of uſeful effects; though the particularizing of ſuch phænomena do rather belong to a hiſtory of precipitations, than to ſuch a diſcourſe as that, which follows, wherein I propoſed not ſo much to deliver the latent myſteries, as to inveſtigate the mechanical cauſes of precipitation.

Of the MECHANICAL CAUSES of CHEMICAL PRECIPITATION.

CHAP. I.

BY precipitation is here meant, ſuch an agitation or motion of a heterogeneous liquor, as in no long time makes the parts of it ſubſide, and that uſually in the form of a powder, or other conſiſtent body.

As, on many occaſions, chemiſts call the ſubſtance, that is made to fall to the bottom of the liquor, the precipitate; ſo, for brevity-ſake, we ſhall call the body, that is put into the liquor to procure that ſubſiding, the precipitant; as alſo that, which is to be ſtruck down, the precipitable ſubſtance or matter, and the liquor, wherein it ſwims before the ſeparation, the menſtruum or ſolvent.

WHEN a haſty fall of a heterogeneous body is procured by a precipitant, the operation is called precipitation, in the proper or ſtrict ſenſe: but when the ſeparation is made without any ſuch addition, or the ſubſtance, ſeparated from the fluid part of the liquor, inſtead of ſubſiding emerges, then the word is uſed in a more comprehenſive, but leſs proper acceptation.

AS for the cauſes of precipitation, the very name itſelf, in its chemical ſenſe, having been ſcarce heard of in the peripatetick ſchools, it is not to be expected, that they ſhould have given us an account of the reaſons of the thing. And it is like, that thoſe few Ariſtotelians, that have, by their converſe with the laboratories or writings of chemiſts, taken notice of this operation, would, according to their cuſtom on ſuch occaſions, have recourſe for the explication of it to ſome ſecret ſympathy or antipathy between the bodies, whoſe action and re-action intervenes in this operation.

BUT if this be the way propoſed of accounting for it, I ſhall quickly have occaſion to ſay ſomewhat to it, in conſidering the ways propoſed by the chemiſts, who were wont to refer precipitation, either, as is moſt uſual, to a ſympathy betwixt the precipitating body and the menſtruum, which makes the ſolvent run to the embraces of the precipitant, and ſo let fall the particles of the body ſuſtained before; or (with others) to a great antipathy, or contrariety between the acid ſalt of the menſtruum and the fixed ſalt of the oil, or ſolution of calcined tartar, which is the moſt general and uſual precipitant they employ.

BUT I ſee not, how either of theſe cauſes will either reach to all the phænomena, that have been exhibited, or give a true account even of ſome of thoſe, to which it ſeems applicable. For firſt, in precipitations, wherein what they call a ſympathy between the liquors, is ſuppoſed to produce the effect, this admired ſympathy does not (in my apprehenſion) evince ſuch a myſterious occult quality, as is preſumed, but rather conſiſts in a greater congruity, as to bigneſs, ſhape, motion, and pores of the minute parts between the menſtruum and the precipitant, than between the ſame ſolvent and the body it kept before diſſolved. And though this ſympathy, rightly explained, may be allowed to have an intereſt in ſome ſuch precipitations, as let fall the diſſolved body in its priſtine nature and form, and only reduced into minute powder; yet I find not, that, in the generality of precipitations, this doctrine will hold; for in ſome, that we have made of gold and ſilver in proper menſtruums, after the ſubſiding matter had been well waſhed and dried, ſeveral precipitates of gold made, ſome with oil of tartar, which abounds with a fixed ſalt, and is the uſual precipitant, and ſome with an urinous ſpirit, which works by virtue of a ſalt highly fugitive, or volatile, I found the powder to exceed the weight of the gold and ſilver I had put to diſſolve; and the eye itſelf ſufficiently diſcovers ſuch precipitates not to be mere metalline powders, but compoſitions, whoſe conſiſting, not (as hath been by ſomebody ſuſpected) of the combined ſalts alone, but of the metalline parts alſo, may be ſtrongly concluded, not only from the ponderouſneſs of divers of them, in reference to their bulk, but alſo manifeſtly from the reduction of true malleable metals from ſeveral of them.

CHAP. II.

THE other chemical way of explicating precipitations may, in a right ſenſe, be made uſe of by a naturaliſt on ſome particular occaſions. But I think it much too narrow and defective, as it is in a general way propoſed, to be fit to be acquieſced in. For firſt it is plain, that it is not only ſalt of tartar, and other fixed alkalies, that precipitate moſt bodies, that are diſſolved in acid menſtruums; as in making of aurum fulminans, oil of tartar precipitates the gold out of aqua regis: but acid liquors themſelves do, on many occaſions, no leſs powerfully precipitate metals and other bodies out of one another. Thus, ſpirit of ſalt (as I have often tried) precipitates ſilver out of aqua fortis: the corroſive ſpirit of nitre copiouſly precipitates that white powder, whereof they make bezoardicum minerale: ſpirit or oil of ſulphur, made by a glaſs bell, precipitates corals, pearls, &c. diſſolved in ſpirit of vinegar, as is known to many chemiſts, who now uſe this oleum ſulphuris per campanam, to make the magiſtery of pearls, &c. for which vulgar chemiſts employ oleum tartari per deliquium.

I have ſometimes made a menſtruum, wherein, though there were both acid and alkalizate ſalts, yet I did not find, that either acid ſpirits, or oil of tartar, or even ſpirit of urine, would precipitate the diſſolved ſubſtances.

AND I have obſerved, both that ſalts of a contrary nature will precipitate bodies out of the ſame menſtruum, as not only ſalt of tartar, but ſea-ſalt, being diſſolved, will precipitate each other, and, each of them apart, will precipitate ſilver out of aqua fortis; and that even, where there is a confeſſed contrariety betwixt two liquors, it may be ſo ordered, that neither of them ſhall precipitate what is diſſolved by the other; of which I ſhall have occaſion to give, ere long, a remarkable inſtance.

BUT it will beſt appear, that the abovementioned theories of the Peripateticks and chemiſts are at leaſt inſufficient to ſolve the phænomena (many of which were probably not known to moſt of them, and perhaps not weighed by any,) if we proceed to obſerve the mechanical ways, by which precipitations may be accounted for; whereof I ſhall, at preſent, propoſe ſome number, and ſay ſomewhat of each of them apart; not that I think all of them to be equally important and comprehenſive, or that I abſolutely deny, that any one of them may be reduced to ſome of the other; but that I think, it may better elucidate the ſubject, to treat of them ſeverally, when I ſhall have premiſed, that I would not thence infer, that though, for the moſt part, nature does principally effect precipitations by one or other of theſe ways, yet, in divers caſes, ſhe may not employ two, or more of them, about performing the operation.

To precipitate the corpuſcles of a metal out of a menſtruum, wherein, being once thoroughly diſſolved, it would, of itſelf, continue in that ſtate, the two general ways, that the nature of the thing ſeems to ſuggeſt to him, that conſiders it, are, either to add to the weight or bulk of the diſſolved corpuſcles, and thereby render them unfit to accompany the particles of the menſtruum in their motions; or to weaken the ſuſtaining power of the menſtruum, and thereby diſable it to keep the metalline particles ſwimming any longer: which falling of the deſerted parts of the metal, or other body, does oftentimes the more eaſily enſue, becauſe in many caſes, when the ſuſtaining particles of the menſtruum come to be too much weakened, that proves an occaſion to the metalline corpuſcles, diſturbed in the former motion, that kept them ſeparate, to make occurſions and coalitions among themſelves, and their fall becomes the effect, though not equally ſo, of both ways of precipitation; as on the other ſide, there are ſeveral occaſions on which the ſame precipitant, that brings the ſwimming particles of the metal to ſtick to one another, does likewiſe, by mortifying or diſabling the ſaline ſpirits, or other parts of the ſolvent, weaken the ſuſtaining power of that liquor.

CHAP. III.

To deſcend now to the diſtinct conſiderations about theſe two ways: the firſt of the moſt general cauſes of precipitation is ſuch a coheſion procured by the precipitant in the ſolution, as makes the compounded corpuſcles, or at leaſt the aſſociated particles of the diſſolved body, too heavy to be ſuſtained, or too bulky to be kept in a ſtate of fluidity by the liquor.

That in many precipitations there is made a coalition betwixt the ſmall parts of the precipitant, and thoſe of the diſſolved metal, or other body and frequently alſo, with the ſaline ſpirits of the menſtruum, may be eaſily ſhewn, by the weight of the precipitate, which, though carefully waſhed and dried, often ſurpaſſes, and ſometimes very conſiderably, that of your crude metal, that was diſſolved; of which we lately gave an inſtance in aurum fulminans, and precipitated ſilver; and we may yet give a more conſpicuous one, in that, which chemiſts call luna cornea: for, if having diſſolved ſilver in good aqua fortis, you precipitate it with the ſolution of ſea ſalt in fair water, and from the very white precipitate waſh the looſe adhering ſalts, the remaining powder, being dried and ſlowly melted, will look much leſs like a metalline body, than like a piece of horn, whence alſo it takes its name; ſo conſiderable is the additament of the ſaline to the metalline particles.

And that part of ſuch additaments is retained, may not only be found by weighing, but, in divers caſes, may be argued from what is obvious to the eye: as if you diſſolve mercury in aqua fortis, and into the philtrated ſolution drop ſpirit of ſalt, or ſalt water, or an urinous ſpirit, as of ſal armoniac, you will have a very white precipitate; but if, inſtead of any of theſe, you drop in deliquated ſalt of tartar, your precipitate will be of a brick, or orange colour. From which experiment, and ſome others, I would gladly take a riſe to perſuade chemiſts and phyſicians, that it is not ſo indifferent, as thoſe ſeem to think, who look on precipitation but as a kind of comminution, by what means the precipitation is performed. For, by reaſon of the ſtrict adheſion of divers ſaline particles of the precipitant and the ſolvent, the precipitated body, notwithſtanding all the wonted ablutions, may have its qualities much diverſified by thoſe of the particles of the liquors, when theſe are fitted to ſtick very faſt to it. Which laſt words I add, becauſe, though that ſometimes happens, yet it does not always, there being a greater difference, than every body takes notice of, between precipitations; as you will be induced to think, if you precipitate the ſolution of ſilver with copper, with ſpirit of ſal armoniac, with ſalt water, with oil of tartar, with quickſilver, with crude tartar, and with zink. And in the lately propoſed example, you will think it probable, that it is not all one, whether to diſſolved mercury or ſilver, you employ the ſubtile diſtilled ſpirits of ſalt, or the groſs body, whether in a dry form, or barely diſſolved in common water. And thus much of the conduciveneſs of weight to the ſtriking down the corpuſcles of a diſſolved body.

That alſo the bulk of a body may very much contribute to make it ſink or ſwim in a liquor, appears by obvious inſtances. Thus ſalt or ſugar, being put into water, either in lumps or even in powder, that is but groſs, falls at firſt to the bottom, and lies there, notwithſtanding the air, that may be intercepted between its parts, or externally adhere to it. But when, by the inſinuating action of the water, it it is diſſolved into minute particles, theſe are carried up and down with thoſe of the liquor, and ſubſide not. The like happens, when a piece of ſilver is caſt into aqua fortis, and in many other caſes.

On the other ſide I have ſeveral times obſerved, that ſome bodies, that had long ſwam in a menſtruum, whilſt their minute parts were kept from convening in it, did afterwards, by the coalition of many of thoſe particles into bodies of a viſible bulk, coagulate and ſubſide, (though ſometimes, to hinder the evaporation of the menſtruum, the veſſels were kept ſtopped.) Of this I elſewhere mention divers examples; and particularly in urinous and animal ſpirits, well dephlegmed, I have found, that after all had, for a conſiderable time, continued in the form of a perfect liquor, and as to ſenſe homogeneous, ſtore of ſolid corpuſcles, convening together, ſettled at the bottom of the glaſſes in the form of ſaline cryſtals. Having alſo long kept a very red ſolution of ſulphur firſt unlocked, (as they ſpeak) made with highly rectified ſpirit of urine, I obſerved, that at length the ſulphureous particles, making little concretions between themſelves, totally ſubſided

ſided, and left the liquor almoſt devoid of tincture. By which you may ſee, that it was not impertinent to mention (as I lately did) among the ſubordinate cauſes of precipitation, the aſſociating of the particles of a diſſolved body with one another. Of which I elſewhere give a notable example in the ſhining powder, that I obtained from gold diſſolved in a peculiar menſtruum, without any precipitant, by the coalition of the metalline particles, to which a tract of time gave opportunity to meet and adhere in a convenient manner.

If in what the chemiſts call *præcipitate per ſe*, the mercury be indeed brought to loſe its fluidity, and become a powder without being compounded with any additional body, (which doubt I elſewhere ſtate and diſcourſe of,) it will afford us a notable inſtance to prove, that the coalitions of particles into cluſters of the ſelf-ſame matter will render them unfit for the motion requiſite to fluidity. For, in this odd precipitation by fire, wherein the ſame menſtruum is both the liquor and the precipitate, being not all made at once, the corpuſcles, that firſt diſcloſe themſelves by their redneſs, are rejected by thoſe of the mercury, that yet remains fluid, as unable to accompany them in the motions, that belong to mercury as ſuch.

CHAP. IV.

BEFORE I diſmiſs that way of precipitating, that depends upon the unwieldineſs, which the precipitant gives to the body it is to ſtrike down, it may not be impertinent, eſpecially in reference to the foregoing part of this paper, to conſider, that perhaps, in divers caſes, the corpuſcles of a diſſolved body may be made unfit to be any longer ſuſtained in the menſtruum, though the precipitant adds very little to their bulk, or at leaſt, much more to their ſpecific weight than to it. For I have elſewhere ſhewn, that in divers ſolutions made of bodies by acid menſtruums, there are either generated or extricated many ſmall aerial particles; and it will be eaſily granted, that theſe may be ſmall enough to be detained in the pores of the liquor, and be inviſible there, if we conſider, what a multitude of aerial and formerly imperceptible bubbles is afforded by common water in our pneumatical receivers, when the incumbent air, that before preſſed the liquor, is pumped out. And if the corpuſcles of the diſſolved body have any little cavities or pores fit to lodge aerial particles, or have aſperous ſurfaces, between whoſe prominent parts the generated air may conveniently lie; in ſuch caſes, I ſay, theſe inviſible bubbles may be looked upon, as making with the ſolid corpuſcles they adhered to, little aggregates much lighter in ſpecie than the corpuſcles themſelves would be; and conſequently if the precipitant conſiſt of particles of ſuch a ſize and ſhape as are fit to expel theſe little bubbles, and lodge themſelves in the cavities poſſeſſed by them before, there will be produced new aggregates compoſed of the corpuſcles of the diſſolved body and the particles of the precipitant; which aggregates, though they do take up very little, or perhaps not at all more room (taking that word in a popular ſenſe) than thoſe, whereof the aerial bubbles made a part, will yet be ſpecifically heavier than the former aggregates were, and may thereby overcome the ſuſtaining power of the menſtruum.

One thing more may be fit to be taken notice of before we paſs on further, namely, that it is upon the ſcore of the ſpecifick gravity of a body, and not barely upon the action of the precipitant, that an aggregate, or a convention, of particles, does rather fall to the bottom than riſe to the top. For, though the agents, that procured the coalition, make the cluſter of particles become of a bulk too unwieldy to continue in the liquor as parts of it; yet if each of them be lighter in ſpecie than an equal bulk of the menſtruum, or if they ſo convene, as to intercept a ſufficient number of little bubbles or aerial corpuſcles between them, and ſo become lighter than as much of the menſtruum, as they take up the room of, they will not be precipitated but emerge; as may be ſeen in the preparation of thoſe magiſteries of vegetables, I elſewhere mention; where ſome deeply coloured plants being made to tinge plentifully the lixivium they are boiled in, are afterwards by the addition of alum made to curdle, as it were, into coloured concretions, which being (totally or in part) too big to ſwim, as they did before they convened, and too light in compariſon of the menſtruum to ſubſide, emerge to the top, and float there. An eaſier and neater example to the ſame purpoſe, I remember I ſhewed by diſſolving camphire in highly rectified ſpirit of wine, till the ſolution was very ſtrong. For though the camphire, when put in lumps into the ſpirit, ſunk to the bottom of it; yet, when good ſtore of water, (a liquor ſomewhat heavier in ſpecie than camphire,) was poured upon the ſolution, the camphire quickly concreted and returned to its own nature, and within a while emerged to the top of the mingled liquors and floated there. Theſe particulars I was willing to mention here, that I might give an inſtance or two of thoſe precipitations, that I formerly ſpake of as improperly ſo called. And here I muſt not decline taking notice of a phænomenon, that ſometimes occurs in precipitations, and at firſt ſight may ſeem contrary to our doctrine about them. For now and then it happens, that after ſome drops of the precipitant have begun a precipitation at the top or bottom of the ſolvent, one ſhakes the veſſel, that the precipitant may be the ſooner diffuſed through the other liquor, but then they are quickly ſurprized to find, that inſtead of haſtening the compleat precipitation, the matter already precipitated diſappears, and the ſolvent returns to be clear, or, as to ſenſe, as uniform, as it was before the precipitant was put into it. But this phænomenon does not at all croſs our theory. For, when this happens, though that part of the ſolvent, to which the precipitant reaches, is diſabled for reaſons mentioned in this diſcourſe to ſupport the diſſolved body, yet this quantity of the precipitant is but ſmall in proportion to the whole bulk of the ſolvent. And therefore, when the agitation of the veſſel diſperſes the cluſters of looſly concreted particles through the whole liquor, (which is ſeldom ſo exactly proportioned to the body it was to work on, as to be but juſt ſtrong enough to diſſolve it) that greater part of the liquor, to which before the ſhaking of the veſſel the precipitant did not reach, may well be looked upon as a freſh menſtruum, which is able to mortify or overpower the ſmall quantity of the precipitant, that is mingled with it, and ſo to deſtroy its late operation on the body diſſolved, by which means the ſolution returns, as to ſenſe, to its former ſtate. Which may be illuſtrated by a not unpleaſant experiment, I remember, I have long ſince made by precipitating a brick-coloured powder out of a ſtrong ſolution of ſublimate made in fair water. For this ſubſiding matter, being laid to dry in the philter, by which it was ſeparated from the water, would retain a deep, but ſomewhat dirty colour; and if then, putting it into the bottom of a wine glaſs, I poured upon it, either clear oil of vitriol, or ſome other ſtrong acid menſtruum, the alcalizate particles being diſabled and ſwallowed up by ſome of the acid ones of the menſtruum, the other acid ones would ſo readily diſſolve the reſidue of the powder, that in a trice the colour of it would diſappear, and the whole mixture be reduced into a clear liquor, without any ſediment at the bottom.

Thus much may ſuffice at preſent about the firſt general way of precipitating bodies out of the liquors they ſwam in.

CHAP. V.

THE other of the two principal ways, by which precipitations may be effected, is the diſabling the ſolvent to ſuſtain the diſſolved body.

There may be many inſtances, wherein this ſecond way of effecting precipitations may be aſſociated by nature with the firſt way formerly propoſed; but notwithſtanding the caſes, wherein nature may (as I formerly noted) employ both the ways therein, yet in moſt caſes they ſufficiently differ, in regard that in the former way the ſubſiding of the diſſolved body is chiefly, if not only, cauſed by the additional weight, as well as action of the external precipitant; whereas, in moſt of the inſtances of the later way, the effect is produced either without ſalt of tartar, or any ſuch precipitant, or by ſome other quality of the precipitant more than by its weight, or at leaſt beſides the weight it adds: though I forget not, that I lately gave an example of a ſhining powder of gold, that fell to the bottom of a menſtruum without the help of an external precipitant: but that was done ſo ſlowly, that it may be diſputed, whether it were a true precipitation; and I alledged it not as ſuch, but to ſhew, that the increaſed bulk of particles may make them unfit to ſwim in menſtruums, wherein they ſwam, whilſt they were more minute. And the like anſwer may be accommodated to the precipitate *per ſe* newly mentioned.

This premiſed, I proceed now to obſerve, that the general way, I laſt propoſed, contains in it ſeveral ſubordinate ways, that are more particular; of which I ſhall now mention the chief, that occur to me, and, though but briefly, illuſtrate each of them by examples. And firſt a precipitation may be made, if the ſaline or other diſſolving particles of the menſtruum are mortified or rendered unfit for their former function, by particles of a precipitant, that are of a contrary nature.

Thus gold and ſome other minerals, being diſſolved in aqua regis, will be precipitated with ſpirit of urine and other ſuch liquors abounding with volatile and ſalino-ſulphureous corpuſcles, upon whoſe account it is, that they act; whence theſe ſalts themſelves, though caſt into a menſtruum in a dry form, will ſerve to make the like precipitations. And I the rather on this occaſion mention urinous ſpirits than ſalt of tartar, becauſe thoſe volatile particles add much leſs of weight to the little concretions, which compoſe the precipitated powder.

Upon inſtances of this kind, many of the modern chemiſts have built that antipathy betwixt the ſalts of the ſolvent and thoſe of the menſtruum, to which they aſcribe almoſt all precipitations. But againſt this I have repreſented ſomething already, and ſhall partly now, and partly in the ſequel of this diſcourſe, add ſome farther reaſons of my not being ſatisfied with this doctrine. For beſides, that it is inſufficient to reach many of the phænomena of precipitation (as will ere long be ſhown) and beſides that it is not eaſy to make out, that there is any real antipathy betwixt inanimate bodies; I conſider, firſt, that ſome of thoſe menſtruums, to which this antipathy is attributed, do, after a ſhort commotion (whereby they are diſpoſed to make convenient occurſions and coalitions) amicably unite into concretions participating of both the ingredients; as I have ſomewhere ſhewn by an example purpoſely deviſed to make this out; to do which I dropped a clear ſolution of fixed nitre, inſtead of the uſual one of common ſalt, upon a ſolution of ſilver, in aqua fortis: for the ſaline particles of the ſolvent and thoſe of the precipitant, will, as I have elſewhere recited, for the moſt part friendly unite into ſuch cryſtals of nitre for the main, as they were obtained from: and though this notion of the chemiſts, if well explained, be applicable to far more inſtances then the propoſers of it ſeemed to have thought on, and may be made good uſe of in practice; yet I take it to be ſuch as is not true univerſally, and, where it is true, ought to be explicated according to mechanical principles. For if the particles of the menſtruum and thoſe of the precipitant be ſo framed, that upon the action of the one upon the other, there will be produced corpuſcles too big and unweildy to continue in the ſtate of fluidity, there will enſue a precipitation: but if the conſtitution of the corpuſcles of the precipitating and of the diſſolved body be ſuch, that the precipitant alſo itſelf is fit to be a menſtruum to diſſolve that body in; then, though there be an union of the ſalt of the precipitant and the metal (or other ſolutum) and perhaps of the ſolvent too, yet a precipitation will not neceſſarily follow, though the ſaline particles of the two liquors ſeemed, by the heat and ebullition excited between them upon their meeting, to exerciſe a great and mutual antipathy. To ſatisfy ſome ingenious men about this particular, I diſſolved zink or ſpeltar in a certain urinous ſpirit; (for there are more than one, that may ſerve the turn;) and then put to it a convenient quantity of a proper acid ſpirit; but though there would be a manifeſt conflict thereby occaſioned betwixt the two liquors; yet the ſpeltar remained diſſolved in the mixture. And I remember, that for the ſame purpoſe I deviſed another experiment, which is ſomewhat more eaſy and more clear. I diſſolved copper calcined *per ſe*, or even crude, in ſtrong ſpirit of ſalt; (for unleſs it be ſuch, it will not be ſo proper,) and having put to it by degrees a good quantity of ſpirit of ſal-armoniac, or fermented urine, though there would be a great commotion with hiſſing and bubbles produced, the copper would not be precipitated, becauſe this urinous ſpirit will, as well as the ſalt, (and much more readily) diſſolve the ſame metal, and it would be kept diſſolved notwithſtanding their operation on one another; the intervening of which, and their action upon the metalline corpuſcles, may be gathered from hence, that the green ſolution, made with ſpirit of ſalt alone, will, by the ſupervening urinous ſpirits, be changed either into a blueiſh green, or, if the proporportion of this ſpirit be very great, into a rich blue almoſt like ultramarine. And from theſe two experiments we may probably argue, that when the precipitation of a metal, &c. enſues, it is not barely on the account of the ſuppoſed antipathy betwixt the ſalts, but becauſe the cauſes of that ſeeming antipathy do likewiſe, upon a mechanical account, diſpoſe the corpuſcles of the confounded liquors ſo to cohere, as to be too unweildy for the fluid part.

CHAP. VI.

ANOTHER way, whereby the diſſolving particles of a menſtruum may be rendered unfit to ſuſtain the diſſolved body, is to preſent them another, that they can more eaſily work on.

A notable experiment of this you have in the common practice of refiners, who, to recover the ſilver out of lace, and other ſuch mixtures wherein it abounds, uſe to diſſolve it in aqua fortis, and then in the ſolution leave copper plates for a whole night (or many hours.) But if you have a mind to ſee the experiment, without waiting ſo long, you may employ the way, whereby I have often quickly diſpatched it. As ſoon then as I have diſſolved a convenient quantity, which needs not be a great one, of ſilver in cleanſed aqua fortis, I add twenty or twenty-five times as much of either diſtilled water, or rain water; (for though common water will ſometimes do well, yet it ſeldom does ſo well;) and then into the clear ſolution, I hang by a ſtring a clean piece of copper, which will be preſently covered with little ſhining plates almoſt like ſcales of fiſh, which one may eaſily ſhake off, and make room for more. And this may illuſtrate what we formerly mentioned about the ſubſiding of metalline corpuſcles, when they convene in liquors, wherein, whilſt they were diſperſed in very minute parts, they ſwam freely. For, in this operation, the little ſcales of ſilver ſeemed to be purely metalline, and there is no ſaline precipitant, as ſalt of tartar or of urine, employed to make them ſubſide. Upon the ſame ground, gold and ſilver, diſſolved in their proper menſtruums, may be precipitated with running mercury; and if a ſolution of blue vitriol (ſuch as the Roman, Eaſt-Indian, or other of the like colours) be made in water, a clean plate of ſteel or iron being immerſed in it, will preſently be over-laid with a very thin caſe of copper, which, after a while, will grow thicker; but does not adhere to the iron ſo looſely as to be ſhaken off, as the precipitated ſilver newly mentioned may be from the copper-plates whereto it adheres. And that, in theſe operations, the ſaline particles may really quit the diſſolved body, and work upon the precipitant, may appear by the lately mentioned practice of refiners, where the aqua fortis, that forſakes the particles of the ſilver, falls a working upon the copper-plates employed about the precipitation, and diſſolves ſo much of them, as to acquire the greeniſh blue colour of a good ſolution of that metal. And the copper we can eaſily again, without ſalts, obtain by precipitation out of that liquor with iron, and that too, remaining diſſolved in its place, we can precipitate with the taſteleſs powder of another mineral.

BESIDES theſe two ways of weakening the menſtruum, namely, by mortifying its ſaline particles, or ſeducing them to work on other bodies, and to forſake thoſe they firſt diſſolved, there are ſome other ways of weakening the menſtruum.

A third way of effecting this, is by leſſening or diſturbing the agitation of the ſolvent. And indeed, ſince we find by experience, that ſome liquors, when they are heated, will either diſſolve ſome bodies they would not diſſolve at all when they were cold, or diſſolve them more powerfully or copiouſly when hot than cold; it is not unreaſonable to ſuppoſe, that what conſiderably leſſens that agitation of the parts of the menſtruum, that is neceſſary to the keeping the diſſolved body in the ſtate of fluidity, ſhould occaſion the falling of it again to the bottom. In ſlow operations, I could give divers examples of the precipitating power of cold; there being divers ſolutions, and particularly that of ambergreaſe, that I had kept fluid all the ſummer, which in the winter would ſubſide. And the like may be ſometimes obſerved in far leſs time in the ſolutions of brimſtone, made in certain oleaginous menſtruums; and I have now and then had ſome ſolutions, and particularly one of benzoin, made in ſpirit of wine, that would ſurprize me with the turbidneſs (which begins the ſtate of precipitation) it would acquire upon a ſudden change of the weather towards cold, though it were not in the winter ſeaſon.

ANOTHER way of weakening the menſtruum, and ſo cauſing the precipitation of a body diſſolved in it, is the diluting or leſſening the tenacity of it, whether that tenacity proceed from viſcoſity, or the competent number and conſtipation of the parts.

OF this we have an inſtance in the magiſteries (as many chemiſts are pleaſed to call them) of jalap, benzoin, and of divers others reſinous and gummous bodies diſſolved in ſpirit of wine. For, by the affuſion of common water, the menſtruum, being too much diluted, is not able to keep thoſe particles in the ſtate of fluidity, but muſt ſuffer them to ſubſide, (as they uſually do, in the form of white powder) or, (as it may happen ſometimes,) make ſome parts emerge. Examples alſo of this kind are afforded us by the common preparations of mercurius vitæ. For, though in oil of antimony, made by the rectification of the butter, the ſaline particles are ſo numerous, and keep ſo cloſe to one another, that they are able to ſuſtain the antimonial corpuſcles they carried over with them in diſtillation, and keep them together with themſelves in the form of a liquor; yet, when by the copious affuſion of the water, thoſe ſuſtaining particles are ſeparated, and removed to a diſtance from each other, the antimonial corpuſcles, and the mercurial, (if any ſuch there were) being of a ponderous nature, will eaſily ſubſide into that emetic powder, which (when well waſhed) the chemiſts, flatteringly enough, call mercurius vitæ.

BUT here I muſt interpoſe an advertiſement, which will help to ſhew us, how much precipitations depend upon the mechanical contextures of bodies. For, though not only in the newly recited examples, but in divers others, the affuſion of water, by diluting the ſalts, and weakening the menſtruum, makes the metal, or other diſſolved body, fall precipitately to the bottom; yet if the ſaline particles of the ſolvent, and thoſe of the body, be fitted for ſo ſtrict an union, that the corpuſcles reſulting from their coalitions, will not ſo eaſily be ſeparated by the particles of water, as ſuffer themſelves to be carried up and down with them, whether becauſe of the minuteneſs of theſe compounded corpuſcles, or becauſe of ſome congruity betwixt them and thoſe of the water; they will not be precipitated out of the weakened ſolution, but ſtill continue a part of it; as I have tried partly with ſome ſolution of ſilver and gold, made in acid menſtruums, but much more ſatisfactorily in ſolutions of copper, made in the urinous ſpirit of ſal armoniac. For, though that blue ſolution were diluted with many thouſand times as much diſtilled water, as the diſſolved metal weighed, yet its ſwimming corpuſcles did, by their colour, manifeſtly appear to be diſperſed through the whole liquor.

CHAP. VII.

BUT, to proſecute our former diſcourſe, which we broke off after the mention of mercurius vitæ, it will now be ſeaſonable to add, that we have made divers other precipitations, by the bare affuſion of water, out of ſolutions, and ſometimes out of diſtilled liquors; which, for brevity-ſake, I here omit, that I may haſten to the laſt way I ſhall now ſtay to mention.

ANOTHER way then, whereby precipitations of bodies may be produced by debilitating the menſtruum they ſwim in, is by leſſening the proportion of the ſolvent to the ſolutum, without any evaporation of the liquor. Theſe laſt words I add, becauſe that, when there is an obſtruction, or any other expulſion of the menſtruum by heat, if it be total, it is called exſiccation, as when dry ſalt of tartar is obtained from the filtrated lixivium of the calcined tartar; and though the evaporation be not total, yet the effects of it are not wont to be reckoned amongſt precipitations. And although the way I am about to propoſe, if it be attentively conſidered, has much affinity with the foregoing, and the phænomena may, perhaps, in ſome ſort, be reduced to them; yet the inſtances, that I ſhall name, having not, that I know, been thought of by others, and being ſuch as every one would not deduce from what I have been mentioning, I ſhall add a word of the inducements I had to make the trials, as well as of the ſucceſs of them.

CONSIDERING then, that water will not diſſolve ſalts indefinitely, but when it has received its due proportion, it will then diſſolve no more; but, if they be put into it, let them fall to the ground, and continue undiſſolved; and that if, when water is ſatiated, any of the liquor be evaporated, or otherwiſe waſted, it will, in proportion, let fall the ſalt it had already taken up; I conclude, that, if I could mingle with water any liquor, with which its particles would more readily aſſociate than with thoſe of ſalt, the depriving the ſolution of ſo many of its aqueous particles, would be equivalent to the evaporation of as much water, or thereabouts, as they, by being united, could compoſe. Wherefore, making a lixivium of diſtilled water, or clean rain water, and of ſalt of tartar, ſo ſtrong, that if a grain more were caſt in it, it would lie undiſſolved at the bottom; I put a quantity of this fiery lixivium into a ſlender cylindrical veſſel, till it had therein reached ſuch a height, as I thought fit; then taking as much as I thought ſufficient of ſtrong ſpirit of wine, that would burn every drop away, that ſo it might have no phlegm nor water of its own, I poured this upon the ſaline ſolution; and ſhaking the liquors pretty well together, to bring them to mix as well as I could, I laid the tube in a quiet place, and afterwards found, as I expected, that there was a pretty quantity of white ſalt of tartar fallen to the bottom of the veſſel, which ſalt had been merely forſaken by the aqueous particles, that ſuſtained it before, but forſook it to paſs into the ſpirit of wine, wherewith they were more diſpoſed to aſſociate themſelves; which I concluded, becauſe having, before I poured on this laſt named liquor, made a mark on the glaſs, to ſhew how far the lixivium reached, I found, (what I looked for) that, after the precipitation, the lixivium, that remained yet ſtrong enough to continue unmixed with the incumbent ſpirit, had its ſurface, not where the mark ſhewed it had been before, but a conſiderable diſtance beneath it, the ſpirit of wine having gained in extent what it loſt in ſtrength, by receiving ſo many aqueous particles into it. I choſe to make this trial, rather with a lixivium of ſalt of tartar, than with oil of tartar per deliquium, becauſe, in this laſt named liquor, the aqueous and ſaline particles are more cloſely combined, and therefore more difficult to be ſeparated, than I thought they would be in a lixivium haſtily made, though very ſtrong. And though, by much agitation, I have ſometimes obtained ſome ſalt of tartar from the above-mentioned oil; yet the experiment ſucceeded nothing near ſo well with that liquor, as with a lixivium.

I made alſo the like trial with exceedingly dephlegmed ſpirit of wine, and as ſtrong a brine as I could make of common ſalt diſſolved, without heat, in common water; and I thereby obtained no deſpicable proportion of finely figured ſalt, that was let fall to the bottom. But this experiment, to be ſucceſsful, requires greater care in him, that makes it, than the former needs.

To confirm, and ſomewhat to vary this way of precipitation, I ſhall add, that having made a clear ſolution of choice gum Arabic in common water, and poured upon it a little high rectified ſpirit of wine; on this occaſion there was alſo made, and that in a trice, a copious precipitation of a light and purely white ſubſtance, not unpleaſant to behold. And, for further confirmation, I diſſolved a full proportion of myrrh in fair water, and into the filtrated ſolution, which was tranſparent, but of a high brown colour, I dropped a large proportion (which circumſtance is not to be omitted) of carefully dephlegmed ſpirit of wine, which, according to expectation, made a copious precipitate of the gum. And theſe inſtances I the rather ſet down in this place, becauſe they ſeem to ſhew, that ſimple water is a real menſtruum, which may have its diſſolving and ſuſtaining virtue weakened by the acceſſion of liquors, that are not doubted to be much ſtronger than it.

By ſpecifying the hitherto mentioned ways, whereby precipitations may be mechanically performed and accounted for, I would by no means be thought to deny, that there may be ſome omitted here, which either others, that ſhall conſider the matter with more attention, or I myſelf, if I ſhall have leiſure to do it, may think on. For I propoſe theſe but as the chief, that occur to my preſent thoughts; and I forbear to add more inſtances to exemplify them, becauſe I would not injure ſome of my other papers, that have a greater right to thoſe inſtances. Only this I ſhall note in general, that the doctrine and hiſtory of precipitations, if well delivered, will be a thing of more extent and moment, than ſeems hitherto to have been imagined; ſince not only ſeveral of the changes in the blood, and other liquors and juices of the human body, may thereby be the better underſtood; and they prevented, or their ill conſequences remedied; but in the practical part of mineralogy, divers uſeful things may probably be performed by the aſſiſtance of ſuch a doctrine and hiſtory. To keep which conjecture from ſeeming extravagant, I ſhall only here intimate, that it is not alone in bodies, that are naturally or permanently liquid, but in thoſe ſolid and ponderous bodies, that are for a ſhort time made ſo by the violence of the fire, that many of the things ſuggeſted by this doctrine may have place. For whilſt divers of thoſe bodies are in fuſion, they may be treated as liquors; and metals, and perhaps other heterogeneous bodies, may be obtained from them by fit, though dry precipitants, as in ſome other writings I partly did, and may elſewhere yet further declare,

* * *

EXPERIMENTS
AND
NOTES,
ABOUT THE
MECHANICAL PRODUCTION
OF
MAGNETISM.

ADVERTISEMENT concerning the following NOTES about OCCULT QUALITIES.

THE following paper (about magnetiſm and electricity) would appear with leſs diſadvantage, if the author's willingneſs and promiſe, that this tome ſhould be furniſhed with notes about ſome occult qualities, as well as about divers ſorts of thoſe, that are preſumed to be manifeſt, did not prevail with him to let the enſuing notes appear, without thoſe about the pores of bodies and figures of corpuſcles, that ſhould have preceded them, and ſome others, that ſhould have accompanied them. But the author choſe rather to venture theſe papers abroad in the condition, ſuch as it is, they now appear in, than make thoſe already printed about manifeſt quantities ſtay longer for acceſſions, which ſome troubleſome accidents will not ſuffer him to haſten to the preſs; and without which, he now fears this tome may ſwell to a more than competent bulk.

EXPERIMENTS and NOTES about the Mechanical Production of MAGNETICAL QUALITIES.

THOUGH the virtues of the loadſtone be none of the leaſt famous of occult qualities, and are perhaps the moſt juſtly admired; yet I ſhall venture to offer ſomething to make it probable, that ſome, even of theſe, may be introduced into bodies by the production of mechanical changes in them.

To make way for what I am to deliver to this purpoſe, it will be expedient to remove that general and ſettled prejudice, that has kept men from ſo much as thinking of any mechanical account of magnetiſms, which is a belief, that theſe qualities do immediately flow from the ſubſtantial form of the loadſtone, whoſe abſtruſe nature is diſproportionate to our underſtandings.

EXPERIMENT I.

BUT for my part, I confeſs, I ſee no neceſſity of admitting this ſuppoſition; for I ſee, that a piece of ſteel fitly ſhaped and well excited will, like a loadſtone, have its determinate poles, and with them point at the north and ſouth; it will draw other pieces of iron and ſteel to it, and, which is more, communicate to them the ſame kind, though not degree, of attractive and directive virtue it had itſelf, and will poſſeſs theſe faculties, not as light and tranſient impreſſions, but as ſuch ſettled and durable powers, that it may retain them for many years, if the loadſtone, to which it has been duly applied, were vigorous enough; of which ſort I remember I have ſeen one (and made ſome trials with it) that yielded an income to the owner, who received money from navigators and others, for ſuffering them to touch their needles, ſwords, knives, &c. at his excellent magnet. Now, in a piece of ſteel or iron thus excited, it is plain, that the magnetic operations may be regularly performed for whole years by a body, to which the form of a loadſtone does not belong, ſince, as it had its own form before, ſo it retains the ſame ſtill, continuing as malleable, fuſible, &c. as an ordinary piece of the ſame metal unexcited: ſo that, if there be introduced a fit diſpoſition into the internal parts of the metal by the action of the loadſtone, the metal, continuing of the ſame ſpecies it was before, will need nothing, ſave the continuance of that acquired diſpoſition to be capable of performing magnetical operations; and if this diſpoſition or internal conſtitution of the excited iron be deſtroyed, though the form of the metal be not at all injured, yet the former power of attraction ſhall be aboliſhed, as appears,

EXPERIMENT II.

WHEN an excited iron is made red hot in the fire, and ſuffered to cool again.

AND here give me leave to take notice of what I have elſewhere related to another purpoſe, namely,

EXPERIMENT III.

THAT a loadſtone may, as I have more than once tried, be eaſily deprived by ignition of its power of ſenſibly attracting martial bodies, and yet be ſcarce, if at all, viſibly changed, but continue a true loadſtone in other capacities, which, according to the vulgar philoſophy ought to depend upon its ſubſtantial form, and the loadſtone thus ſpoiled may, notwithſtanding this form, have its poles altered at pleaſure like a piece of iron; as I have elſewhere particularly declared.

AND I will confirm what I have been ſaying with an experiment, that you do not perhaps expect; namely, that though it be generally taken for granted (without being contradicted, that I know of, by any man) that, in a ſound loadſtone, that has never been injured by the fire, not only the attractive power, but the particular virtue, that it has to point conſtantly, when left to itſelf, with one of its determinate extremes to one determinate pole, flows immediately from the ſubſtantial, or at leaſt eſſential form; yet this form remaining undeſtroyed by fire, the poles may be changed, and that with eaſe and ſpeed. For among my notes about magnetical experiments, whence I borrow ſome paſſages of this paper, I find the following account.

EXPERIMENT IV.

TO ſhew, that the virtue, that a loadſtone hath by this determinate pole or extreme to attract, for example, the ſouth-end of a poiſed needle, and with the oppoſite extreme or pole the north-end of the ſame needle, I made, among other trials, the following experiment.

TAKING a very ſmall fragment of a loadſtone, I found, agreeably to my conjecture, that by applying ſometimes one pole, ſometimes the other, to that pole of (a ſmall, but) a very vigorous loadſtone, that was fit for my purpoſe, I could at pleaſure, in a few minutes, change the poles of the little fragment, as I tried by its operations upon a needle freely poiſed; though by applying a fragment a pretty deal bigger, (for in itſelf it appeared very ſmall,) I was not able, in far more hours than I employed minutes before, to make any ſenſible change of the poles.

THIS ſhort memorial being added to the preceding part of this diſcourſe, will, I hope, ſatisfy you, that how unanimouſly ſo ever men have deduced all magnetick operations from the form of the loadſtone, yet ſome internal change of pores, or ſome other mechanical alterations, or inward diſpoſition, either of the excited iron, or of the loadſtone itſelf, may ſuffice to make a body capable or uncapable of exerciſing ſome determinate magnetical operations; which may invite you to caſt a more unprejudiced eye upon thoſe few particulars, I ſhall now ſubjoin, to make it probable, that even magnetical qualities may be mechanically produced or altered.

EXPERIMENT V.

I HAVE often obſerved in the ſhops of artificers, as ſmiths, turners of metals, &c. that, when hardened and well tempered tools are well heated by attrition, if, whilſt they are thus warmed, you apply them to filings or chips, as they call them, or thin fragments of ſteel or iron, they will take them up, as if the inſtruments were touched with a loadſtone: but as they will not do ſo, unleſs they be thus excited by rubbing till they be warmed, by which means a greater commotion is made in the inner parts of the ſteel, ſo neither would they retain ſo vigorous a magnetiſm, as to ſupport the little fragments of ſteel, that ſtuck to them after they were grown cold again. Which may be confirmed by what, if I much miſremember not, I ſhewed ſome acquaintances of yours:

EXPERIMENT VI.

WHICH was, that, by barely rubbing a conveniently ſhaped piece of ſteel againſt the floor, till it had gained a ſufficient heat, it would, whilſt it continued ſo, diſcover a manifeſt, though but faint attractive power, which vaniſhed together with the adventitious heat.

EXPERIMENT VII.

WE elſewhere obſerve, which perhaps you alſo may have done, that the iron bars of windows, by having ſtood very long in an erected poſture, may, at length, grow magnetical, ſo that, if you apply the north point of a poiſed and excited needle to the bottom of the bar, it will drive it away, and attract the ſouthern; and if you raiſe the magnetic needle to the upper part of the bar, and apply it as before, this will draw the northern extreme, which the other end of the bar expelled; probably becauſe, as it is elſewhere declared, the bar is in tract of time, by the continual action of the magnetical effluvia of the terraqueous globe, turned into a kind of magnet, whoſe lower end becomes the north-pole of it, and the other the ſouthern. Therefore, according to the magnetical laws, the former muſt expel the northern extreme of the needle, and the latter draw it.

EXPERIMENT VIII.

I HAVE found indeed, and I queſtion not but other obſervers may have done ſo too, that if a bar of iron, that has not ſtood long in an erected poſture, be but held perpendicular, the forementioned experiment will ſucceed (probably upon ſuch an account as that I lately intimated:) but then this virtue, diſplayed by the extremes of the bar of iron, will not be at all permanent, but ſo tranſient, that if the bar be but inverted and held again upright, that end, which juſt before was the uppermoſt, and drew the north-end of the needle, will now, being lowermoſt, drive it away, which, as was lately obſerved, will not happen to a bar, which has been ſome years, or other competent time, kept in the ſame poſition. So that, ſince length of time is requiſite to make the verticity of a bar of iron ſo durable and conſtant, that the ſame extreme will have the ſame virtues in reference to the magnetical needle, whether you make it the upper end or the lower end of the bar, it ſeems not improbable to me, that by length of time the whole magnetick virtue of this iron may be encreaſed, and conſequently ſome degree of attraction acquired.

AND by this conſideration I ſhall endeavour to explicate that ſtrange thing, that is reported by ſome moderns to have happened in *Italy*, where a bar of iron is affirmed to have been converted into a load-ſtone, whereof a piece was kept, among other rarities, in the curious *Aldrovandus*'s *Muſæum Metallicum*. For conſidering the greatneſs of its ſpecific gravity, the malleableneſs and other properties, wherein iron differs from load-ſtone, I cannot eaſily believe, that by ſuch a way, as is mentioned, a metal ſhould be turned into a ſtone. And therefore, having conſulted the book it ſelf, whence this relation was borrowed, I found the ſtory imperfectly enough delivered; the chiefeſt and cleareſt thing in it being, that at the top of the church of *Arimini* a great iron bar, that was placed there to ſupport a croſs of an hundred pound weight, was at length turned into a loadſtone. But whether the reality of this tranſmutation was examined, and how it appeared, that the fragment of the load-ſtone preſented to *Aldrovandus* was taken from that bar of iron, I am not fully ſatisfied by that narrative. Therefore, when I remember the great reſemblance I have ſometimes ſeen in colour, beſides other manifeſt qualities, betwixt ſome load-ſtones and ſome coarſe or almoſt ruſty iron, I am tempted to conjecture, that thoſe, that obſerved this iron bar, when broken, to have acquired a ſtrong magnetical virtue, which they dreamed not, that tract of time might communicate to it, might eaſily be perſuaded, by this virtue and the reſemblance of colour, that the iron was turned into load-ſtone: eſpecially they being prepoſſeſſed with that Ariſtotelian maxim, whence our author would explain this ſtrange phænomenon, that *inter ſymbolum habentia facilis eſt tranſmutatio*.

BUT leaving this as a bare conjecture, we may take notice, that what virtue an oblong piece of iron may need a long tract of time to acquire, by the help only of its poſition, may be imparted to it in a very ſhort time, by the intervention of ſuch a nimble agent as the fire.

EXPERIMENT IX.

AS may be often, though not always, obſerved in tongs, and ſuch like iron utenſils, that, having been ignited, have been ſet to cool, leaning againſt ſome wall or other prop, that kept them in an erected poſture, which makes it probable, that the great commotion of the parts, made by the vehement heat of the fire, diſpoſed the iron, whilſt it was yet ſoft, and had its pores more lax, and parts more pliable, diſpoſed it, I ſay, to receive much quicker impreſſions from the magnetical effluvia of the earth, than it would have done, if it had ſtill been cold.

EXPERIMENT X.

AND it is very obſervable to our preſent purpoſe, what differing effects are produced by the operation of the fire, upon two magnetic bodies, according to their reſpective conſtitutions. For, by keeping a loadſtone red-hot, though you cool it afterwards in a perpendicular poſture, you may deprive it of its former power of manifeſtly attracting: but a bar of iron being ignited, and ſet to cool perpendicularly, does thereby acquire a manifeſt verticity. Of which differing events I muſt not now ſtay to enquire, whether or no the true reaſon be, that the peculiar texture, or internal conſtitution, that makes a loadſtone ſomewhat more than an ordinary ore of iron, (which metal, as far as I have tried, is the uſual ingredient of loadſtones) being ſpoiled by the violence of the fire, this rude agent leaves it in the condition of common iron, or, perhaps, of ignited iron-ore: whereas the fire does ſoften the iron itſelf, (which is a metal, not an ore) agitating its parts, and making them the more flexible, and by relaxing its pores, diſpoſes it to be eaſily and plentifully pervaded by the magnetical ſteams of the earth, from which it may not improbably be thought to receive the verticity it acquires; and this the rather, becauſe, as I have often tried, and elſewhere mentioned.

EXPERIMENT XI.

IF an oblong loadſtone, once ſpoiled by the fire, be thoroughly ignited and cooled, either perpendicularly, or lying horizontally north and ſouth, it will, as well as a piece of iron handled after the ſame manner, be made to acquire new poles, or change the old ones, as the ſkilful experimenter pleaſes. But whatever be the true cauſe of the diſparity of the fire's operation upon a ſound loadſtone and a bar of iron, the effect ſeems to ſtrengthen our conjecture, that magnetical operations may much depend upon mechanical principles. And I hope you will find further probability added to it, by ſome phænomena recited in another paper, to which I once committed ſome promiſcuous experiments and obſervations magnetical.

EXPERIMENT XII.

IF I may be allowed to borrow an experiment from a little tract [*], that yet lies by me, and has been ſeen but by two or three friends, it may be added to the inſtances already given about the production of magnetiſm. For in that experiment I have ſhewn, how having brought a good piece of a certain kind of Engliſh oker, which yet, perhaps, was no fitter than other, to a convenient ſhape, though, till it was altered by the fire, it diſcovered no magnetical quality; yet, after it had been kept red-hot in the fire, and was ſuffered to cool in a convenient poſture, it was enabled to exerciſe magnetical operations upon a poiſed needle.

EXPERIMENT XIII.

AS for the abolition of the magnetical virtue in a body endowed with it, it may be made without deſtroying the ſubſtantial, or the eſſential form of the body, and without ſenſibly adding, diminiſhing, or altering any thing, in reference to the ſalt, ſulphur, and mercury, which chemiſts preſume iron and ſteel, as well as other mixed bodies, to be compoſed of. For it has been ſometimes obſerved, that the bare continuance of a loadſtone itſelf, in a contrary poſition to that, which, when freely placed, it ſeems to effect, has either corrupted, or ſenſibly leſſened the virtue of it. What I formerly obſerved to this purpoſe, I elſewhere relate, and ſince that, having a loadſtone, whoſe vigour was looked upon, by ſkilful perſons, as very extraordinary, and which, whilſt it was in an artificer's hand, was therefore held at a high rate, I was careful, being by ſome occaſions called out of *London*, to lock it up, with ſome other rarities, in a cabinet, whereof I took the key along with me, and ſtili kept it in my own pocket. But my ſtay abroad proving much longer than I expected, when, being returned to *London*, I had occaſion to make uſe of this loadſtone for an experiment, I found it indeed where I left it, but ſo exceedingly decayed, as to its attractive power, which I had formerly examined by weight, by having lain almoſt a year in an inconvenient poſture, that if it had not been for the circumſtances newly related, I ſhould have concluded, that ſome body had purpoſely got it out in my abſence, and ſpoiled it by help of the fire, the virtue being ſo much impaired, that I cared little to employ it any more about conſiderable experiments.

EXPERIMENT XIV.

AND this corruption of the magnetical virtue, which may, in tract of time, be made in a loadſtone itſelf, may, in a trice, be made by the help of that ſtone in an excited needle. For, it is obſerved by magnetical writers, and my own trials, purpoſely made, have aſſured me of it, that a well-poiſed needle, being, by the touch of a good loadſtone, excited and brought to turn one of its ends to the north, and the other to the ſouth, it may, by a contrary touch of the ſame loadſtone, be deprived of the faculty it had of directing its determinate extremes to determinate poles. Nay, by another touch (or the ſame, and even without immediate contact, if the magnet be vigorous enough) the needle may preſently have its direction ſo changed, that the end, which formerly pointed to the north pole, ſhall now regard the ſouth, and the other end ſhall, inſtead of the ſouthern, reſpect the northern pole.

EXPERIMENT XV.

AND to make it the more probable, that the change of the magnetiſm, communicated to iron, may be produced, at leaſt, in good part, by mechanical operations, procuring ſome change of texture in the iron; I ſhall ſubjoin a notable experiment of the ingenious Dr. *Power*, which, when I heard of, I tried as well as I could; and though, perhaps for want of conveniency, I could not make it fully anſwer what it promiſed, yet the ſucceſs of the trial was conſiderable enough to make it pertinent in this place, and to induce me to think, it might yet better ſucceed with him, whoſe experiment, as far as it concerns my preſent purpoſe, imports, that if a puncheon, as ſmiths call it, or a rod of iron, be, by being ignited and ſuffered to cool north and ſouth, and hammered at the ends, very manifeſtly endowed with magnetical virtue, this virtue will, in a trice be deſtroyed, by two or three ſmart blows of a ſtrong hammer, upon the middle of the oblong piece of iron.

BUT magnetiſm is ſo fertile a ſubject, that, if I had now the leiſure and conveniency to range among magnetical writers, I ſhould ſcarce doubt of finding, among their many experiments and obſervations, divers, that might be added to thoſe above delivered, as being eaſily applicable to my preſent argument. And I hope you will find farther probability added to what has been ſaid, to ſhew, “that magnetical operations may much depend upon mechanical principles,” by ſome phænomena recited in another paper, to which I once committed ſome promiſcuous experiments and obſervations magnetical.

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[*] Relating to the magnetiſm of the earth.

EXPERIMENTS AND NOTES,

ABOUT THE

Mechanical ORIGIN OF PRODUCTION

OF

ELECTRICITY.

THAT it is not neceſſary to believe electrical attraction (which, you know, is generally liſted among occult qualities) to be the effect of a naked and ſolitary quality, flowing immediately from a ſubſtantial form; but that it may rather be the effect of a material effluvium, iſſuing from, and returning to the electrical body (and perhaps in ſome caſes aſſiſted in its operation by the external air) ſeems agreeable to divers things, that may be obſerved in ſuch bodies and their manner of acting.

There are differing hypotheſes (and all of them mechanical, propoſed by the moderns) to ſolve the phænomena of electrical attraction. Of theſe opinions the firſt is, that of the learned Jeſuit *Cabæus*, who, though a Peripatetick and commentator on *Ariſtotle*, thinks the drawing of light bodies by jet, amber, &c. may be accounted for, by ſuppoſing, that the ſteams, that iſſue, or, if I may ſo ſpeak, ſally out of amber, when heated by rubbing, diſcuſs and expel the neighbouring air; which after it has been driven off a little way, makes as it were a ſmall whirlwind, becauſe of the reſiſtance it finds from the remoter air, which has not been wrought on by the electrical ſteams; and that theſe, ſhrinking back ſwiftly enough to the amber, do in their returns bring along with them ſuch light bodies, as they meet with in their way. On occaſion of which hypotheſis I ſhall offer it to be conſidered, whether by the gravity of the atmoſpherical air, ſurmounting the ſpecifick gravity of the little and rarified atmoſphere, made about the amber by its emiſſions, and compriſing the light body faſtened on by them, the attraction may not in divers caſes be either cauſed or promoted.

Another hypotheſis is that propoſed by that ingenious gentleman, Sir *Kenelm Digby*, and embraced by the very learned Dr. *Browne*, (who ſeems to make our *Gilbert* himſelf to have been of it) and divers other ſagacious men. And, according to this hypotheſis, the amber, or other electrick, being chafed or heated, is made to emit certain rays or files of unctuous ſteams, which, when they come to be a little cooled by the external air, are ſomewhat condenſed, and having loſt of their former agitation, ſhrink back to the body, whence they ſallied out, and carry with them thoſe light bodies, that their further ends happen to adhere to, at the time of their retraction: as when a drop of oil or ſyrup hangs from the end of a ſmall ſtick, if that be dextrouſly and cautiouſly ſtruck, the viſcous ſubſtance will, by that impulſe, be ſtretched out, and preſently retreating, will bring along with it the duſt or other light bodies, that chanced to ſtick to the remoter parts of it.

And this way of explaining electrical attractions is employed alſo by the learned *Gaſſendus*, who adds to it, that theſe electrical rays, if they may be ſo called, being emitted ſeveral ways, and conſequently croſſing one another, get into the pores of the ſtraw, or other light body to be attracted, and by means of their decuſſation, take the faſter hold of it, and have the greater force to carry it along with them, when they ſhrink back to the amber, whence they were emitted.

A third hypotheſis there is, which was deviſed by the acute *Carteſius*, who diſlikes the explications of others, chiefly becauſe he thinks them not applicable to glaſs, which he ſuppoſes unfit to ſend forth effluvia, and which is yet an electrical body; and therefore attempts to account for electrical attractions by the intervention of certain particles, ſhaped almoſt like ſmall pieces of ribband, which he ſuppoſes to be formed of this ſubtile matter harboured in the pores or creviſes of glaſs. But this hypotheſis, though ingenious in itſelf, yet depending upon the knowledge of divers of his peculiar principles, I cannot intelligibly propoſe it in few words, and therefore ſhall refer you to himſelf for an account of it: which I the leſs ſcruple to do, becauſe though it be not unworthy of the wonted acuteneſs of the author, yet he ſeems himſelf to doubt, whether it will reach all electrical bodies; and it ſeems to me, that the reaſon, why he rejects the way of explicating attraction by the emiſſion of the finer parts of the attrahent (to which hypotheſis, if it be rightly propoſed, I confeſs myſelf very inclinable) is grounded upon a miſtake, which, though a philoſopher may, for want of experience in that particular, without diſparagement fall into, is nevertheleſs a miſtake. For whereas our excellent author ſays, that electrical

Princip. part IV. *art.* 184.

cal effluvia, ſuch as are ſuppoſed to be emitted by amber, wax, &c. cannot be imagined to proceed from glaſs, I grant the ſuppoſition to be plauſible, but cannot allow it to be true. For as ſolid a body as glaſs is, yet if you but dexterouſly rub for two or three minutes a couple of pieces of glaſs againſt one another, you will find, that glaſs is not only capable of emitting effluvia, but ſuch ones, as to be odorous, and ſometimes to be rankly ſtinking.

But it is not neceſſary, that in this paper, where I pretend not to write diſcourſes but notes, I ſhould conſider all, that has been, or I think may be, ſaid for and againſt each of the abovementioned hypotheſes; ſince they all agree in what is ſufficient for my preſent purpoſe, namely, that electrical attractions are not the effects of a mere quality, but of a ſubſtantial emanation from the attracting body; and it is plain, that they all endeavour to ſolve the phænomena in a mechanical way, without recurring to ſubſtantial forms, and inexplicable qualities, or ſo much as taking notice of the hypoſtatical principles of the chemiſts. Wherefore it may ſuffice in this place, that I mention ſome phænomena, that in general make it probable, that amber, &c. draws ſuch light bodies, as pieces of ſtraw, hair, and the like, by virtue of ſome mechanical affections either of the attracting, or of the attracted bodies, or of both the one and the other.

1. The firſt and moſt general obſervation is, that electrical bodies draw not, unleſs they be warmed; which rule, though I have now and then found to admit of an exception, (whereof I elſewhere offer an account,) yet as to the generality of common electricks, it holds well enough to give much countenance to our doctrine, which teaches the effects of electrical bodies to be performed by corporeal emanations. For it is known, that heat, by agitating the parts of a fit body, ſolicites it as it were to ſend forth its effluvia, as is obvious in odoriferous gums and perfumes, which, being heated, ſend forth their fragrant ſteams, both further and more copiouſly, than otherwiſe they would.

2. Next, it has been obſerved, that amber, &c. warmed by the fire, does not attract ſo vigorouſly, as if it acquire an equal degree of heat by being chafed or rubbed: ſo that the modification of motion in the internal parts, and in the emanations of the amber, may, as well as the degree of it, contribute to the attraction. And my particular obſervations incline me to add, that the effect may oftentimes be much promoted, by employing both theſe ways ſucceſſively; as I thought I manifeſtly found, when I firſt warmed the amber at the fire, and preſently after chafed it a little upon a piece of cloth. For then a very few rubbings ſeemed to excite it more than many more would otherwiſe have done: as if the heat of the fire had put the parts into a general, but confuſed agitation; to which it was eaſy for the ſubſequent attrition (or reciprocation of preſſure) to give a convenient modification in a body, whoſe texture diſpoſes it to become vigorouſly electrical.

3. Another obſervation, that is made about theſe bodies, is, that they require terſion as well as attrition; and though I doubt whether the rule be infallible, yet I deny not, but that weaker electricks require to be as well wiped as chafed; and even good ones will have their operation promoted by the ſame means. And this is very agreeable to our doctrine, ſince terſion, beſides that it is, as I have ſometimes manifeſtly known it, a kind or degree of attrition, frees the ſurface from thoſe adherences, that might choak the pores of the amber, or at leaſt hinder the emanation of the ſteams to be ſo free and copious, as otherwiſe they would be.

4. It is likewiſe obſerved, that whereas the magnetical ſteams are ſo ſubtile, that they penetrate and perform their operation through all kind of mediums hitherto known to us; electrical ſteams are like thoſe of ſome odoriferous bodies, eaſily checked in their progreſs, ſince it is affirmed by learned writers, who ſay they ſpeak upon particular trial, that the interpoſition of the fineſt linnen, or ſarſnet, is ſufficient to hinder all the operation of excited amber upon a ſtraw or feather placed never ſo little beyond it.

5. It has been alſo obſerved, that the effects of electrical attraction are weakened, if the air be thick and cloudy; and eſpecially if the ſouth wind blows; and that electricks diſplay their virtue more faintly by night than by day, and more vigorouſly in clear weather, and when the winds are northerly. All which the learned *Kircherus* aſſerts himſelf to have found true by experience; inſomuch, that thoſe bodies, that are but faintly drawn, when the weather is clear, will not, when it is thick and cloudy, be at all moved.

6. We have alſo obſerved, that divers concretes, that are notably electrical, do abound in an effluviable matter, (if I may ſo call it) which is capable of being manifeſtly evaporated by heat and rubbing. Thus we ſee, that moſt reſinous gums, that draw light bodies, do alſo, being moderately ſollicited by heat, (whether this be excited by the fire, or by attrition or contuſion) emit ſteams. And in pieces of ſulphur conveniently ſhaped, I found, upon due attrition, a ſulphureous ſtink. And that piece of amber, which I moſt employ, being ſomewhat large, and very well poliſhed, will, being rubbed upon a piece of woollen cloth, emit ſteams, which the noſtrils themſelves may perceive; and they ſometimes ſeem to me not unlike thoſe, that I took notice of, when I kept in my mouth a drop or two of the diluted tincture (or ſolution of the finer parts) of amber made with ſpirit of wine, or of ſal armoniac.

7. It agrees very well with what has been ſaid of the corporeal emanations of amber, that its attractive power will continue ſome time after it has been once excited. For the attrition having cauſed an inteſtine commotion in the parts of the concrete, the heat or warmth, that is thereby excited, ought not to ceaſe, as ſoon as ever the rubbing is over, but to continue capable of emitting effluvia for ſome time afterwards, which will be longer or ſhorter, according

ing to the goodneſs of the electric, and the degree of the antecedent commotion: which, joined together, may ſometimes make the effect conſiderable, inſomuch that in a warm day, about noon, I did, with a certain body, not much, if at all, bigger than a pea, but very vigorouſly attractive, move to and fro a ſteel needle, freely poiſed, about three minutes (or the twentieth part of an hour) after I had left off rubbing the attrahent.

8. THAT it may not ſeem impoſſible, that electrical effluvia ſhould be able to inſinuate themſelves into the pores of many other bodies, I ſhall add, that I found them ſubtile enough to attract not only ſpirit of wine, but that fluid aggregate of corpuſcles we call ſmoke. For having well lighted a wax taper, which I preferred to a common candle, to avoid the ſtink of the ſnuff, I blew out the flame; and, when the ſmoke aſcended in a ſlender ſtream, held, at a convenient diſtance from it, an excited piece of amber, or a chafed diamond, which would manifeſtly make the aſcending ſmoke deviate from its former line, and turn aſide, to beat, as it were, againſt the electric, which, if it were vigorous, would act at a conſiderable diſtance, and ſeemed to ſmoke for a pretty while together.

9. THAT it is not in any peculiar ſympathy between an electric and a body, whereon it operates, that electrical attraction depends, ſeems the more probable, becauſe amber, for inſtance, does not attract only one determinate ſort of bodies, as the loadſtone does iron, and thoſe bodies, wherein it abounds; but, as far as I have yet tried, it draws indifferently all bodies whatſoever, being placed within a due diſtance from it, (as my choiceſt piece of amber draws not only ſand and mineral powders, but filings of ſteel and copper, and beaten gold itſelf) provided they be minute or light enough, except, perhaps, it be fire: I employ the word perhaps, becauſe I am not yet ſo clear in this point. For having applied a ſtrong electric, at a convenient diſtance, to ſmall fragments of ignited matter, they were readily enough attracted, and ſhined, whilſt they were ſticking to the body, that had drawn them. But, when I looked attentively upon them, I found the ſhining ſparks to be, as it were, cloathed with light aſhes, which, in ſpite of my diligence, had been already formed about the attracted corpuſcles, upon the expiring of a good part of the fire; ſo that it remained ſomewhat doubtful to me, whether the ignited corpuſcles, whilſt they were totally ſuch, were attracted; or whether the immediate objects of the attraction were not the new formed aſhes, which carried up with them thoſe yet unextinguiſhed parts of fire, that chanced to be lodged in them. But, as for flame, our countryman *Gilbert* delivers, as his experiment, that an electric, though duly excited and applied, will not move the flame of the ſlendereſt candle; which ſome will think not ſo eaſy to be well tried with common electricks, as amber, hard wax, ſulphur, and the like unctuous concretes, that very eaſily take fire: therefore I choſe to make my trial, with a rough diamond, extraordinarily attractive, which I could, without injuring it, hold, as near as I pleaſed, to the flame of a candle, or taper; and though I was not ſatisfied, that it did either attract the flame, as it viſibly did the ſmoke, or manifeſtly agitate it; yet, granting, that *Gilbert*'s aſſertion will conſtantly hold true, and ſo, that flame is to be excepted from the general rule, yet this exception may well comport with the hypotheſis hitherto countenanced, ſince it may be ſaid, as it is, if I miſtake not, by *Kircherus*, that the heat of the flame diſſipates the effluvia, by whoſe means the attraction ſhould be performed. To which I ſhall add, that poſſibly the celerity of the motion of the flame upwards may render it very difficult, for the electrical emanations to divert the flame from its courſe.

10. WE have found by experiment, that a vigorous and well excited piece of amber will draw, not only the powder of amber, but leſs minute fragments of it. And as, in many caſes, one contrary directs to another, ſo this trial ſuggeſted a further, which, in caſe of good ſucceſs, would probably argue, that, in electrical attraction, not only effluvia are emitted by the electrical body, but theſe effluvia faſten upon the body to be drawn, and that in ſuch a way, that the intervening viſcous ſtrings, which may be ſuppoſed to be made up of thoſe cohering effluvia, are, when their agitation ceaſes, contracted or made to ſhrink inwards, towards both ends, almoſt as a highly ſtretched lute-ſtring does, when it is permitted to retreat into ſhorter Dimenſions. But the conjecture itſelf was much more eaſy to be made, than the experiment requiſite to examine it. For we found it no eaſy matter to ſuſpend an electric, great and vigorous enough, in ſuch a manner, that it might, whilſt ſuſpended, be excited, and be ſo nicely poiſed, that ſo faint a force as that, wherewith it attracts light bodies, ſhould be able to procure a local motion to the whole body itſelf. But after ſome fruitleſs attempts with other electricks, I had recourſe to the very vigorous piece of poliſhed amber, formerly mentioned; and when we had, with the help of a little wax, ſuſpended it by a ſilken thread, we chafed very well one of the blunt edges of it upon a kind of large pin-cuſhion, covered with a coarſe and black woollen ſtuff, and then brought the electric, as ſoon as we could, to ſettle, notwithſtanding its hanging freely at the bottom of the ſtring. This courſe of rubbing on the edge of the amber we pitched upon for more than one reaſon; for if we had chafed the flat ſide, the amber could not have approached the body it had been rubbed on, without making a change of place in the whole electric; and, which is worſe, without making it move (contrary to the nature of heavy bodies) ſomewhat upwards; whereas the amber having, by reaſon of its ſuſpenſion, its parts counterpoiſed by one another, to make the excited edge approach to another body, that edge needed not all aſcend, but only be moved horizontally, to which way of moving the gravity of the electric (which the ſtring kept from moving downwards) could be but little or no hinderance. And, agreeably to this, we found, that if, as ſoon as the ſuſpended and well rubbed electric was brought to ſettle freely, we applied to the chafed edge, but without touching

ing it, the lately mentioned cuſhion, which, by reaſon of its rough ſuperficies and poroſity, was fit for the electrical effluvia to faſten upon, the edge would manifeſtly be drawn aſide by the cuſhion ſteadily held, and if this were ſlowly removed, would follow it a good way; and when this body no longer detained it, would return to the poſture, wherein it had ſettled before. And this power of approaching the cuſhion, by virtue of the operation of its own ſteams, was ſo durable in our vigorous piece of amber, that, by once chafing it, I was able to make it follow the cuſhion no leſs than ten or eleven times. Whether from ſuch experiments one may argue, that it is but, as it were, by accident, that amber attracts another body, and not this the amber; and whether theſe ought to make us queſtion, if electricks may, with ſo much propriety, as has been hitherto generally ſuppoſed, be ſaid to attract, are doubts, that my deſign does not here oblige me to examine.

SOME other phænomena might be added of the ſame tendency with thoſe already mentioned, (as the advantage, that electrical bodies uſually get by having well poliſhed, or, at leaſt, ſmooth ſurfaces;) but the title of this paper promiſing ſome experiments about the production of electricity, I muſt not omit to recite, how I have been ſometimes able to produce or deſtroy this quality in certain bodies, by means of alterations, that appeared not to be other than mechanical.

EXPERIMENT I.

AND firſt, having, with a very mild heat, ſlowly evaporated about a fourth part of good turpentine, I found, that the remaining body would not, when cold, continue a liquor, but hardened into a tranſparent gum almoſt like amber, which, as I looked for, proved electrical.

EXPERIMENT II.

SECONDLY, by mixing two ſuch liquid bodies, as petroleum and ſtrong ſpirit of nitre, in a certain proportion, and then diſtilling them till there remained a dry maſs, I obtained a brittle ſubſtance as black as jet; and whoſe ſuperficies (where it was contiguous to the retort) was gloſſy, like that mineral, when poliſhed; and, as I expected, I found it alſo to reſemble jet, in being endowed with an electrical faculty.

EXPERIMENT III.

THIRDLY, having burnt antimony to aſhes, and of thoſe aſhes, without any addition, made a tranſparent glaſs, I found, that, when rubbed, as electrical bodies ought to be to excite them, it anſwered my expectation, by manifeſting a not inconſiderable electricity. And this is the worthier of notice, becauſe, that as a vitrum antimonii, that is ſaid to be purer than ordinary, may be made of the regulus of the ſame mineral, in whoſe preparation you know a great part of the antimonial ſulphur is ſeparated, and left among the ſcoriæ; ſo glaſs of antimony, made without additament, may eaſily, as experience has informed us, be in part reduced to a regulus, (a body not reckoned amongſt electrical ones.) And that you may not think, that it is only ſome peculiar and fixed part of the antimony, that is capable of vitrification, let me aſſure you, that, even with the other part, that is wont to fly away, (namely, the flowers) an antimonial glaſs may, without an addition of other ingredients, be made.

EXPERIMENT IV.

FOURTHLY, the mention of a vitrified body brings into my mind, that I more than once made ſome glaſs of lead *per ſe*, (which I found no very eaſy work) that alſo was not wholly deſtitute of an electrical virtue, though it had but a very languid one. And it is not here to be overlooked, that this glaſs might eaſily be brought to afford again malleable lead, which was never reckoned, that I know of, among electrical bodies.

EXPERIMENT V.

FIFTHLY, having taken ſome amber, and warily diſtilled it, not with ſand or powdered brick, or ſome ſuch additament, as chemiſts are wont to uſe, for fear it ſhould boil over, or break their veſſels; but by itſelf, that I might have an unmixed caput mortuum: having made this diſtillation, I ſay, and continued it, till it had afforded a good proportion of phlegm, ſpirit, volatile ſalt, and oil, the retort was warily broken, and the remaining matter was taken out in a lump, which, though it had quite loſt its colour, being burned quite black, and though it were grown ſtrangely brittle, in compariſon of amber, ſo that they, who believe the virtue of attracting light bodies to flow from the ſubſtantial form of amber, would not expect it in a body ſo changed and deprived of its nobleſt parts; yet this caput mortuum was ſo far from having loſt its electrical faculty, that it ſeemed to attract more vigorouſly than amber itſelf is wont to do, before it be committed to diſtillation.

AND from the foregoing inſtances afforded us by the glaſs of antimony, we may learn, that when the form of a body ſeems to be deſtroyed by a fiery analyſis, that diſſipates the parts of it, the remaining ſubſtance may yet be endowed with electricity, as the *caput mortuum* of amber may acquire it; as in the caſe of the glaſs of antimony made of the calx and of the flowers. And from the ſecond example abovementioned, and from common glaſs, which is electrical, we may alſo learn, that bodies, that are neither of them apart obſerved to be endowed with electricity, may have that virtue reſult in the compounded ſubſtance, that they conſtitute, though it be but a factitious body.

To the foregoing experiments, whoſe ſucceſs is wont to be uniform enough, I ſhall add the recital of a ſurpriſing phænomenon, which, though not conſtant, may help to make it probable, that electrical attractions need not be ſuppoſed ſtill to proceed from the ſubſtantial, or even from the eſſential form of the attrahent, but may be the effects of unheeded, and, as it were, fortuitous cauſes. And, however, I dare not ſuppreſs ſo ſtrange an obſervation, and therefore ſhall relate that, which I had the luck to make of an odd ſort of electrical attraction (as it ſeemed,) not taken notice of (that I know of) by any, either naturaliſt or other writer, and it is this.

EXPERIMENT VI.

THAT falſe locks, as they call them, of ſome hair, being by curling or otherwiſe brought to a certain degree of dryneſs, or of ſtiffneſs, will be attracted by the fleſh of ſome perſons, or ſeem to apply themſelves to it, as hair is wont to do to amber or jet excited by rubbing. Of this I had a proof in ſuch locks worn by two very fair ladies, that you know. For at ſome times I obſerved, that they could not keep their locks from flying to their cheeks, and (though neither of them made any uſe, or had any need of painting) from ſticking there. When one of theſe beauties firſt ſhewed me this experiment, I turned it into a complemental raillery, as ſuſpecting there might be ſome trick in it, though I after ſaw the ſame thing happen to the others locks too. But as ſhe is no ordinary virtuoſa, ſhe very ingeniouſly removed my ſuſpicions, and, as I requeſted, gave me leave to ſatisfy my ſelf further, by deſiring her to hold her warm hand at a convenient diſtance from one of thoſe locks taken off and held in the air. For as ſoon as ſhe did this, the lower end of the lock, which was free, applied itſelf preſently to her hand: which ſeemed the more ſtrange, becauſe ſo great a multitude of hair would not have been eaſily attracted by an ordinary electrical body, that had not been conſiderably large, or extraordinarily vigorous. This repeated obſervation put me upon enquiring among ſome other young ladies, whether they had obſerved any ſuch like thing; but I found little ſatisfaction to my queſtion, except from one of them eminent for being ingenious, who told me, that ſometimes ſhe had met with theſe troubleſome locks; but that all ſhe could tell me of the circumſtances, which I would have been informed about, was, that they ſeemed to her to fly moſt to her cheeks, when they had been put into a ſomewhat ſtiff curl, and when the weather was froſty[*].

You will probably be the leſs diſpoſed to believe, that electrical attractions muſt proceed from the ſubſtantial forms of the attrahents, or from the predominancy of this or that chemical principle in them, if I acquaint you with ſome odd trials, wherein the attraction of light bodies ſeemed to depend upon very ſmall circumſtances. And though forbearing at preſent, to offer you my thoughts about the cauſe of theſe ſurpriſing phænomena, I propoſe it only as a problem to your ſelf, and your curious friends, yet the main circumſtances ſeeming to be of a mechanical nature, the recital of my trials will not be impertinent to the deſign and ſubject of this paper.

EXPERIMENT VII.

I TOOK then a large and vigorous piece of amber, conveniently ſhaped for my purpoſe, and a downy feather, ſuch as grows upon the bodies, not wings or tails of a ſomewhat large chicken: then having moderately excited the electrick, I held the amber ſo near it, that the neighbouring part of the feather was drawn by it, and ſtuck faſt to it; but the remoter parts continued in their former poſture. This done, I applied my fore-finger to theſe erected downy feathers, and immediately, as I expected, they left their preceding poſture, and applied themſelves to it, as if it had been an electrical body. And whether I offered to them my nail, or the pulpy part of my finger, or held my finger towards the right hand or the left, or directly over, theſe downy feathers, that were near the little quill, did nimbly, and, for aught appeared, equally turn themſelves towards it. And to ſhew, that the ſteams, that iſſued out of ſo warm a body as my finger, were not neceſſary to attract, as men ſpeak, the above-mentioned feathers, inſtead of my finger, I applied to them, after the ſame manner, a little cylindrical inſtrument of ſilver, to which they bowed and faſtened themſelves, as they had done to my finger, though the tip of this inſtrument were preſented to them in ſeveral poſtures. The like ſucceſs I had with the end of an iron key, and the like alſo with a cold piece of poliſhed black marble; and ſometimes the feathers did ſo readily and ſtrongly faſten themſelves to theſe extraneous and unexcited bodies, that I have been able, though not eaſily, to make one of them draw the feather from the amber itſelf.

But it is diligently to be obſerved, that this unuſal attraction happened only, whilſt the electrical operation of the excited amber continued ſtrong enough to ſuſtain the feathers. For afterwards, neither the approach of my finger, nor that of the other bodies, would make the downy feathers change their poſture. Yet, as ſoon as ever the amber was by light affriction excited again, the feather would be diſpoſed to apply itſelf again to the abovementioned bodies.

And leſt there ſhould be any peculiarity in that particular feather, I made the trials, with others, (provided they were not long enough to exceed the ſphere of activity of the amber) and found the experiment to anſwer my expectation.

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[*] Some years after the making the experiments about the production of electricity, having a deſire to try, whether in the attractions made by amber, the motions excited by the air had a conſiderable intereſt, or whether the effect were not due rather to the emiſſion and retraction of effluvia, which being of a viſcous nature may conſiſt of particles either branched or hooked, or otherwiſe fit for ſome kind of coheſion, and capable of being ſtretched, and of ſhrinking again, as leather thongs are: to examine this, I ſay, I thought the fitteſt way, if it were practicable, would be, to try, whether amber would draw a light body in a glaſs whence the air was pumped out. And though the trial of this ſeemed very difficult to make, and we were ſomewhat diſcouraged by our firſt attempt, wherein the weight of the ambient air broke our receiver, which chanced to prove too weak, when the internal air had been with extraordinary diligence pumped out; yet having a vigorous piece of amber, which I had cauſed to be purpoſely turned and poliſhed for electrical experiments, I afterwards repeated the trial, and found, that in warm weather, it would retain a manifeſt power of attracting for ſeveral minutes (for it ſtirred a poiſed needle after above $\frac{1}{4}$ of an hour) after we had done rubbing it. Upon which encouragement we ſuſpended it, being firſt well chafed, in a glaſs receiver, that was not great, juſt over a light body; and making haſte with our air-pump to exhauſt the glaſs, when the air was withdrawn, we did by a contrivance let down the ſuſpended amber, till it came very near the ſtraw or feather, and perceived, as we expected, that in ſome trials, upon the leaſt contact it would lift it up; and in others, (for we repeated the experiment,) the amber would raiſe it without touching it, that is, would attract it.

I made the experiment alſo at differing times, and with ſome months, if not rather years, of interval, but with the like ſucceſs.

AND leſt you ſhould think theſe phænomena proceed from ſome peculiarity in the piece of amber I employed, I ſhall add, that I found uniformity enough in the ſucceſs, when, in the place of amber, I ſubſtituted another electrick, and particularly a ſmooth maſs of melted brimſtone.

THESE are the phænomena I thought fit to mention, at preſent, of this unuſual way of drawing light bodies, and with this experiment I ſhould conclude my notes about electricity, but that, I think, it will not be amiſs, before I take leave of this ſubject, to give this advertiſement, that the event of electrical experiments is not always ſo certain as that of many others, being ſometimes much varied by ſeemingly ſlight circumſtances, and now and then by ſome, that are altogether over-looked. This obſervation may receive credit from ſome of the particulars above recited (eſpecially concerning the intereſt of the weather, &c. in electrical phænomena.) But now I ſhall add, that, not only there may happen ſome variations in the ſucceſs of trials made with electrical bodies, but that it is not ſo certain as many think, whether ſome particular bodies be or be not electrical. For the inquiſitive *Kircherus* reckons cryſtal among thoſe gems, to whom nature has denied the attractive power we are ſpeaking of; and yet I remember not, that among all the trials I have made with natural cryſtal, I have found any, that was deſtitute of the power he refuſes them. Alſo a late moſt learned writer reciting the electricks, reckoned up by our induſtrious countryman *Gilbert*, and increaſing their number by ſome obſerved by himſelf, (to which I ſhall now add, beſides white ſapphires, and white Engliſh amethyſts, the almoſt diaphanous ſpar of lead ore) denies electricity to a couple of tranſparent gems, the cornelian and the emerald. And I do the leſs wonder he ſhould do ſo to the former, becauſe I have myſelf, in vain, tried to make any attraction with a piece of cornelian ſo large and fair, that it was kept for a rarity; and yet with divers other fine cornelians I have been able to attract ſome light bodies very manifeſtly, if not briſkly; and I uſually wear a cornelian ring, that is richly enough endowed with electricity. But as for emeralds, as I thought it ſtrange, that nature ſhould have denied them a quality ſhe has granted to ſo many other diaphanous gems, and even to cryſtal, ſo I thought the aſſertion deſerved an examen, upon which I concluded, that, at leaſt, it does not univerſally and conſtantly hold true. I had, indeed, ſeen in a ring a ſtone of price and great luſtre, which, though green, I found to be, (as I gueſſed it would prove) vigorouſly enough electrical. But this experiment, though ſeemingly concluſive, I did not look upon as a fair trial, becauſe the ſtone was not a true emerald, but, which is rare, a green ſapphire. And I learned by enquiry of the ſkilful jeweller, that cut it, that it was ſo far from having the ſoftneſs of an emerald, that he found it harder than blue ſapphires themſelves, which yet are gems of great hardneſs, and by ſome reputed ſecond to none, but diamonds. Without therefore concluding any thing from this experiment, ſave that, if the aſſertion I was to examine were true, the want of an electrical faculty might be thought a concomitant rather of the peculiar texture of the emerald than of its green colour, I proceeded to make trial with three or four emeralds, whoſe being true was not doubted, and found them all ſomewhat, though not equally, endowed with electricity, which I found to be yet more conſiderable in an emerald of my own, whoſe colour was ſo excellent, that by ſkilful perſons it was looked on as a rarity. And though, by this ſucceſs of my enquiry, I perceived I could not, as elſe I have done, ſhew the curious a new way of judging of true and falſe emeralds, yet the like may be, though not always certain, yet oftentimes of uſe, in the eſtimating whether diamonds be true or counterfeit, eſpecially if, being ſet in rings, the ſureſt way of trying them cannot conveniently be employed. For whereas glaſs, though it have ſome electricity, ſeems, as far as I have obſerved, to have but a faint one, there are often found diamonds that have a very vigorous one. And I do not remember I met with any electrick of the ſame bulk, that was more vigorous than a rough diamond I have, which is the ſame, that I formerly mentioned to have moved a needle above three minutes after I had ceaſed to chafe it. And this brings into my mind, that it has been obſerved, that diamonds draw better whilſt rough, than they do after they are cut and poliſhed; which ſeeming to contradict what has been obſerved by others, and by us alſo, that amber, for inſtance, attracts more vigorouſly, if the ſurface be made very ſmooth than otherwiſe, it induces me to conjecture, that, if this obſervation about diamonds be true, as ſome of my trials have now and then inclined me to think it, and if it do not in ſome caſes conſiderably depend upon the loſs of the (electrical) ſubſtance of the ſtone, by its being cut and ground, the reaſon may poſſibly be, that the great rapidneſs, with which the wheels, that ſerve to cut and poliſh diamonds muſt be moved, does excite a great degree of heat, (which the ſenſes may eaſily diſcover) in the ſtone, and by that and the ſtrong concuſſion it makes of its parts, may force it to ſpend its effluviable matter, if I may ſo call it, ſo plentifully, that the ſtone may be impoveriſhed, and perhaps alſo, on the account of ſome little change in its texture, be rendered leſs diſpoſed to emit thoſe effluvia, that are inſtruments of electrical attraction. But as I willingly leave the matter of fact to further trial, ſo I do the cauſe of it, in caſe it prove true, to further enquiry.

The END *of the* THIRD VOLUME.

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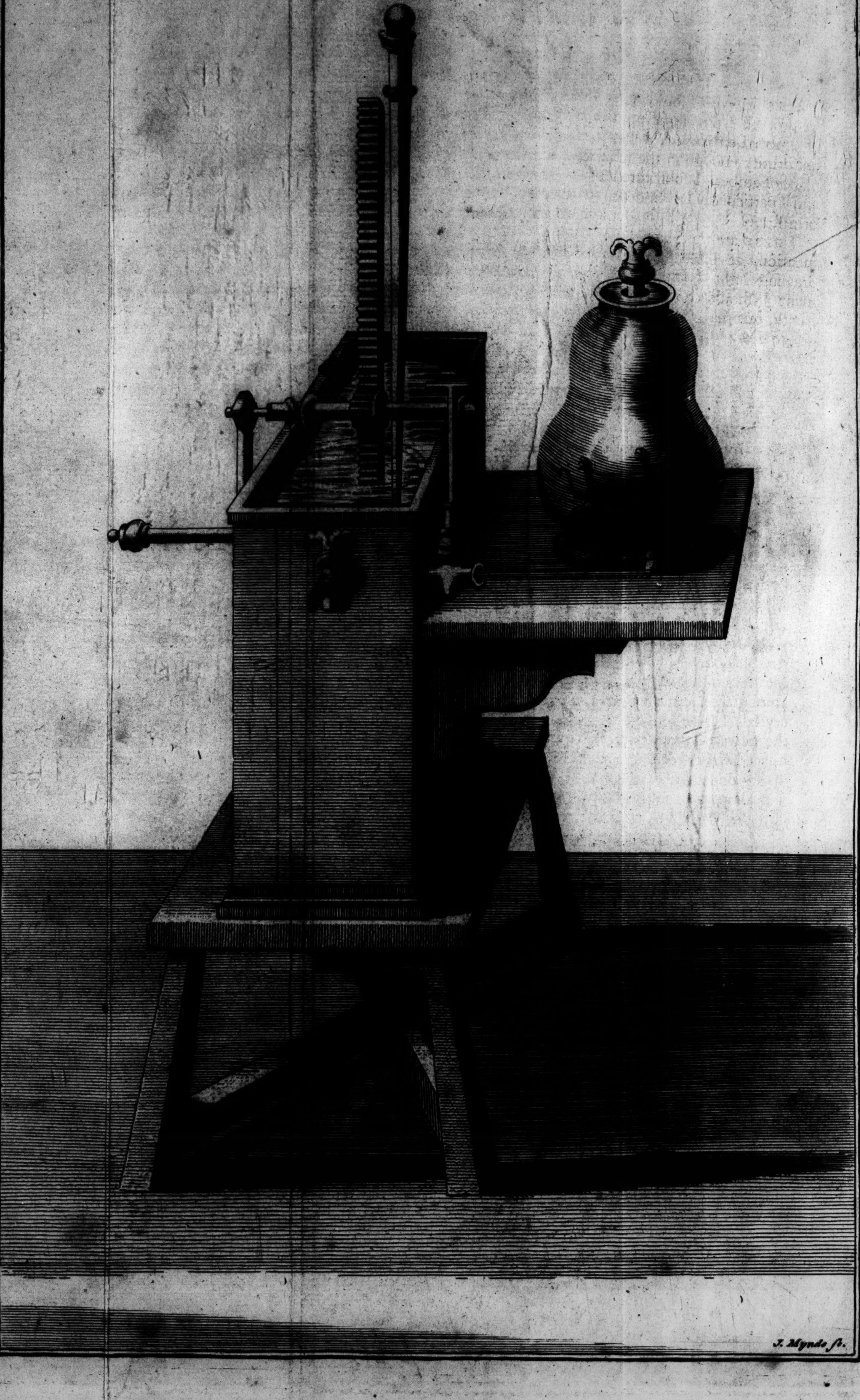
VOL. III. *Pl.* 1. *p.* 4.



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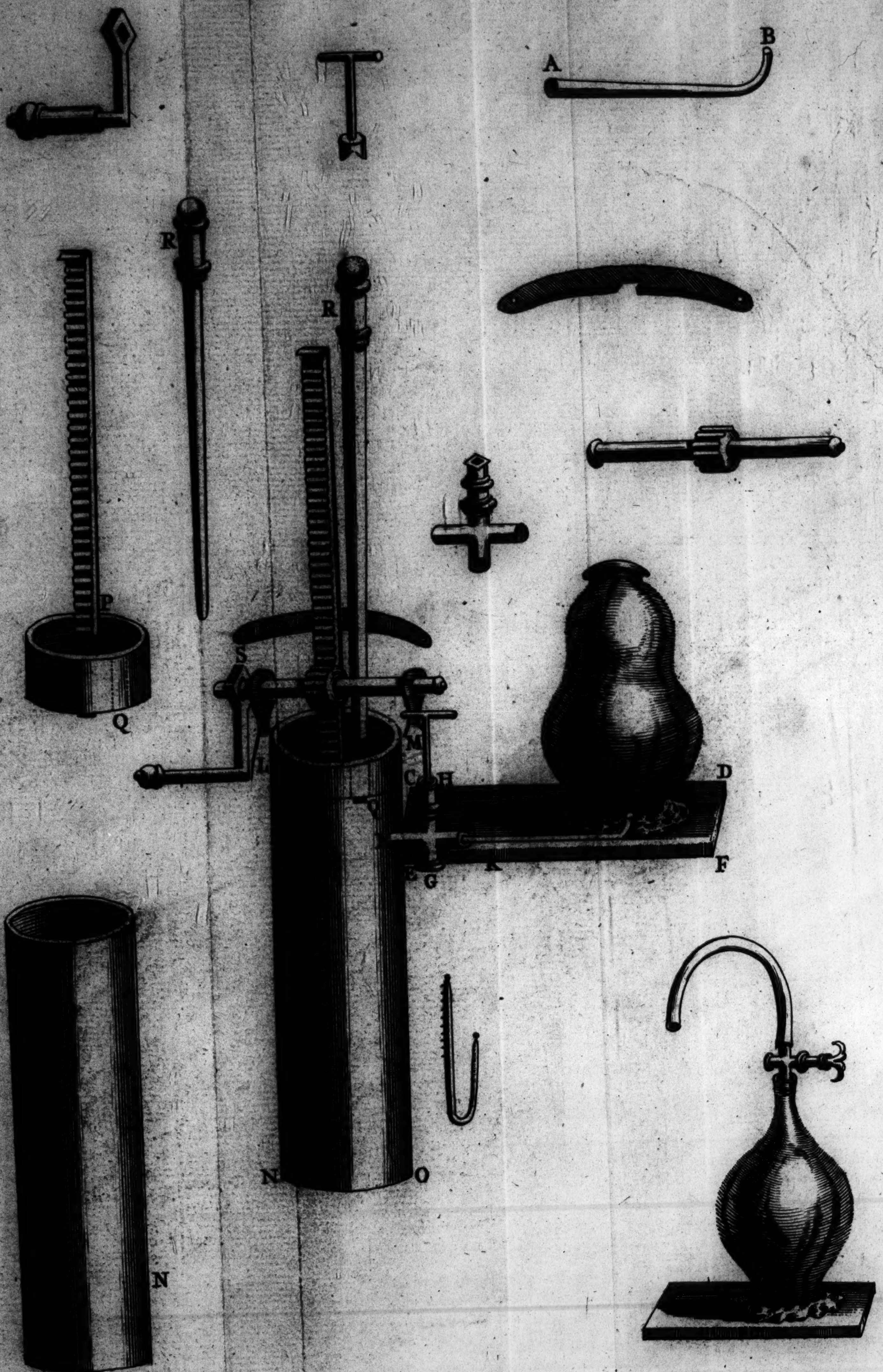
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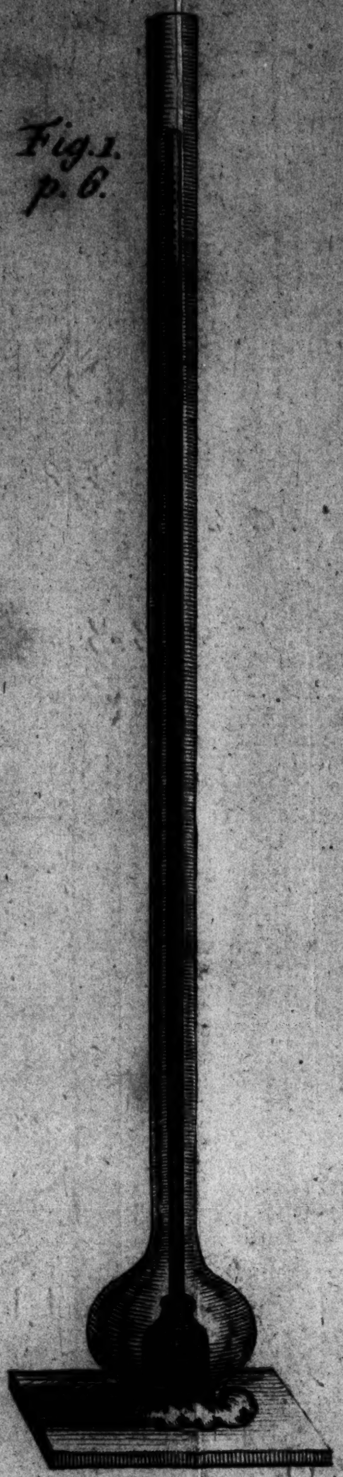
Fig. 1. p. 6.



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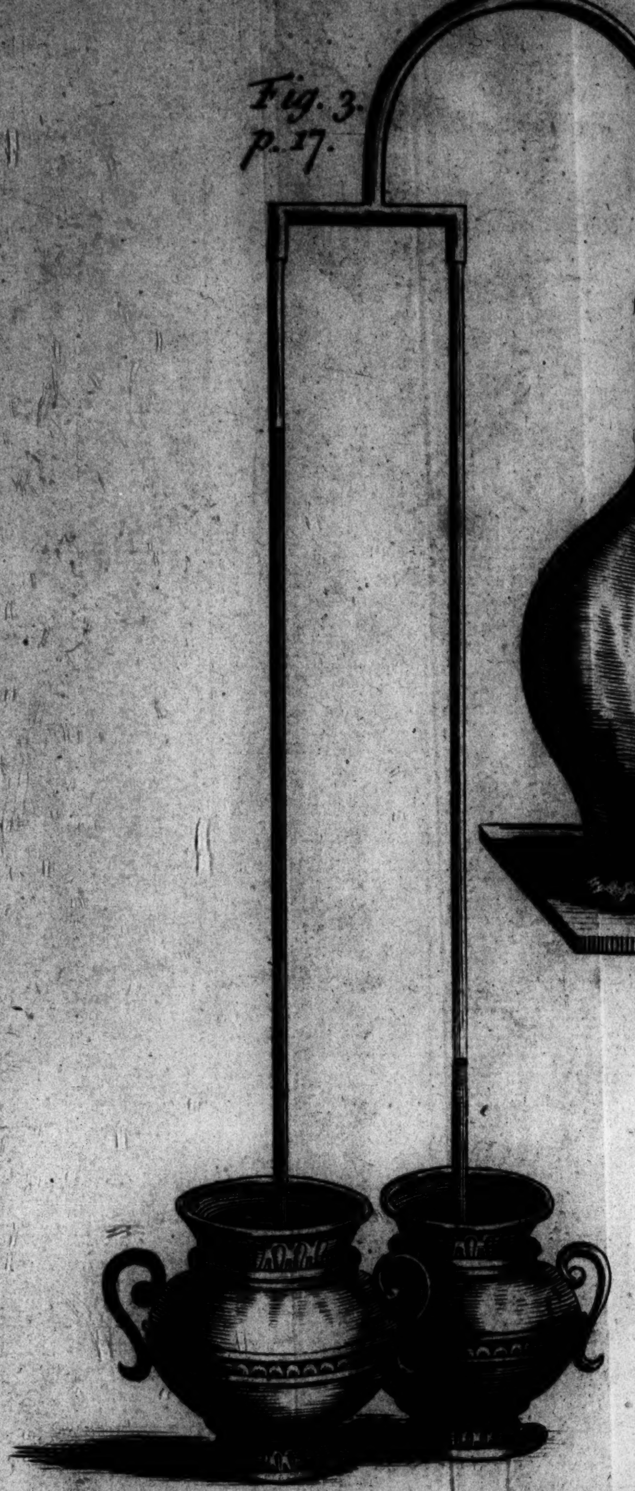

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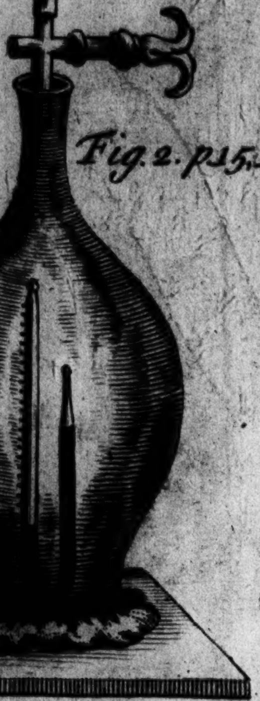

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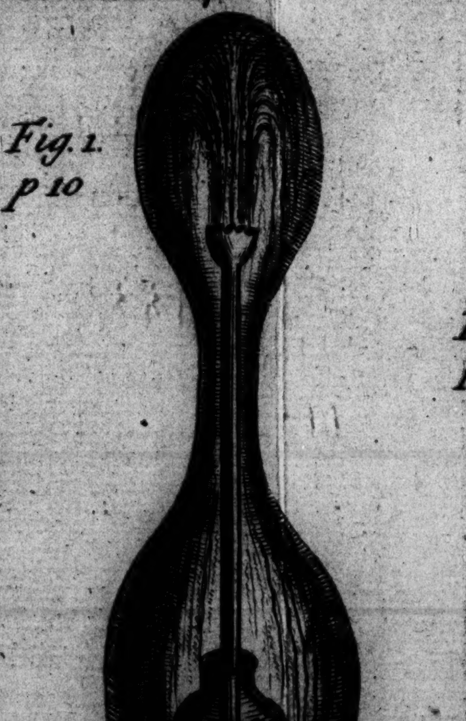

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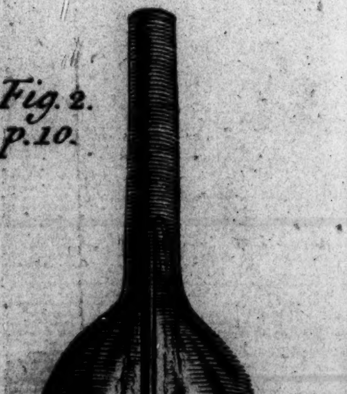

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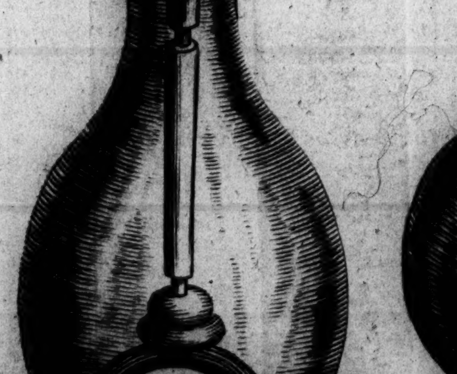

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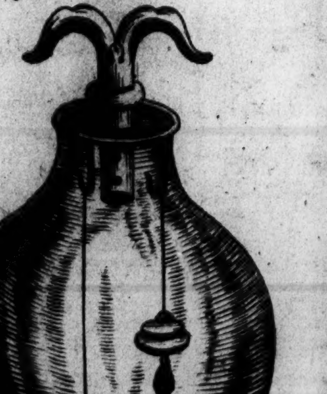

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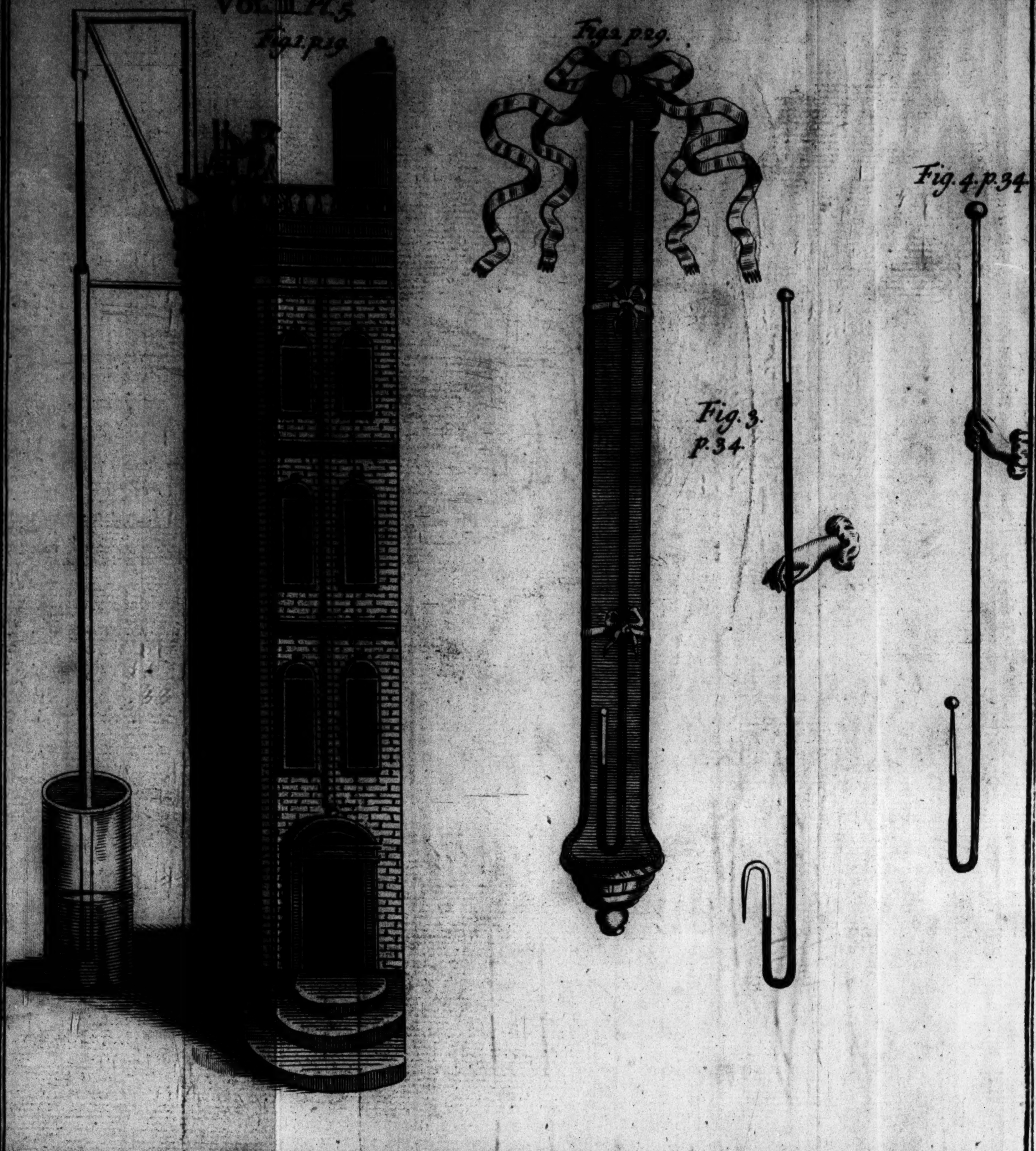
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Fig.2. p.29.
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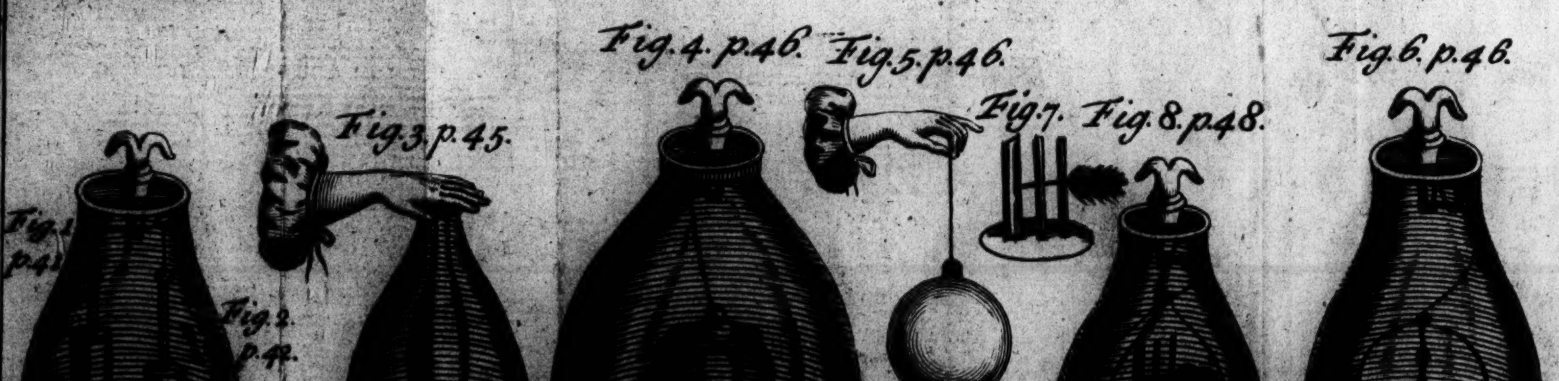
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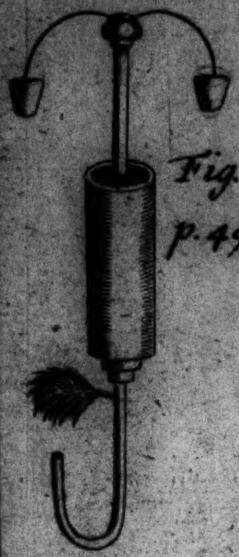

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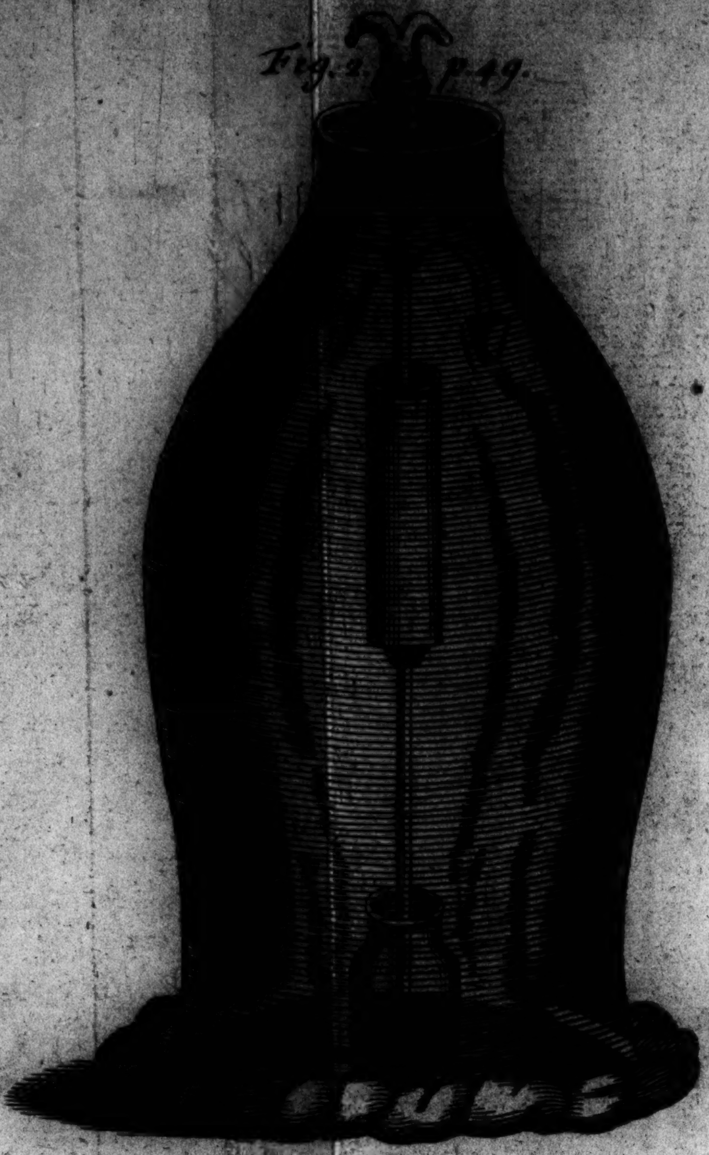

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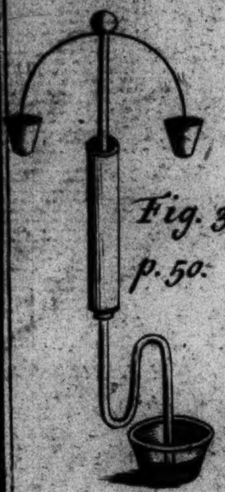

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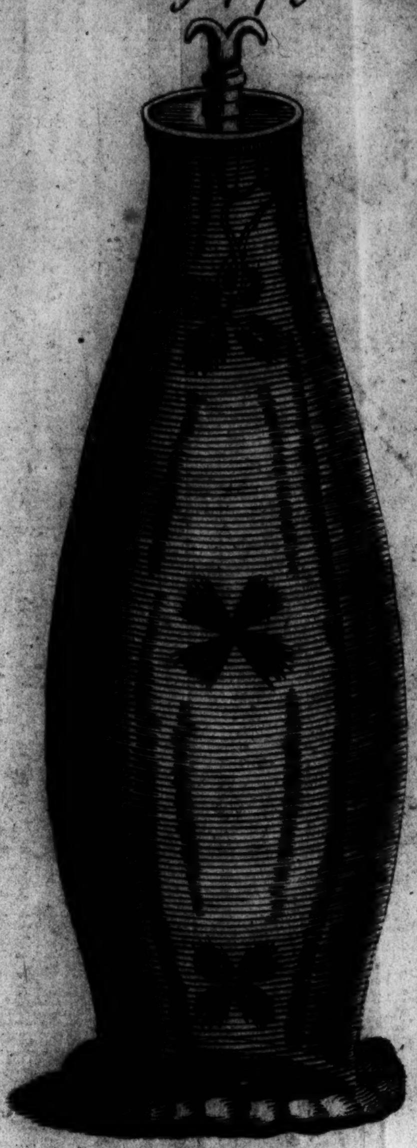

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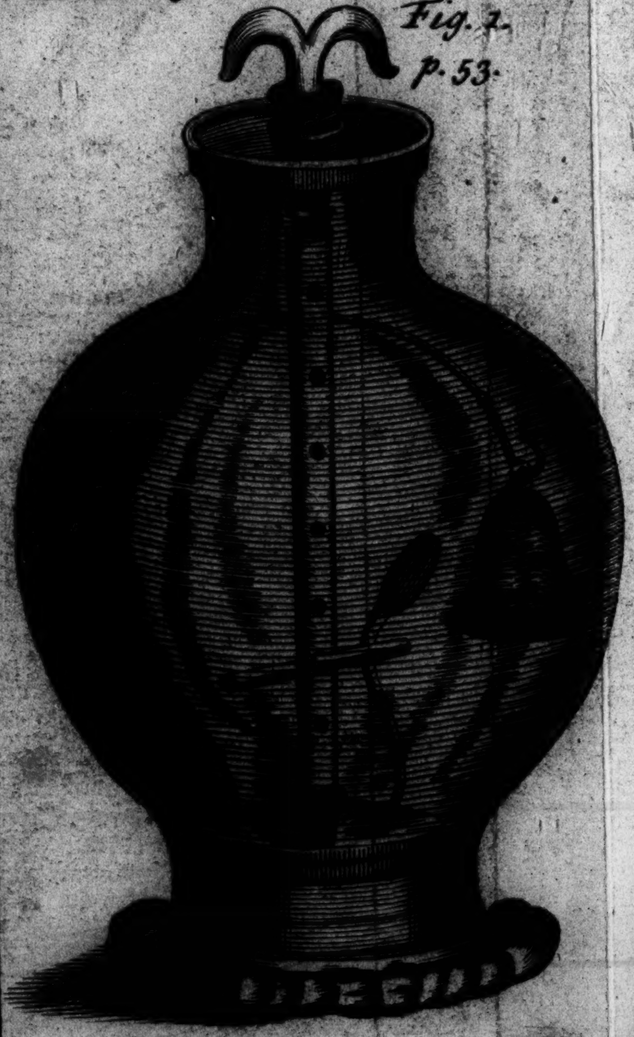

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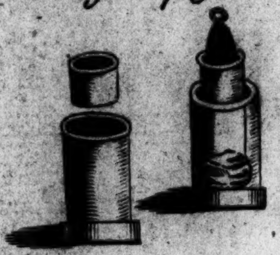

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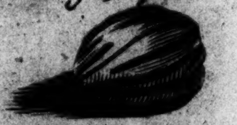

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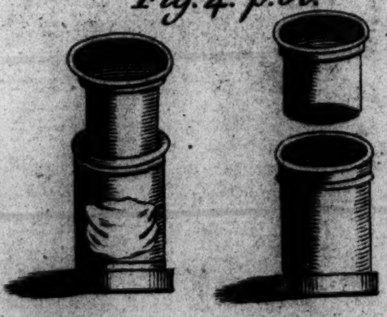

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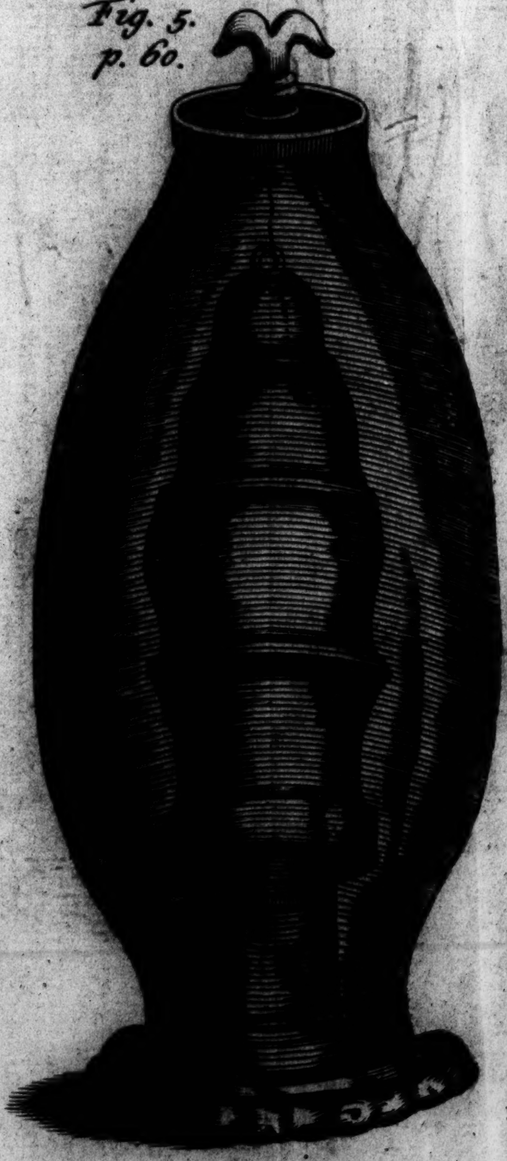

Fig. 5. p. 60.

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